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PAPERS AND PROCEEDINGS

OF THE

Seventy-eighth Annual Meeting

OF THE

AMERICAN ECONOMIC ASSOCIATION

New York, New York, December 28-30, 1965

Edited by the Secretary of the Association

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Organized at Saratoga, New York, September 9, 1885

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PAPERS AND PROCEEDINGS

OF THE

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OF THE

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New York, New York, December 28-30, 1965

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and

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New York, New York, December 28-30, 1965

As President-elect, charged with the preparation of the program for the Annual Meeting, I had the ambition to arrange a set of sessions displaying thematic unity without exception and without undue strain. The chosen theme, "Knowledge Production and Innovation," was developed in the Ely Lecture as well as in the fifteen sessions whose papers are published in this volume. It was further developed in six additional sessions which were sponsored jointly with allied organizations:

Agricultural Economics: Innovations in Agriculture (sponsored jointly with the American Farm Economic Association)

Business Finance: Innovations in Analysis (sponsored jointly with the American Finance Association)

Industrial Relations: Adjustments to Technological Change (sponsored jointly with the Industrial Relations Research Association)

Econometric Models for Educational Planning (sponsored jointly with the Econometric Society)

Innovational Impacts on American Capitalism (sponsored jointly with the Association for Comparative Economics)

Round Table on Study Problems in the Economics of Research and Development (arranged by the Office of Economic and Manpower Studies of the National Science Foundation)

The papers presented in these six sessions will probably be published elsewhere.

In the preparation of the program I was aided by several friends who either organized complete sessions or offered advice on particular papers or speakers. I wish to express my indebtedness to all of them, especially to George Leland Bach, Lester V. Chandler, Milton Friedman, Alexander Gerschenkron, Charles J. Hitch, Harry G. Johnson, Peter B. Kenen, Simon Kuznets, Edwin Mansfield, Edward S. Mason, James R. Nelson, Theodore W. Schultz, and Robert M. Solow.—FRITZ MACHLUP.

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12:30 P.M.

Executive Committee Luncheon

Tuesday, December 28, 1965
8:30 A.M.

Agricultural Economics: Innovations in Agriculture (Joint session with the American Farm Economic Association)

Chairman: R. J. HILDRETH, Farm Foundation

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Discussion: GLEN T. BARTON, U.S. Department of Agriculture; BURL BACK, U.S. Department of Agriculture

Business Finance: Innovations in Analysis (Joint session with the American Finance Association)

Chairman: MYRON J. GORDON, University of Rochester

Papers: WILLIAM W. ALBERTS, University of Chicago; EUGENE M. LERNER and WILLARD T. CARLETON, New York University; ALEXANDER A. ROBICHEK and STEWART C. MYERS, Stanford University; HENRY A. LATANÉ, University of North Carolina

Discussion: JOHN BOSSONS, Carnegie Institute of Technology; WILLIAM F. SHARPE, University of Washington; JACOB MICHAELSON, University of California, Berkeley; WILLIAM BERANEK, University of Wisconsin

10:30 A.M.

Allocation and Distribution Theory: Technological Innovation and Progress

Chairman: SIDNEY WEINTRAUB, University of Pennsylvania

Papers: KELVIN J. LANCASTER, Johns Hopkins University; WILLIAM J. FELLNER, Yale University; BERNARD A. CORRY, London School of Economics

Discussion: HANS J. BREMS, University of Illinois; JOHN S. CHIPMAN, University of Minnesota; TIBOR SCITOVSKY, University of California, Berkeley

Public Regulation: The Impact of Changing Technology
Chairman: JAMES R. NELSON, Amherst College
Papers: FRANKLIN M. FISHER, Massachusetts Institute of Technology; WILLIAM R. HUGHES, Wesleyan University; HARVEY J. LEVIN, Hofstra University
Discussion: BEN W. LEWIS, Oberlin College; GEORGE WILSON, Indiana University; WILLIAM IULO, Washington State University

2:30 P.M.

Money and Banking: Innovations in Finance
Chairman: LESTER V. CHANDLER, Princeton University
Papers: GEORGE W. MITCHELL, Board of Governors of the Federal Reserve System; A. JAMES MEIGS, First National City Bank; FRANCO MODIGLIANI, Massachusetts Institute of Technology
Discussion: WARREN L. SMITH, University of Michigan; ALBERT R. KOCH, Board of Governors of the Federal Reserve System; BURTON G. MALKIEL, Princeton University

The Economics of Broadcasting and Advertising
Chairman: SIDNEY S. ALEXANDER, Massachusetts Institute of Technology
Papers: R. H. COASE, University of Chicago; DAVID M. BLANK, Columbia Broadcasting System; LESTER G. TELSER, University of Chicago
Discussion: HAROLD J. BARNETT, Washington University; HYMAN H. GOLDIN, National Commission on Educational Television; PETER O. STEINER, University of Wisconsin

The Economics of Science Policy
Chairman: CHARLES J. HITCH, University of California, Berkeley
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Panel: EDWIN MANSFIELD, University of Pennsylvania; FREDERIC M. SCHERER, Princeton University; IRVING H. SIEGEL, Research Analysis Corporation; PAUL J. STURM, Department of Defense; GEORGE W. WRIGHT, National Aeronautics and Space Administration

8:00 P.M.

Richard T. Ely Lecture
Chairman: BERNARD F. HALEY, University of California, Santa Cruz
Invited Lecture: KENNETH E. BOULDING, University of Michigan

Wednesday, December 29, 1965

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Chairman: GARTH L. MANGUM, President's Automation Commission
Papers: MARK L. KAHN, Wayne State University; AARON W. WARNER, Columbia University
Discussion: PHILIP ARNOW, U.S. Department of Labor; ROBERT S. BOWERS, Western Michigan University; JOHN R. COLEMAN, Ford Foundation

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Knowledge, Information, and Innovation in the Soviet Economy
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Discussion: Alexander Eckstein, University of Michigan; Joseph S. Berliner, Brandeis University

2:30 P.M.

Public Finance: Promotion of Knowledge Production and Innovation
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Discussion: Douglas H. Eldridge, National Bureau of Economic Research; Martin J. Bailey, Institute for Defense Analyses; William Craig Stubblebine, University of Delaware and Massachusetts Institute of Technology

Labor Economics: Effects of More Knowledge
Chairman: Margaret S. Gordon, University of California, Berkeley
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Discussion: W. Lee Hansen, University of Wisconsin; Robert Evans, Jr., Brandeis University; Joseph Shister, State University of New York, Buffalo

4:00 P.M.

Invited Doctoral Dissertations
Chairman: George Leland Bach, Carnegie Institute of Technology
Papers: Vikaschandra S. Chitre, University of Toronto; John Conlisk, Rice University; Edward M. Gramlich, Board of Governors of the Federal Reserve System
Discussion: Allan H. Meltzer, Carnegie Institute of Technology; Robert M. Solow, Massachusetts Institute of Technology; Richard A. Musgrave, Harvard University

8:00 P.M.

Presidential Address[1]
Chairman: Calvin B. Hoover, Duke University
Speaker: Joseph J. Spengler, Duke University

Thursday, December 30, 1965

8:30 A.M.

Econometric Models for Educational Planning (Joint session with the Econometric Society)
Chairman: Andre Daniere, Harvard University
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Discussion: John Fei, Cornell University; James Henderson, University of Minnesota

Graduate Students' Papers
Chairman: Solomon Fabricant, New York University
Papers: Norman C. Miller, University of Pittsburgh; Feng-Yao Lee, Michigan State University; Roy W. Bahl, Jr., and Robert J. Saunders, University of Kentucky

[1] Published in the March, 1966, *A.E.R.*

Discussion: CHARLES J. SIEGMAN, Swarthmore College; HARLAND W. WHITMORE, Michigan State University; JOHN D. GUILFOIL, New York University

Economic Development: Advanced Technology for Poor Countries

Chairman: EDWARD S. MASON, Harvard University

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10:30 A.M.

International Economics: Progress and Transfer of Technical Knowledge

Chairman: HARRY G. JOHNSON, University of Chicago

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Discussion: STEPHEN HYMER, Yale University; BURTON A. WEISBROD, University of Wisconsin; HARRY G. JOHNSON, University of Chicago

The Economics of Publishing

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Antitrust and Patent Laws: Effects on Innovation

Chairman: CLAIR WILCOX, Swarthmore College

Papers: MARK S. MASSEL, Brookings Institution; JESSE W. MARKHAM, Princeton and Harvard Universities; ALMARIN PHILLIPS, University of Pennsylvania

Discussion: CORWIN D. EDWARDS, University of Oregon; ALFRED E. KAHN, Cornell University; RICHARD B. HEFLEBOWER, Northwestern University

The Production and Use of Economic Knowledge

Chairman: HOMER JONES, Federal Reserve Bank of St. Louis

Papers: RALPH L. NELSON, City University of New York; ROBERT D. CALKINS, Brookings Institution; MARTIN BRONFENBRENNER, Carnegie Institute of Technology

Discussion: HENRY H. VILLARD, City University of New York; JACOB L. MOSAK, United Nations; SOLOMON FABRICANT,[1] New York University

4:30 P.M.

Business Meeting

[1] No manuscript received.

THE purpose of the American Economic Association, according to its charter, is the encouragement of economic research, the issue of publications on economic subjects, and the encouragement of perfect freedom of economic discussion. The Association as such takes no partisan attitude, nor does it commit its members to any position on practical economic questions. It is the organ of no party, sect, or institution. Persons of all shades of economic opinion are found among its members, and widely different issues are given a hearing in its annual meetings and through its publications. The Association, therefore, assumes no responsibility for the opinions expressed by those who participate in its meetings. Needless to say, the papers presented are the personal opinions of the authors and do not commit the organizations or institutions with which they are associated.

HAROLD F. WILLIAMSON
Secretary

RICHARD T. ELY LECTURE

THE ECONOMICS OF KNOWLEDGE AND THE KNOWLEDGE OF ECONOMICS

By KENNETH E. BOULDING
University of Michigan

What might be called, perhaps somewhat grandiloquently, the Epistemological Question has received rather scant attention at the hands of economists.[1] There are, of course, a number of epistemological questions, some of which lie more in the province of the philosopher than they do the economist or the social scientist. The one with which I am particularly concerned here is that of the role of knowledge in social systems, both as a product of the past and as a determinant of the future. There is a little terminological problem here, that the word "knowledge" in English has some tendency to approach the meaning of "truth." We really have no convenient word to describe the content of the human mind without regard to the question as to whether this content corresponds to anything outside it. For this reason I have in the past used the term "image" to mean this cognitive content of the human mind.[2] But this term also is subject to misunderstanding, so for the purposes of this paper I will revert to the term "knowledge," with a warning, however, that I make no assumptions about the content of people's minds being true. We may recall the classic *bon mot* attributed to Will Rogers, that "the trouble isn't what people don't know; it's what they do know that isn't so." So little accustomed are we to analyzing this problem that there is even an ambiguity in the word "ignorance." It may mean that people have no image at all about something where an image is possible, or it may mean that they have images which are false or untrue. The pursuit of the question as to what we mean by truth or untruth, however, leads us into a philosophical morass from which, as David Hume suggested, the only escape is to climb out, clean oneself off, and go home and have a good dinner and forget all about philosophy. Otherwise we may be swallowed up in a paralyzing skepticism, and become, like Hamlet, "sicklied o'er with the pale cast of thought."

[1] Naming names is always a little invidious, but I must give honorable mention to F. A. Hayek, Fritz Machlup, T. E. Schultz, and Fred Harbison, as members of the little band who have taken this problem seriously.

[2] K. E. Boulding, *The Image: Knowledge in Life and Society* (Univ. of Michigan Press, 1956 and 1961).

I shall become very pragmatic at this point and consign the philosophical problems to my esteemed colleagues who make this their specialty, and I shall assume simply that knowledge, that is, images, exist; they can be observed or at least deduced through the instrument of language, combined with introspection; and that some images get us into more trouble than others; and that we tend to revise those images which get us into trouble. A decent, orderly, and at the same time imaginative and systematic revision of images that get us into trouble is a process which edges us, one hopes constantly, towards truth. This proposition, I must confess, is an act of faith. At its most sophisticated and orderly, this is the method of science. The same method, however, also produces images which approximate the truth in both what I would call folk knowledge, which is the knowledge gained in the ordinary living of daily life, and literary knowledge, which is folk knowledge chewed over, reflected upon, digested, and expanded by intakes from the written word.

I must resist the temptation to be philosophical, however, and come back to business; that is, economics. The question of what is economics can be almost as troublesome as what is knowledge? Here again I will be fairly ruthless and define economics as the study of the "econosphere" with a view of gaining knowledge about it, and I will go on to define the econosphere as that subset of the sociosphere, or the sphere of all human activity, relationships, and institutions, which is particularly characterized by the phenomenon of exchange. One might limit it further and consider only that part of the sphere of exchange which is subject, in A. C. Pigou's great phrase, to "the measuring rod of money." As I am a great believer in making boundaries of all kinds insignificant enough to be taken off the human agenda, in both the international system and in the republic of letters, I am not going to bother very much about where the boundary lies.

As it is exchange or potentiality of exchange or relevance to exchange that makes things commodities, one would think that economists would be interested in knowledge itself as a commodity. It is certainly something which is bought and sold. It is a little hard to put a price on it because of the difficulties of measuring the quantity of the commodity itself. We can put prices on the printed page, the hour's lecture, the newspaper, the tip sheet, or the newsletter and even perhaps on the golf course or the cocktail hour. The absence of any unit of knowledge itself, however, and perhaps the intrinsic heterogeneity of its substance, makes it very difficult to think of a price of knowledge as such, and indeed has probably contributed to a certain resistance which we feel to thinking of knowledge as a commodity. One longs, indeed, for a unit of knowledge, which perhaps might be called a "wit,"

analogous to the "bit" as used in information theory; but up to now at any rate no such practical unit has emerged. It is certainly tempting to think of knowledge as a capital stock of information, knowledge being to information what capital is to income, and to use the bit itself in the form of a stock as the measure of knowledge. Certainly the improbability of a structure, which is what the bit really measures, is highly related to the knowledge concept. The bit, however, abstracts completely from the content of either information or knowledge, and while it is enormously useful for telephone engineers, who have no interest in what is being said over their telephones, for purposes of the social system theorist we need a measure which takes account of significance and which would weight, for instance, the gossip of a teenager rather low and the communications over the hot line between Moscow and Washington rather high. Up to now we seem to have no way of doing this, short of a kind of qualitative guesswork, though even this will be better than nothing.

Another difficulty is that only things which are clearly capable of being appropriated are subject to being exchanged, and if a thing cannot be property, it obviously cannot be a commodity. While knowledge has many of the aspects of property, its capacity for reproduction in many minds and its accessibility in the form of the published word make it a very peculiar form of property. Thus as Major John Wesley Powell said to a congressional committee in 1886: "Possession of property is exclusive; possession of knowledge is not exclusive, for the knowledge which one man has may also be the possession of another."[3] In spite of Major Powell's dictum, some knowledge, of course, is exclusive, such as trade secrets and patents, and thereby becomes property. What is perhaps even more important, knowledge which has the capacity of generating more knowledge in a single head is also exclusive and becomes property to the individual possessing it.

These difficulties may have led to a certain neglect of the commodity aspects of knowledge, even in economic theory itself. One notices this in at least three areas of economic thought: in the theory of the market, in the theory of development, and in the theory of decision making, both public and private. In the theory of the competitive market, there is usually made an explicit assumption about "perfect knowledge." What this means in effect is that the acquisition of knowledge of prices or exchange opportunities in a perfect market is costless, so that knowledge is, as it were, a free good. This assumption might be plausible if there were only a few buyers and sellers. However, the perfect market also assumes large numbers of buyers and sellers, and pre-

[3] Quoted in Don K. Price, *The Scientific Estate*, p. 284, footnote 36 (Belknap-Harvard 1965).

sumably large numbers of prices, and the more prices there are, the more transactions there are, clearly the less plausible becomes the assumption that knowledge is costless. We can perhaps wriggle our way out of this dilemma by supposing that the knowledge problem in perfect markets is taken care of by specialized arbitrageurs, who by devoting themselves full time to the problem of knowing what prices there are in different parts of the market and by taking advantage themselves of the price differentials thereby revealed, reduce these price differentials to so small a quantity that all the rest of the people in the market are justified in assuming that the price which they happen to observe at one point is characteristic of all transactions all over the market. From a social point of view, the income of the arbitrageurs might be regarded as the cost of acquiring the knowledge which is necessary to operate the market, and the other people in the market are evidently willing to pay this rather than become arbitrageurs themselves.

We can then think of the development of imperfect markets as a result of the fact that when commodities become extremely diverse and complicated, when we have to know not only their price but also their quality, arbitrage in effect breaks down, because the cost of acquiring the relevant knowledge is more than the market is willing to support. Hence we get imperfect markets facing both buyers and sellers, in which they face not merely a price at which they can buy and sell as much as they wish but a function relating the amount that can be bought or sold to the price at which it can be bought or sold. Once we have imperfect markets, however, the epistemological problem for the marketers themselves increases enormously. If prices are advertised in a perfect market, or "cried," every seller knows his sales function and every buyer knows his purchase function immediately. If, however, we have an imperfect market, the problem of knowing what are the sales or purchase functions becomes not only acute but almost insoluble, simply because in order to know a function we must have experience with a system beyond its present point. It is this failure to understand the epistemological problem involved which has vitiated much of the otherwise laudable attempt to expand the theory of perfect competition into imperfect markets. This attempt which began so hopefully in the 1930's now seems to have petered out in an epistemological swamp.

When it comes to the theory of economic development, the failure to recognize explicitly the essentially epistemological nature of the problem has led to a proliferation of mechanical models of very doubtful value, and, one fears, the giving of a large amount of bad advice. The theory of economic development is part of the general problem of evolutionary change, and its poor condition reflects the general poverty of

the theory of dynamic systems. Throughout the sciences, physical, biological, and social, we are still really more at home with equilibrium systems than we are with dynamic systems.

The plain fact is that knowledge or something equivalent to it in the form of improbable structures is the only thing that can grow or evolve, and the concept is quite crucial in any evolutionary theory. As far as matter and energy are concerned, we are subject to inexorable laws of conservation. Here we are faced with simple exchange: what one system acquires, another system must give up. In the case of available energy, there is not even conservation; the second law of thermodynamics informs us there is constant degradation and decay. From the point of view of energy alone, the universe is clearly running down into a very thin brown soup, and all processes in time are seen merely as the exhaustion of preexisting potential, a kind of squandering of available energy capital. It is only information and knowledge processes which in any sense get out from under the iron laws of conservation and decay, though they only do this, as it were, by operating at another level. Two processes may be distinguished here. The first might be called printing, in which a structure is able to reproduce itself by making a copy of itself out of the incoherent matter around it. The gene evidently operates in this way; the mass production of commodities is largely three-dimensional printing; and even the transmission of a good deal of knowledge by rote learning in the educational process falls into this category. Printing by itself, however, would never organize an evolutionary or developmental process. It would merely fill the whole universe with copies of an initial structure. There must therefore be a second process to which we might give the name of organizing. This is the kind of process, for instance, by which the coded information contained in the gene is able to organize a phenotype such as a man. This is the way in which a blueprint organizes the construction of a building. This is the way in which an idea creates an organization, or an image of the future governs an individual life.

We then see any developmental process, whether this is the development of a fertilized egg into a human being, the development of an idea into an organization by an entrepreneur, the development of a religion out of a "sacred history," or even the process of economic development itself, as essentially a combination of printing and organizing, the one developing rote knowledge, the other new knowledge. Thus we can think of capital essentially as knowledge imposed on the material world, in the first place by an organizing process which creates a producing organization and in the second case by a process akin to three-dimensional printing. In this view, consumption is essentially consumption of knowledge-structures, either human knowledge

through death or decay, or of the bodily structure through metabolic processes, or through wear and tear of material structures, or even through the disorganizing processes which afflict organizations. Production is then seen essentially as a process of increasing structure, repairing the decay and depreciation of consumption, replacing the knowledge lost by death, and so on. We could further think of production as having two functions: one a replacement function, which is necessary to restore an existing knowledge and capital structure; the other a developmental function which expands, improves, and reorganizes the structure of knowledge in general into new forms. If consumption is so great that all production has to be used for maintenance, there will, of course, be no development. We also get certain consumption processes which can be remedied by no known input, such as aging. Fortunately in society we have solved this problem by having babies, and in organizations we solve it by having competition, bankruptcy, and various forms of organizational death. Birth and death, indeed, are the price that we pay for aging, so that we can have a population that does not age, even though the individuals do.

The recognition that development, even economic development, is essentially a knowledge process has been slowly penetrating the minds of economists, but we are still too much obsessed by mechanical models, capital-income ratios, and even input-output tables, to the neglect of the study of the learning process which is the real key to development. It is true, of course, that what might be called the "human resources school" of Theodore Schultz and Fred Harbison has laid very proper stress on education as the mainspring of the developmental process. Even here, however, there has perhaps not been sufficient attention paid to the problem of learning as a whole, outside as well as inside the institutions of formal education; and there has been a considerable neglect of the role of the price system as a teacher.

It is always depressing to go back to Adam Smith, especially on economic development, as one realizes how little we have learned in nearly two hundred years. It is, however, perhaps worthy of notice that our father Adam saw very clearly that the learning process was the key to development, for if we examine his causes of the increase in the productive powers of labor, which is what we mean by economic development, we see that they all involve the knowledge process. The first of these, the development of skill and dexterity through the division of labor, is a learning process mainly in the lower nervous system. The second, the gains due to constant application at a single task and the elimination of "sauntering," involve the problem of forgetting and relearning as we take up tasks intermittently; and the third, and by far the most important, is the development of machines (frozen knowl-

edge, as I would call them) as a result of the work not only of specialists in the production of such things, but also as the result of the work of "philosophers" who augment knowledge in general. Thus even before 1776 Adam Smith had perceived the enormous importance of what today we would call research and development in the processes by which everybody gets richer.

The third area of interest to economists where the epistemological problem is overwhelmingly important is in the area of decision making itself, in the private sector, in households and businesses, and in government; for the problem of government policy is just as much a problem in decision making as is the problem of the behavior of private persons and organizations. In my book, *The Image*, I have sketched what might be called an epistemological theory of behavior, pointing out that a decision is always a choice among alternative perceived images of the future. The study of decision, therefore, must concentrate on how these images of the future are derived from the information inputs of the past, as this is the only place from which they can come. That is, we have to think of our images of the future as essentially learned out of our inputs from the past, and the nature of this learning process is therefore of overwhelming importance. Similarly, the utility or welfare function, which we impose over these images of the future, is likewise learned, though economists have been surprisingly unwilling to recognize this fact, perhaps because it was called to their attention in such strident tones by Veblen, who argued most convincingly, to my mind, that if we wanted to have a dynamic economics, we could not simply take preferences for granted but had to regard them as essentially learned. The process by which we learn our preferences, however, is mysterious indeed. A substantial monkey wrench is thrown into dynamic economics by the fact that the price system itself may operate as a teacher, and preferences may change in response to the price structure just as the price structure changes in response to preferences. We have, for instance, what might be called the "sour grapes" principle—that what we cannot get we decide we do not like. There is also a counterprinciple that might be called the "Mount Everest" principle, that if something is hard to get, we want it, just because it is hard to get. Furthermore, if we know somebody else has paid a different price from what we have paid, our satisfaction may be correspondingly increased or diminished.

The epistemological theory of decision making is, of course, pretty empty unless we can specify ways in which the inputs of the past determine the present images of the future. Unfortunately, the observations of economists on this question are for the most part simple-minded to the point of embarrassment. The concept of elasticity of expecta-

tions, for instance, would only be interesting if there were any evidence at all that as a parameter it had some stability, or even that its rate of change had some stability. There may be some stability in expectations when there is nothing to expect, that is, in a poor, stable environment, but outside of this the evidence for any simple relationship between present rates of change and future is not well supported. Perhaps the most plausible theory is that people tend to interpret the present in terms of the traumatic experiences of their youth. Thus a generation that was traumatized by inflation will have different images from one traumatized by depression. It is clear we are on the borderline here between economics and psychology, and it is to the interstitial discipline of economic psychology that we must look for answers. The trouble is, of course, that even psychology knows very little about the human learning process, mainly because it takes place over such a long period and is almost certainly subject to phenomena such as "imprinting" in which inputs at certain moments of "readiness" in the development of the person produce effects which far outweigh their intrinsic importance.

Another profitable line of study lies in economic sociology, in the analysis of the way in which organizational structure affects the flow of information, hence affects the information input into the decision-maker, hence affects his image of the future and his decisions, even perhaps his value function. There is a great deal of evidence that almost all organizational structures tend to produce false images in the decision-maker, and that the larger and more authoritarian the organization, the better the chance that its top decision-makers will be operating in purely imaginary worlds. This perhaps is the most fundamental reason for supposing that there are ultimately diminishing returns to scale. In the most extreme form of this view, we can suppose that the role structure and communication network of an organization determine the inputs to each role so completely that there is virtually no freedom of decision at all, and that no matter who is the role occupant, the decisions will be much the same. The inference of this theory, of course, is that fools in high places will make just the same decisions as wise men, and though there is something comforting in this, one certainly hesitates to believe it too wholeheartedly.

Let me now focus my attention even more narrowly on the problem of the contribution of economic knowledge itself, that is, what economists know, to the processes of operation of the economic system. We have here a certain epistemological paradox, that where knowledge is an essential part of the system, knowledge about the system changes the system itself. This is a kind of generalized Heisenberg Principle, which is particularly troublesome in the social sciences. What this

means, of course, is not that knowledge is unattainable, but that we must regard it as part of a total dynamic system. That is to say, we are not simply acquiring knowledge about a static system which stays put, but acquiring knowledge about a whole dynamic process in which the acquisition of the knowledge itself is a part of the process. It is quite legitimate, therefore, to ask ourselves what is the impact of economic knowledge, that is, of the image of the economic system or econosphere, in the minds of professional economists, on the dynamic processes of the econosphere itself. The only point at which knowledge can affect a social system is through its impact on decisions. This impact can be small or large, depending on the relevance of the knowledge in question. Thus in the case of the operations of a market and the behavior involved in buying and selling, it is doubtful whether the knowledge of economics as such makes very much difference. Economists, for instance, have not been noted for their success in market speculation, with two notable exceptions of Ricardo and J. M. Keynes, and even in their cases, they made their major contributions to economics after, not before, they made fortunes in speculative markets. There are certainly few marketers who have been assisted in their operations by knowledge of the Walrasian Equations, just as few tennis players are much assisted by knowledge of the mathematics of moving balls.

At some points, however, economic knowledge is showing some danger of being useful. Economists can take a good deal of credit for the stabilization policies which have been followed in most Western countries since 1945 with considerable success. It is easy to generate a euphoric and self-congratulatory mood when one compares the twenty years after the first World War, 1919-39, with the twenty years after the second, 1945-65. The first twenty years were a total failure; the second twenty years, at least as far as economic policy is concerned, have been a modest success. We have not had any great depression; we have not had any serious financial collapse; and on the whole we have had much higher rates of development in most parts of the world than we had in the 1920's and 1930's, even though there are some conspicuous failures. Whether the unprecedented rates of economic growth of the last twenty years, for instance in Japan and Western Europe, can be attributed to economics, or whether they represent a combination of good luck in political decision making with the expanding impact of the natural and biological sciences on the economy, is something we might argue. I am inclined to attribute a good deal to good luck and noneconomic forces, but not all of it, and even if economics only contributed 10 percent, this would amount to a very handsome rate of return indeed, considering the very small amount of resources we have really put into economics.

Another point where the knowledge of economics has had som payoffs in the social system has been through the development of ope ations research and management science, with the aid of comput technology. Here again it is not altogether clear how much economi itself has contributed to this, as the basic ideas, for instance of max mizing something under constraints, are so obvious that it is almo embarrassing to credit economics with them, and it is the technolog that has really made the difference. However, I suppose it can b argued that if economics had not beaten out the marginal analysis wit an intellectual sledge hammer over a couple of generations, the com puter boys might have had to spend a few minutes in thinking abo what they were doing. Some of us, perhaps, still have to learn th arithmetic is a complement to, not a substitute for, thought, and th what my spy in IBM calls the "gigo principle" (that is, garbage i garbage out) is a sound approach even to the most elegantly compute ized simulation. I confess I am a little worried about one aspect of th movement, fruitful as it undoubtedly is. The very power of the com puter to simulate complex systems by very high-speed arithmetic ma prevent search for those simplified formulations which are the essen of progress in theory. I have an uneasy feeling, for instance, that if th computer had been around at the time of Copernicus, nobody wou have ever bothered with him, because the computers could have han dled the Ptolemaic epicycles with perfect ease.

The general movement towards the rationalization of decision-mak ing processes in both private and public life through the use of opt mizing procedures applied to complex masses of information may hav some other costs lurking among the benefits, particularly in regard t political decision making. For one thing, these elaborate procedur may easily produce a sense of subjective certainty, which is quite un warranted by the uncertainties of the actual system. One worries abou this particularly in the international system, where the principle tha "he who hesitates is saved" is usually very sound, and an illusion c certainty can be quite disastrous. The use of political war games an of computer simulation in the Department of Defense is a genuin cause for alarm on this score, and one would very much like to se some studies of the effect of gaming, for instance, on business beha ior. It could easily be that the euphoria produced by these exercise resulted in some disastrous decisions, though I have not been able t document this hypothesis. The great danger of rationality is of cours suboptimization; that is, finding and choosing the best position of par of the system which is not the best for the whole. Too many peopl indeed, and especially too many experts, devote their lives to findin

the best way of doing something that should not be done at all. Decision making by instinct, gossip, visceral feeling, and political savvy may stand pretty low on the scale of total rationality, but it may have the virtue of being able to take in very large systems in a crude and vague way, whereas the rationalized processes can only take subsystems in their more exact fashion, and being rational about subsystems may be worse than being not very rational about the system as a whole. I would not argue, of course, that rationality about the system as a whole is impossible. On the other hand, the economist has a certain mind-set in favor of his own skills, and it is easy for him to leave out essential variables with which he is not familiar. Here, indeed, a little learning may be a dangerous thing, or even a little rationality.

One area where economists have a good deal to be humble about is in the field of economic development of the poor countries. In the rich countries we have done fairly well; in the poor countries our record is distinctly spotty. This is almost certainly because we are dealing in this case with a total social process, and the economic abstractions are simply not sufficient to deal with the problem. Here what we need is clearly economic anthropology, and this science, unfortunately, hardly exists. Our great gift to the world is national income statistics and the percentage rate of growth of GNP. In fact, as every economist knows, calculations of GNP, especially in the poor countries, are largely exercises in the statistical imagination, and even if they were accurate, the GNP itself can be a very poor measure of welfare. The GNP can rise because of arms races, because of stupid dam-building, or even through the building of presidential palaces. It can be rising because a small proportion of a population is getting better off while the vast majority remain in stagnant misery. Valuable as the GNP is, therefore, as a rough overall measure of economic success, it can easily become a fetish and a quite misleading statistic. Economists certainly should be the first to issue warnings against its misuse.

Let me conclude with some brief notes on the state of economic knowledge in the United States. At the moment I get the impression that economists in this country are bathed in a warm glow of self-congratulation, rising out of the long Kennedy-Johnson upswing and the successful tax cut, and they are all climbing onto the bandwagon of the Great Society, waving flags and tooting horns. That we have some causes for self-congratulation I would not deny, and I hate to seem like a skeleton at the feast. There is real danger, however, that our current euphoria will prevent us from seeing the immensity of our unsolved problems and the enormous intellectual task that still awaits us. It is not much to the credit of the economics profession, for instance,

that it took an engineer, Seymour Melman,[4] to call our attention to the fact that our obsession with being a great power and our neurotic masculine compulsions about military strength are seriously depleting the technical resource base in the civilian sectors of the economy. The nonsense which is talked about cyberculture and the hooting and hollering about automation at a time when substantial segments of the economy are technologically stagnant or even deteriorating is another tribute to a major intellectual default on the part of the economics profession. The plain fact is that economists have neglected the study of technical change at the structural and micro level to the point where we are quite incapable of answering many of the most important questions of our day. We have been obsessed with macroeconomics, with piddling refinements in mathematical models, and with the monumentally unsuccessful exercise in welfare economics which has preoccupied a whole generation with a dead end, to the almost total neglect of some of the major problems of our day. Almost the only group of economists who have much sense of realism are the agricultural economists, and these are dealing with a vanishing sector that is now only 5 percent of the total economy. The whole economics profession, indeed, is an example of that monumental misallocation of intellectual resources which is one of the most striking phenomena of our times. It would be an interesting exercise to compare the distribution of economists specializing in different sectors of the economy with the contribution of these sectors to the GNP. I would not be surprised to find 75 percent of the economists are concentrated in 10 percent of the GNP. Where, for instance, are the economists who are really studying the service trades and the tertiary industries? Where are the economists who are really studying the 10 percent of the economy devoted to the space-military complex? Where are the economists even who are really studying the impact of automation? And the answer is, practically nowhere. Far from being in a mood of self-congratulation, we should be in a mood of repentance.

A mood of repentance, however, implies a hope of salvation. It is on this note that I would like to conclude. In almost every generation, the oldsters mourn that things are not what they were in their young days. I remember Hicks once telling me that he heard Foxwell's last lectures at London School of Economics, in which he commiserated with the young men of the 1920's that they lived in a dull age of economics, and that they could never hope to recapture the enormous thrills of the bimetallism controversy. One is tempted to sing the same song today, in describing the Keynesian raptures of the 1930's. "Bliss was it in that dawn to be alive, but to be young was very heaven," as the aging

[4] Seymour Melman, *Our Depleted Society* (Holt, Rinehart, and Winston, 1965).

Wordsworth wrote about the French Revolution. Little indeed did Foxwell know. It is tempting to say, "Those were the days," and leave it at that. But these are the days too. It may be, of course, that the intellectual fervor which in the 1930's we devoted to the problem of unemployment must now be devoted to the graver problem of human survival in an international system which has clearly broken down. It may be that intellectual excitement has shifted from economics towards political science or towards social psychology. Let us not think, however, that all our problems have been solved. An enormous intellectual task still awaits the economist. We are a very long way from writing finis to this chapter of the human enterprise. We still cannot handle some of the most elementary problems regarding economic development, economic dynamics, the function of the price system, the relative merits of centrally planned as against market economies, the economics of distribution, the development of the "grants economy," the behavior of economic organizations of all types, from the corporation to the foundation, the role of the price system in the developmental and learning process, the learning process itself by which we acquire our images of our economic environment. We are still, like Isaac Newton, only a boy playing on the seashore, and the great ocean of Truth still lies all undiscovered before us. That undiscovered ocean is Man himself. What we discover about him, I hope, will be for his healing. I did not become an economist for anybody's applause; I became an economist because I thought there was an intellectual task ahead, of desperate importance for the welfare and even the survival of mankind. A mere thirty-five years have not been long enough to change my motivation. Something has been accomplished; a great deal more remains to be done. To this unfinished task I commend us all.

ALLOCATION AND DISTRIBUTION THEORY: TECHNOLOGICAL INNOVATION AND PROGRESS

CHANGE AND INNOVATION IN THE TECHNOLOGY OF CONSUMPTION

By KELVIN LANCASTER
Johns Hopkins University

We typically think of technology as applying to production rather than consumption, and my first task is to establish just what I mean by the technology of consumption.

I am drawing on ideas which have been set out in some detail in another paper of mine which is to be, but unfortunately has not yet been, published elsewhere.[1] This paper, "A New Approach to Consumer Theory," sets out a model of consumption and the consumer with certain features which provide the basis for the present explorations. I must necessarily start with a brief description of those features.

"A New Approach. . ." presents the following view of consumption. Goods, as such, are not the immediate objects of preference or utility or welfare, but have associated with them characteristics which are directly relevant to the consumer. The term "characteristics" was chosen for its normative neutrality; in my earliest draft of this idea I called them "satisfactions," but that has too many connotations. The consumer is assumed to have a preference ordering over the set of all possible characteristics vectors, and his aim is to attain his most desired bundle of characteristics subject to the constraints of the situation. The consumer's demand for goods arises from the fact that goods are required to obtain characteristics and is a derived demand.

An analogy to production theory is starting to appear. We are viewing goods as inputs into a process in which these characteristics are the outputs. The structure of consumption activities is, however, typically different from the structure of production activities. In the typical production activity we have joint inputs and a single output, while we shall regard the typical consumption activity as having a single input (a good) and joint outputs (a bundle of characteristics). Some consumption activities may require several goods, or even other inputs. For example, the activity driving a car requires the use of a consumer capital good, the using up of other goods (gas and oil), and the labor

[1] In the *J.P.E.*, Apr., 1966

of the consumer to give the bundle of characteristics associated with the activity. If we were discussing the theory of consumer durables, we would pursue this example further, but, in the present context, we shall think of the typical consumption activity as using up a unit of some good and deriving the bundle of joint characteristics from it.

The jointness of the characteristics is really the core of the whole approach. If we eat an apple, we are enjoying a bundle of characteristics—flavor, texture, juiciness. Another apple may have the same flavor but associated with a different texture, or be more or less juicy. A single good may have more than one characteristic, and a single characteristic may be obtainable from more than one good. Goods which share a common characteristic may have their other characteristics qualitatively different, or they may give the same characteristics but in a quantitatively different combination. If the relationship between goods and characteristics was merely one-to-one in both directions, so that the only characteristic of an apple was appleness and the only source of appleness was an apple, then there would be no operational difference between the traditional approach to consumer theory and that being portrayed here.

It will be assumed that characteristics are, in principle, intrinsic and objective properties of consumption activities. Given arbitrary units, each consumption activity is defined by its inputs (most often assumed to be a unit of a single good) and by the vector of characteristics which forms its output. It will further be assumed that the activities are linearly homogeneous, so that doubling the goods input gives double the characteristics. Essentially psychological effects, such as the consumer's relative interest in different characteristics or effects similar to diminishing marginal utility, are assumed to make their appearance in the preference ordering of the characteristics vectors, not in the relationship between goods and characteristics.

The set of all possible consumption activities forms the consumption technology. In a highly developed economy, with many different goods and product variants, the technology will be complex; in a less developed economy, the technology will be simpler. In a country like the U.S.S.R. we may have a complex production technology combined with a relatively simpler consumption technology.

The consumption technology will relate goods on the one hand with characteristics on the other. In general, there is no reason why the number of characteristics and the number of goods should be related to each other (any more than the number of goods and the number of factors should be related in the production technology), and I shall make the working hypothesis that the number of goods in a complex consumption technology like that of the U.S. will probably exceed the

number of operationally distinguishable characteristics. There may well be several combinations of goods which give rise to the same bundle of characteristics, and this gives rise to a very important distinction between the present and traditional approaches to consumer theory.

Consider a simple example of a consumer in a world of two characteristics and three goods. Each good gives rise to a vector of the two characteristics, and the consumption technology consists of the activities, consuming each of the goods separately, and consuming them in linear combination. If we impose a budget constraint on the goods, we can explore the characteristics vectors attainable by the consumer. The attached diagram shows the two-dimensional characteristics space and the points A, B represent the characteristics attainable if the whole

CAPITAL THEORY: TECHNICAL PROGRESS AND CAPITAL STRUCTURE

SOURCES OF MEASURED PRODUCTIVITY CHANGE: CAPITAL INPUT*

By Zvi Griliches, *University of Chicago*
and Dale W. Jorgenson, *University of California, Berkeley*

I. *Introduction*

In accord with the general theme, "Technical Progress and Capital Structure," the purpose of this paper is to study the relationship between the structure of capital and measured productivity change. Our first objective is to develop capital theoretic methods for the measurement of capital input. We apply these methods to the measurement of capital input in the U.S. private domestic economy, 1929–64. We then present evidence on the role of changes in the structure of capital in the explanation of measured changes in total factor productivity. In previous studies the effects of changes in the structure of capital have not been properly accounted for, giving rise to significant errors of measurement. Our conclusion is that such errors are an important source of measured productivity change. As a consequence the proportion of growth in output assigned to the residual, our ignorance, or advance of knowledge has been substantially overstated.

II. *Capital Theory and Productivity Measurement*

If capital services were bought and sold by distinct economic units in the same way as labor services, there would be no conceptual or empirical difference between the construction of a quantity index of total capital input and the construction of the corresponding index of total labor input. Beginning with data on the value of transactions in each type of capital service, this value could be separated into a price of capital service or rental and a quantity of capital service in, say, machine-hours. A quantity index of total capital input would be constructed from the quantities of each type of capital service, using the relative shares of the rental value of each capital service in the rental value of all capital services as weights.

The measurement of capital services is less straightforward than the measurement of labor services because the consumer of a capital service

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the ratio of assets to income, then it becomes child's play to explain the long-run stability of income shares. All you still need, then, is something to explain, say, the stability of the rate of return on capital. This is quite likely to be maintained by appropriate changes in the rate of capital accumulation.

Should this turn out to be not only a simple but also a valid explanation, we would have a macroeconomic theory of income distribution that would definitely not be neoclassical but neither could it be called Kaldorian. Explanations of this kind seem to me not only the least fancy but also the most promising, because the stability and regularity of group behavior is probably the greatest in the case of households.

consumption function to explain the functional relation between such nonobservable and nonmeasurable variables as permanent income and permanent consumption; we have a new and better theory of money the better to explain the demand, not for money, but for that fluid and slippery thing, liquidity.

Let me now come to Professor Corry's paper. It is a very thoughtful paper; but I am not quite certain I know what his main concern is. Is he trying to explain the relative constancy of income shares or does he want to choose the best among competing theories of income distribution? He rightly points out that none of these is contradicted by the constancy of income shares, because none explains it. Each needs some additional assumptions to explain this constancy; and the only difference between competing distribution theories is that the additional assumptions needed by some are more fancy than those needed by others.

The one explanation Dr. Corry himself offers to explain the relative constancy of income shares is the inverse relation between price and quantity, between the price and quantity of labor and the price and quantity of capital. Here I should like to point out that the two are inversely related only when the disturbance comes from the supply side. Only a shift in the supply curve will cause price and quantity to move in opposite directions. A shift in demand will cause them to move in the same direction. What is also needed, therefore, is an argument or proof that disturbance always comes from the supply side. This, as far as I know, has not yet been supplied; although there is a new theory of innovation that presents all innovation as equivalent to an increase in the supply either of labor or of capital.

As to the choice between alternative theories of income distribution, to my mind, differences in the degree of fanciness of the additional assumptions needed are a valid criterion of choice. Is not an important reason why we prefer the heliocentric to the geocentric theory of the universe that an elliptical orbit of the planets is less fancy than a cycloidal one?

I should like to submit in the same spirit that the theory of innovation-bias needed to explain the constancy of income shares in the framework of the neoclassical theory of income distribution is considerably more fancy and less believable than some of the alternative additional assumptions. In this connection, I should also like to draw attention to an exceptionally simple and nonfancy "addition assumption" that Dr. Corry has ignored and I too have ignored when, some time ago, I wrote on a related subject. I am referring to the Pigou effect. We now know that this is too weak to eliminate Keynesian underemployment equilibrium; but it is strong enough to maintain an approximate long-run stability in the asset-to-income ratio, which is a first cousin to the capital-output ratio. Professor Modigliani has done a lot of work on this in connection with his work on the consumption function; and especially his latest article, soon to be published in *Econometrica,* argues persuasively that the consumer's wish to keep his asset holdings in a certain relation to his income is a more important stabilizing force than the corresponding forces on the production side. If Modigliani is right, and I for one am willing to believe that he is right, and that it is the behavior of households that keeps constant

ing problem in his discussion, raised also by Professor Corry, is the question of whether innovations have the effect of counteracting the favorable influence of capital accumulation on wages. I rather tend to agree with Professor Corry that too much attention has been paid to labor's relative share rather than to the wage rate or (under conditions of inflexible wages) unemployment. One aspect of this problem which I have not seen discussed is this: if there are two factors of production (labor and capital) and constant returns to scale, and if there are positive marginal products and diminishing returns to each factor, then an increase in capital necessarily raises the marginal product of labor; for if the production function is $Q=F(L, K)$ then by Euler's theorem we have $F_L L+F_K K=Q$, and differentiating this identity with respect to K we obtain $F_{LK}L+F_{KK}K=0$, hence diminishing marginal productivity $F_{KK}<0$ and positive employments of the factors implies $F_{LK}>0$; i.e., a rise in capital must increase the marginal product of labor. Whether this carries over to the case of more than two factors (assuming a strictly quasi-concave production function) appears to be an open question. If it does not, then clearly the kind of one- and two-sector models that have been so much in vogue recently must be regarded as hiding one of the most interesting aspects of the situation. But within the framework of a two-factor model it is clear that one must invoke innovation rather than simply capital accumulation in order to find a mechanism for counteracting a rising trend in real wages.

TIBOR SCITOVSKY: Before I would comment on Dr. Corry's paper, which is what I am supposed to comment upon, I should just like to say how much I welcome Professor Lancaster's new approach to the theory of consumer's demand. There is nothing more frustrating than to watch, and watch silently, the stupid way in which some people squander their money and get little to show for it. I used to be told that I may not even criticize them, since all they were doing was to spend money their own way, and I as an economist must respect the other consumer's sovereignty. Now Professor Lancaster has relieved my frustration. At last I can look down my nose, without a pang of professional conscience, upon the sorry mess some people make of the noble art of spending money. Now I can respect the poor sucker's sovereignty and still criticize him for his inefficiency in catering to his own sovereign tastes. Another advantage of the new approach is that at long last we can embrace and accept as an equal a much maligned and looked-down upon colleague, the home economist. It is now clear that her work is no more humble, no more lowly, than that of the management scientist and the operations researcher.

This is genuine advance; but, like most advances, it has been obtained at a cost. Before Lancaster we not only called a spade a spade but could actually count the number of spades produced and consumed. From now onwards, we shall have to measure the supply of and demand for digging characteristics; and I do not look forward to studying and having to learn the ingenious new statistical techniques that will soon be developed for measuring the production and consumption of this or that consumer characteristic or vector of consumer characteristics. All this is part of a general trend. We have a new and better

the same kind of thing as the approach of Heckscher and Ohlin has done for production theory.

These considerations give rise to important consequences for general equilibrium theory: if the number of characteristics exceeds or equals the number of factors of production, there is a theoretical presumption in favor of a corner solution (particularly if tastes are similar), in which case we may expect (contrary to Lancaster's claim) that the number of commodities produced will turn out to be equal to the number of characteristics. Conversely, if the number of factors exceeds the number of characteristics, the taste pattern may be expected to exert a dominant influence on equilibrium prices. Numbers of characteristics and numbers of factors become the two blades of Marshall's scissors.

Lancaster's approach is quite similar in one respect to the theory of separable utility developed by Sono (1945), Leontief (1947), Strotz (1957), Rajaoja (1958), and Gorman (1959). Both approaches have the property that

$$U = f(x_1, x_2, \cdots, x_n) = g(h_1(x), h_2(x), \cdots, h_m(x)).$$

The simplification introduced by separability is to restrict the arguments of each function h_i to a separate group of commodities, whereas Lancaster's simplification is to make each h_i a linear function of quantities of all the commodities. The old assumption of independent utilities is—it should be noted—a special case of both.

Lancaster relies heavily on the monotonicity assumption: $y' \geq y''$ implies $g(y') > g(y'')$. Indeed this seems to make more sense than the corresponding assumption in terms of commodities ($x' \geq x''$ implies $f(x') > f(x'')$), though the latter is implied by the former as long as $a_{ij} \geqq 0$ and

$$\sum_{j=1}^{n} a_{ij} \neq 0.$$

This plus the linearity give rise to the appealing twofold nature of choice in his scheme: first find the efficient set and then choose the optimal bundle from this set. The same analysis could be carried out in the commodity space, since the slope of the flat subsets of the indifference surfaces depend only on A and not on g. I feel there is one respect, however, in which the analogy with production theory has been overdrawn. Lancaster appears to take it for granted that the coefficients a_{ij} relating characteristics levels to product levels are the same for all consumers, but that the utility functions $g(y)$ describe the "private choice" of different individuals. This may well be, but it is a postulate and not a consequence of Lancaster's scheme, and should be stated as such. Further, it could be and deserves to be tested. In the meantime I shall remain a little anxious, lest on Professor Lancaster's authority the Consumers Union should advise me to conform to Stigler's efficient diet consisting largely of navy beans and tapioca.

Coming to Professor Fellner's paper, I am once again handicapped by the fact that he builds on results contained in an article by Samuelson which (as of yesterday) has not yet appeared. What strikes me as the most interest-

Professors Fellner and Corry have done, to the more difficult job of constructing a theory of induced making of technical progress.

JOHN S. CHIPMAN: In discussing Professor Lancaster's paper I feel like one of his inefficient consumers, since one of the characteristics of his product is an as yet unpublished article to which I have not had access. I hope he will forgive me, therefore, if I raise points which he may have settled in that paper.

The essential idea underlying Lancaster's approach is an old one going back to Menger, but it has either been expressed somewhat vaguely or differently, or when formulated in Lancaster's express terms has never formed a central part of conventional theory. Perhaps the first explicit treatment of a product as a bundle of attributes or characteristics is to be found in the formulation of the famous diet problem by Jerome Cornfield in 1941 and George Stigler in 1945. The concept was used again in 1954 by Kenneth May, who was mainly concerned with explaining how it might serve to reveal a possible source of intransitivity. The idea appears in the writings of Chamberlin (1933), Brems (1951), Hofsten (1952), Theil (1953), Houthakker (1953), Stone (1956), V. Smith (1959), and Adelman and Griliches (1961). It must be admitted, however, that none of these earlier developments made much of a dent on consumption theory. In showing how such a dent can be made, Lancaster must be congratulated for introducing an idea which is original, promising, and thoroughly sound.

While Lancaster leaves it open whether the number of characteristics is greater or less than the number of products, I think that the interesting and fruitful hypothesis is that it is less. Let us consider some of the implications of this for conventional theory. Utility is a function of the quantities of the m characteristics: $U=g(y_1, y_2, \cdots, y_m)$. The jth product being a bundle or column vector $\{a_{1j}, a_{2j}, \cdots, a_{mj}\}$ of characteristics, the amount of each characteristic can be expressed as a quantity index, i.e., a weighted average

$$y_i = \sum_{j=1}^{n} a_{ij}x_j$$

of consumption levels. In terms of the latter the conventional utility function becomes

$$U = f(x_1, x_2, \cdots, x_n) = g\left(\sum_{j=1}^{n} a_{1j}x_j, \sum_{j=1}^{n} a_{2j}x_j, \cdots, \sum_{j=1}^{n} a_{mj}x_j\right)$$

or $U=f(x)=g(Ax)=g(y)$ in matrix notation. If $m<n$ (fewer characteristics than products) then the indifference surfaces in the n-dimensional commodity space are ruled, containing $(n-m)$-dimensional flats; that is, the Lancasterian utility function has some built-in perfect substitutability. This is entirely analogous to what is variously called Samuelson's substitution theorem or nonsubstitution theorem in production: here, if the number of factors is less than the number of products, the transformation surface will be ruled. Thus, Lancaster's approach does for consumption theory much

progress in the special sense of increased consumption efficiency, even with no change in the nature of goods or consumption activities." But if so, under his own definition there is no technical change whatsoever—let alone technical progress. All that is happening is that consumers choose more intelligently from the same set of possible consumption activities. Progress it is, but not technical progress.

Turning now to the technology of production rather than consumption, a prominent problem, to which both Professor Fellner and Professor Corry have an affirmative answer, is whether or not technical progress is induced. My own feeling is that this problem really has two distinct parts: an easier one and a more difficult one. The difficult one is whether or not the making of technical progress is induced; the easier one is whether or not the adoption of technical progress, once made, is induced. It is, perhaps, typical of our research strategy that we all seem to be attacking head-on the difficult part, often without even seeing the easier part. The reason we do not see it, I suspect, is that in their original forms neither the neo-Keynesian nor the neoclassical growth models came to grips with embodied technical progress. These models were net models in the sense that saving, investment, and output were always net saving, net investment, and net output. But once embodied technical progress is faced squarely, retirement of durable producers' goods moves into prominence, and a gross model will have to be formulated, for now technical progress simply cannot be adopted by a firm until the firm retires its durable producers' goods and replaces them with new advanced ones. In short, *it is not enough for a firm to make technical progress*; the firm must also adopt it; and there must be an optimum time rate at which to do so. Producers' goods should not be retired too soon, or the cost of acquiring them occurs too frequently. They should not be retired too late either, or they put their owner at a competitive disadvantaged vis-à-vis firms, actual or potential, owning newer more advanced producers' goods. When exactly should producers' goods be retired? Intuition tells us that the higher the rate of interest, the more urgent it becomes to save capital cost by lengthening the useful life of producers' goods. Our intuition may easily be confirmed by formal rigor.

Having established the relationship between rate of interest and optimum useful life of producers' goods, we conclude that whether the making of technical progress is induced or not, the adoption of it surely is. Two economies facing the same technical progress will adopt it at different speeds as long as their interest rates differ. The low-rate-of-interest economy will adopt the same technical progress faster than will the high-rate-of-interest economy. Empirically this result makes good sense. It is well known that in some industries of the capital-short underdeveloped economies the useful life of plant and equipment is much longer than in the corresponding industries of the capital-rich developed economies, and observers from the latter are often struck by the antiquated equipment still in use in the former economies. Observations like that can be explained only by vintage models of growth, not mentioned in any of our three papers. Setting out the easier theory of induced adoption of technical progress, however, should keep no one from devoting himself, as

DISCUSSION

HANS BREMS: Professor Lancaster's paper represents a very elegant and fresh approach to the theory of consumption. As one reads it, things begin to fall into place. Personally, I suddenly understood much better why I often agree with all the premises of a *Consumer Reports* article, yet disagree with its best-buy conclusion. Like Consumers Union, I understand that a unitized automobile body is a package of two Lancaster characteristics: first, superior structural rigidity but, second, considerable transmission of wheels-to-road noise. But structural rigidity seems to rank much higher on my preference scale than it does on that adopted by Consumers Union.

Professor Lancaster defines consumption technology as the set of all possible consumption activities. Like most good economists, Professor Lancaster does not stick to his own definition; so I shall feel free to try to sharpen it in order to bring out the almost perfect analogy between a Lancaster consumption technology and a Von Neumann or Leontief production technology.

Skipping a rather difficult subject, let us assume all Lancaster characteristics to be measurable, and let there be n of them. Let there be m different consumers' goods. Let c_{ij} be the quantity of the ith Lancaster characteristic obtained from the jth consumers' good, where $i = 1, \ldots, n$, and $j = 1, \ldots, m$. Let C_j be the quantity consumed of the jth consumers' good. One may then define an input-output coefficient a_{ij} as the quantity of the ith characteristic obtained per unit of consumption of the jth consumers' good:

$$c_{ij} = a_{ij} C_j.$$

The coefficient a_{ij} is called an input-output coefficient, because the consumption of the jth consumers' good is considered an input, and the resulting quantity obtained of the ith characteristic is considered an output. Of such input-output coefficients there are clearly mn. My suggestion is to define a Lancaster consumption technology as simply the m times n matrix of the input-output coefficients a_{ij}. Like a Von Neumann production technology matrix —but unlike a Leontief one—a Lancaster consumption technology matrix need not be square; indeed Lancaster himself thinks that for the United States, $m > n$. And just like a Von Neuman or an Leontief matrix, a Lancaster matrix may contain a large number of elements whose value is zero. After all, no miles per hour are obtained from the consumption of apples, and no juiciness is obtained from driving an automobile.

Having defined a Lancaster consumption technology as the m times n matrix of a_{ij}, we almost automatically may define technical change as a change of the value of any of its elements or the addition of new elements to it. Whether or not technical change is also technical advance is harder to say. The replacement of subsonic airplanes by supersonic ones will raise traveling speed, a desirable Lancaster characteristic, but unfortunately also greatly raise the noise level, an undesirable one.

Professor Lancaster claims that in his model "there is scope for technical

and income elasticities for the specific group products where there was high immobility of the labor force involved. That means technological unemployment; or more broadly, a concern with the differential effects of innovation on the subcomponents of the labor force. Whatever the collective interests, it seems clear that workers in these situations were not acting against their own interest in trying to control the innovation rate. In this sense the problem of innovation is age-old and contemporary. Much valuable work has been done and is being done by research into the whole area of redundancy schemes and the general methods of specific type labor adjustment to changing technology. Here the tools of partial analysis offer great insights, are fruitful and conducive to the public good, and a just claim for advance in economic thinking even though we may not have solved the magnificent question of general equilibrium.

In general, the conclusion of this paper is that in investigating the effects of innovation on income distribution we should investigate the effects on distribution within the category labor rather than between labor and capital.

case for it has been argued by Mrs. Robinson. Both conclusions are apparently consistent with the facts, but it is difficult to conceive of a method of statistical testing.[13]

What are we to conclude about the state of the subject matter of this paper?

1. The state of economic knowledge does not enable us to predict the direction of impact of innovation on relative shares.

2. There is a certain presumption that innovations have been roughly neutral in their total effect although this result is derived indirectly by looking at relative shares.

3. The neutrality of innovation seems a most unlikely occurrence if it really were autonomous. Hence we feel that there must be some mechanism regulating the speed of innovation and its direction in a compensatory manner to adjust the relative scarcity of factors.

But I do not wish to end on this note. I want finally to raise two points which I think deserve serious consideration.

Our problem situation has been that of the impact of technological change—whether induced or autonomous—on relative shares between those peculiar lumps called labor and capital. Now leaving on one side for the moment all questions of statistical measurements or the correct relationship between our theoretical notions of profits in a production function and the actual numbers we use in estimation, I want to ask the naïve question, why are we so fixated with the relative shares question? Some years ago Mrs. Robinson admonished the profession for its failure to explain relative constancy, but will that explain our compulsion? Perhaps it is the welfare aspects of the problem? Let me suggest some of these aspects. We could believe (postulate) that the rate of change of our social welfare function is negative with respect to the share of profits. Or rather more narrowly postulate a worker's utility function consisting (say) of two elements: the average real wage and the wage share, so that worker utility falls as the profit share rises. Is this a reasonable account of the world? It may be, but suppose for example that the increased profit share is all attributable to share ownership by workers? Would we still put the same sign on the partial derivative?

I have raised this question of our concern with relative shares because it leads me into my final conclusion. Let us return to those recurrent subversive remarks that I mentioned at the beginning of this paper: recurring fears that technological innovation would be detrimental to the work force. As I mentioned, their basic concern was not with relative shares. The central problem was with the effect of rapid innovation of an absolutely laborsaving nature in industries with low price

[13] Investigations at the micro level of innovation cannot throw much light on overall macro effects. It is these that we are concerned with in relative share discussions.

vations into autonomous and induced.

This latter division may be useful in enabling us to distinguish between two very different but confused situations: (1) the rate of invention; i.e., the rate at which potential innovations are added to the existing list; (2) the rate of utilization of this stock; i.e., the rate of innovation.

It is arguable that we can regard "1" as truly exogenous being determined by "science" and "2" as endogenous being a function of economic variables. The validity of this distinction is a matter for empirical research but there does seem to be strong presumptive evidence against it. Thus to the extent to which research and development expenditures are determined (on the margin) by factors like foreseeable changing factor scarcities, we do need to investigate the invention production function.

Hicks, in his *Theory of Wages,* divided innovations into autonomous and induced and made the persuasive suggestion that the autonomous element could be thought of as neutral (falling manna like an equiprobabilistic manner) whereas induced innovations would assuredly be laborsaving because of the change in the wage-rental ratio. Thus overall the prediction is that innovations are laborsaving. Few would dispute the point about (at least some) innovations being induced. The question is what is the mechanism? Many innovations appear to have been a direct response to what are loosely called technical bottlenecks; these in turn are likely to have been caused by earlier changes that upset the complementarity of the existing productive process. And, of course, the system multiplies—each change being a lagged response to earlier innovations. It is an open question how far technological innovations have been a response to this sort of situation and how far it has been a response to changes in the relative price of labor and capital. The bottleneck to be eased may easily be within the labor group. We tend to think of the relative scarcity of labor and capital, but what of the relative scarcity of, e.g., skilled and unskilled labor? Innovations may be labor neutral but unskilled using and skilled saving, or vice versa.

The trouble with the price induced laborsaving theory is twofold: (1) the statistical difficulties of distinguishing the effects of innovation from the effects of substitution; (2) (more fundamental) why should a rise in the wage-rental ratio induce capital intensive techniques?

More recently Professor Fellner has argued that even without the negative price effect "the character of innovating activity adjusts reasonably well to basic resource positions," which presumably means long-term laborsaving.[12] Induced neutrality is also a possibility and the

[12] W. Fellner, "Two Propositions in the Theory of Induced Innovations," *Econ. J.*, 1961.

sumption, then markup theories have perforce to postulate (and explain) that technological innovation does not alter the weighted average degree of imperfection. This presumes that we can measure market imperfection independently of the profit share (and thus save Kalecki from tautology) and that we can sort out the impact of technology from the total observed results. Plausible reasons come readily to hand to suggest that innovations may raise or lower market imperfections and to suggest that there is an equally strong relationship the other way around.[11]

The implied judgment in this very sketchy survey of relative share theories may now be made explicit. I have argued that the basic models, from Ricardian to Kaldorian, can be made to fit the broad facts of history. But suppose the facts had been different, would we have rejected the models? I think not. These models are really better described as frameworks for handling the relative share problem and only make specific predictions with added restrictions. Thus, what I have called the neoclassical approach does not in general predict the course of relative shares. What it says is that the crucial influences on the time path will be things like the rates of growth of factor supply, degrees of substitutability, differences in factor intensities between sectors, technical progress.

The central problem is of course to devise methods of sorting out the differential effects. Since our frameworks are so open-ended, even without the considerations of technical progress, it is not surprising that when we introduce that factor they remain so. Within the neoclassical framework, if we think that the elasticity of substitution has been one or less than one, then we can say that innovations have been neutral or laborsaving. What we have basically done so far is to look at the record of history and classify technological innovations into particular boxes according to their effects on shares. But, if we hope to predict the effects of innovation, we need more than taxonomy. We require a theory of innovation or more specifically a theory of innovation bias. Nonetheless, at the pretheorizing or classificatory stage, we can make some useful subdivisions of the problem. We can for example differentiate innovations embodied in human capital and those embodied in nonhuman capital. We can classify into process innovations and product innovations. The distinction can perhaps be drawn between (following Schumpeter) the gestation period effect and the realization effect. The question of the introduction of an innovation itself may be distinguished from the spread of its use throughout the system (the diffusion problem). And finally, we may attempt to classify inno-

[11] Space prevents me considering the Marxian model, but similar conclusions would follow; viz., that with technical progress it is not "inevitable" that the rate of profit should fall, etc.

The question is how do we distinguish between these two theories? Let us press the point a little further. Suppose we think that in fact labor's share has risen. We can account for this by postulating neutral innovations and an elasticity of substitution of less than one. Or we may prefer capital-saving innovations and an elasticity of one. Or capital-saving plus an elasticity of more than one. If we felt great confidence in the meaning and statistical significance of fitted macroeconomic production functions, we might make progress,[10] but in the absence of such agreement is it possible to find predictions that differ and the relevant economic conditions when these different predictions would show up? A possibility would occur if we could identify periods of low or near-zero technical innovation and see whether noticeable changes in relative shares do occur in these periods. In the absence of an agreed index of innovation this seems unlikely.

Let me now briefly turn to the Kaldorian approach. Kaldor of course rejects the neoclassical analysis, lock, stock, and barrel. In his survey of relative share models he appears to reject the classical and Marxian models on straight empirical grounds (although he tends to abstract from technical progress). His rejection of neoclassicism is not, as I understand it, on empirical grounds; i.e., that the predictions that are made are false; rather, the rejection is a priori. Be that as it may, in the Kaldor model relative shares depend upon the investment-output ratio and on the factor savings ratios. The investment-output ratio is "determined" by a technical progress function and thus innovation only comes into the relative share problem via its effect on this function. The from of technical progress (i.e., its degree of neutrality) does not appear to effect the issue. The fact that both in the U.K. and the U.S.A. the investment ratio has undergone considerable long-period fluctuations in the face of near constancy of shares would persumably have to be explained via compensating changes in the savings functions although the evidence on this factor is too sketchy to enable us to reach any definite conclusions.

Embodied in Kaldor's macro theory of relative shares there is a micro counterpart. His micro explanation of relative shares (i.e., constancy of the wages share) rests on the assumption of a normal rate of profit towards which the actual rate will tend via changes in the rate of capital accumulation. It clearly has close affinity to the Kalecki degree of monopoly approach, and we can here just say a few words about the markup approach to relative shares and the way in which it can handle the question of innovation. If we stick to the constancy of shares as-

[10] Recent fits of the CES production function seem more reliable statistically than the Cobb-Douglas fits and give elasticities of less than one. See K. J. Arrow, H. B. Chenery, B. S. Minhas, and R. M. Solow, "Capital-Labour Substitution and Economic Efficiencies," *Rev. of Econ. and Statis.*, 1961.

form is used).

This approach has in common with the classical one the central property that movements along a production function are (at least analytically) distinguishable from shifts of that function due to innovation in the system.

But even in the absence of technological change the assumption of constant returns in the production function does not pin us down to any relative share predictions; resources may be growing at differential rates, thus altering relative factor prices and inducing substitution. It is thus useful to specify a form of the function that makes factor shares independent of differential rates of factor supply. Hence, the concentration until recently on the Cobb-Douglas form.[8] In this case, as is well known, the ratio of the marginal to the average product of a factor is constant, which means that the share of that fact is constant.

We now allow for technological innovation, and keep within our stylized fact constraint of constancy of shares. This can be accounted for by postulating that innovations have been neutral, and the Cobb-Douglas underpinning makes consistent the Hicksian and Harrodian definitions of neutrality. Moreover, we also get out as predictions the constant rate of profit and (inevitably) the constancy of the capital-output ratio.[9]

Thus (and keeping in the background of our discussion the empirical validity of our stylized facts) it seems that the Cobb-Douglas assumption of neutral innovations is not a patently false picture of the world. Unfortunately, there are other interpretations within the neoclassical framework that are equally consistent with the facts. We may postulate that for a given state of technology the wages share would rise as capital accumulates at a greater rate than labor (i.e., the elasticity of substitution is less than one), but that this bias is offset by technical progress which is laborsaving. We could prop up this postulate with some theory of induced innovation to "economize" the relatively scarce factor. I shall come to this point in a moment.

[8] Until quite recently the Cobb-Douglas form appeared to be substantiated by statistical fitting. However, the fitted form of this function is now seen to be open to very basic difficulties. See, e.g., H. Phelps Brown, "The Meaning of the Fitted Cobb-Douglas Function," *Q.J.E.*, Vol. 71.

[9] The assumption of an elasticity of substitution of unity and neutral technical progress, unambiguous though it appears, becomes very cloudy once we introduce (1) more than two factors and (2) more than one sector. The first problem is that of deciding what factors are held constant as we measure the elasticity in a pairwise manner (on this see J. E. Meade, *A Neo-Classical Theory of Economic Growth*, Appendix 1). The second question is more fundamental. In the absence of technical progress a shift in demand between sections will alter distribution even if both sectors were Cobb-Douglas but had different components on, say, the quantity of labor. With technical progress, neutrality must mean that the balance between sectors is not disturbed. Thus neutral innovations that fall solely in the consumption goods sector will have different results to innovations that fall solely on the capital goods sector.

The stationary solution of this model is where the rate of profit falls to the rate where net accumulation is zero. This is the classical stationary state and the time paths of the variables that we are concerned with are predicted to be: the rate of profit falls, the share of wage rises, the share of profit falls, and the share of rent rises. (We should note that strictly speaking the last two assumptions do not follow from the assumptions of the model. The rising rent share and falling profit share require that the marginal product curve of labor-capital doses be falling at a constant or increasing rate.)[5]

At this level it seems clear that the predictions of the model patently conflict with the agreed facts of economic history. The stationary state does not seem to be around the corner nor has it in fact ever been. But of course these predictions are *ceteris paribus,* which in this case means "in the absence of technical progress." We now superimpose technological innovation (without explaining the whys and wherefores of the mechanism). What predictions do we now get about our key variables? The answer will now be of the form "it all depends." And crucially it will depend upon things like (1) how rapidly innovation pushes upwards the average and marginal productivity curves to offset the tendency to diminishing returns; (2) whether the innovations are labor-capital saving or land saving; (3) what assumptions we make about the labor supply adjustment function; i.e., whether real wages rise above subsistence in response to an excess demand for labor.

The classical approach, then, I would argue is compatible with the failure of the secular rate of profit to fall noticably, with the near constant secular wage share, etc. Now you may say that this "it all depends" view of the classical theory is really farfetched and is not in the spirit of the classical writers themselves.[6] Let us then turn to the neoclassical approach.

Neoclassical models are, as their designation suggests, an extension of the classical analysis. The key respects in which they differ are: (1) They use a generalized marginal productivity approach as an explanation of all factor rewards. The capital-labor division replaces the threefold classical one at the functional share level of analysis. (2) Continuous substitution is assumed between all factors.[7] (3) The assumption of diminishing returns to scale tends to be replaced by the assumption of constant returns to scale (often a specific Cobb-Douglas

[5] On this point, see H. Barkai, "Ricardo on Factor Prices and Income Distribution in a Growing Economy," *Economica,* 1959.

[6] How far the classical writers really did take seriously the approach to a stationary state is an interesting question. My view would be that the stagnationists were not on the whole to be found in the orthodox camp.

[7] Even if we abandon the assumption of substitution within a given activity, the assumption of many activities with different factor intensities keeps substitution in the model.

may not enable us to distinguish between competing theories that make the same (correct) sets of predictions.

The agreed facts relevant for relative share theory (I state this rather baldly in the face of mounting counterempirical evidence) are, or were until recently: (1) the secular constancy (with a subtle prolabor drift) of the wages share, (2) the secular constancy of capital output ratios, (3) the secular constancy of the rate of profit, (4) a secular increase in capital-labor ratios, (5) a cyclical swing in shares—prolabor in downswings and vice versa.

I shall not at this stage risk your disapprobation and question the fact of these facts (although I think that at least "1" and "3" should be given up).

What I now want to do is to look at some basic theories of relative share determination and see what predictions they make about the effects of technological innovation on shares. We can then see whether the particular predictions are conformable to the facts and if this is true of more than one model whether they ever make different predictions that do enable us to distinguish their competing claims. My list of theories to be considered is completely nonexhaustive and for this I must apologize for I think it does grave injustice to many important contributions. However, my point is to illustrate a theme. So far I have avoided any attempt to come to grips with the concept of "technological innovation" and I shall continue this avoidance in this section of the paper. But as a partial explanation I regard innovation as anything that raises the ratio

$$\frac{\text{weighted index of output}}{\text{weighted index of input}}$$

Due allowance being made for (1) nonconstant returns production functions and (2) the disentangling of embodied innovations.[4]

I shall take three basic theories (or, as I would rather call them, frameworks for handling the relative share problem). Following precedent we shall designate these classical, neoclassical, and Kaldorian.

A classical approach has as its main unifying features: (1) A threefold division of total product between land, labor, and capital. Fixed proportions are assumed between labor and capital (so that marginal productivity theory cannot be used to disentangle their rewards) and substitution only takes place between land and "doses" of labor-capital. (2) The production function is assumed to exhibit decreasing returns to scale.

[4] This is a very broad use of the term innovation and follows the Schumpeterian view. Some of you may prefer to think of technological innovations as those embodied in nonhuman capital. But is not operational research a technological innovation and is it not (basically) embodied in human capital?

wages share; i.e., that it would fail to rise or even fall. The fears were either about what we would now term "structural unemployment" or the failure of real wages to rise over time. Neither of these predictions has any necessary connection with hypotheses concerning relative shares. I shall leave this point on one side for the moment but will return to it later.

A very simple way of looking at our problem is to present the relative share issue in terms of two ratios. We have them quite simply:

$$\frac{W}{P} = \frac{w}{r} \cdot \frac{L}{K}$$

where L is the quantity of labor, w the average wage, K the quantity of capital, r its average return, W the wages bill, and P total profits. This way of putting things highlights two important aspects of the problem: (1) since price and quantity are inversely related we would expect a certain stability in the W/P ratio (although not of course constancy) and (2) each one of our variables is really a very slippery index number. For example, the capital stock is a heterogeneous collection of different inputs, with different ages, different technological efficiencies, different costs, different lengths of life, different designs and doing different jobs. Much the same sort of classificatory schema can be, and is increasingly being, used to describe the notion of a labor force (although the retention of the homogeneity assumption lingers longer here).

When we contemplate the possible influences on the four variables in our equation above, e.g., rates of growth of factors, demand conditions, international trade, and on top of all this the process of technological innovation, we rapidly see the obvious: that this is *par excellence* a general equilibrium problem. In the face of this indubitable fact economists have been forced (as always) to simplify. If you like we can think of this as variable saving innovation. We have produced highly aggregative models which attempt to pinpoint certain key relationships that "determine" the pattern of relative shares over time. It is no criticism of these models that they are oversimplified; ultimately they must be judged in terms of their empirical validity.

In an effort to limit the possible theoretical permutations that can be dreamed up, resort has been made to what I shall call (following Kaldor) the constraint of the stylized fact. Let me say something about this device. This constraint is of course a commendable one and has much to be said for it on methodological grounds. It constrains our models to square with the real world and (potentially) enables us to eliminate theories that make clearly false predictions. The dangers of its use lie in the possibility that the stylized facts are fiction and that it

THE ROLE OF TECHNOLOGICAL INNOVATION IN THEORIES OF INCOME DISTRIBUTION

By Bernard A. Corry
London School of Economics

Expressions to the effect that technological innovations were not an unmixed blessing to the labor force (or at least to some of its components) have been present as an undercurrent in economic thought since at least the classical period. This paper is an attempt to survey one aspect of that broad question; namely, the effects of innovation on relative shares. The literature on this subject is now enormous, and in recent years has had at least a constant rate of innovation if not of improvement. I cannot possibly encompass it all, not to say do it justice, in the limited time available, nor would I be presumptuous enough to think that this is possible in a less finite horizon. Luckily the basic components of the question have their own admirable surveys and I refer the inquirer to them.[1]

As the title of this paper suggests, I shall attempt to take an overall look at our theories of functional distribution and see whether it is possible to graft on to them our theories (if any) about technological innovation. Functional distribution has always, of course, been of central concern to economists, whereas theories of innovation (as distinct from the effects of innovation) are a more recent area of speculation. The general progress of economic thinking about the effects of innovation has been one of unguarded optimism, innovation being regarded as the countervailing force to the "niggardliness of nature." Yet throughout the development of economic analysis there has been the undercurrent of dissent. Thus doubts as to the benefits to the working class of the introduction of machinery pitched Ricardo[2] into short-run analysis for about the only time in his life. They induced Wicksell[3] to make some oblique remarks to the effect that accumulation was the worker's friend and innovation his enemy, and more recently these fears were formalized in Lange's *Price Flexibility and Employment*.

Now when we look at these recurrent warnings it seems clear that, Wicksell apart, the concern was not basically with the fate of the

[1] See in particular: F. H. Hahn and R. C. O. Matthews, "The Theory of Economic Growth—A Survey," *Econ. J.*, 1964; R. R. Nelson, "The Economics of Invention—A Survey of the Literature," *J. of Bus.*, 1959; M. Blaug, "A Survey of the Theory of Process Innovation," *Economica*, 1963; T. Scitovsky, "A Survey of Some Theories of Income Distribution" in *The Behaviour of Income Shares* (N.B.E.R.).

[2] D. Richardo, *Principles of Political Economy*, Chaps. 2, p. 31.

[3] K. Wicksell, *Lectures on Political Economy*, Vol. I, pp. 163–64.

way to a theory of market-induced innovational factor-saving effects. We are, I believe, living in an era of accelerated innovation, and such an era could become one of increasingly pronounced difficulties as a result of the overshooting of the labor-saving or of the capital-saving effect. But despite Ricardo's chapter "On Machinery" and Marx's *Kapital* (on the labor-saving side), and despite some of the stagnationist convictions expressed during the 1930's (on the capital-saving side), I believe that in this regard economic theory supports optimistic views much more nearly than pessimistic ones.

On the other hand, technical-economic analysis can throw little light on the general cultural consequences of very rapid technological-organizational change. Are loyalties to a social and economic system and to a type of civilization ultimately not loyalties to a way of life? What may be expected to happen to these loyalties and to the spirit of dedication in a world in which it takes no more than two to three decades' worth of innovating activity to change the way of life of Western countries quite radically?

Fortunately, one need not pretend to have answers to all questions that are worth asking.

augmentation, firms will select innovations which put them at a point of the frontier where the slope equals the marginal advantage of labor-augmentation relative to capital-augmentation, and this marginal advantage is equal to the ratio of the labor-share to the capital-share in factor cost (hence also in income). The assumed concavity to the origin of the innovation possibility frontier assures that the firms will gradually move to a point where the distribution of income stays unchanged; that is, where the innovations are just enough labor-saving to offset the labor-share-increasing effect of the rising physical capital-labor ratio. Until the time when this point of the frontier is reached, the distributive shares will be changing; thus the marginal advantage of labor-augmentation relative to capital-augmentation will also be changing, and the firms will be moving along the frontier toward distributive-shares equilibrium.

To me these new results seem rather impressive. We have here an attempt, not merely to explain an innovational tendency to counteract diminishing returns by an induced bias, but to explain also the observed stability (or near stability) of distributive shares. Yet it must of course be realized that the results require assumptions about which an attitude of "wait and see" is justified until further evidence becomes available. The assumptions include dynamic production functions that, while staying unchanged, incorporate all the innovating activity of our economies by means of "factor-augmentation"; and the assumptions include less than unitary elasticity of substitution as a requirement of stable distributive-shares equilibrium; the assumptions include also production functions which permit changes in distributive shares within a sufficiently wide range, and they include innovation possibility frontiers along which sufficiently large labor-augmentation rates are shown as possible in view of the actual increase in the capital-labor ratio; last but not least, it is assumed that the innovation possibility frontier is everywhere concave to the origin and is continuous (i.e., all innovations shown in Figure 1 are available at the cost there implied), and that the marginal trade-off rates stay unchanged over time. In spite of my optimism concerning these recent developments in the theory of innovation, I would consider it premature as yet to lose sight of the weaker propositions which relate merely to a counteracting tendency generated by innovational biases in economies in which labor is getting scarce relative to capital. These weaker propositions—which are not equilibrium theorems but are concerned merely with the existence of tendencies in an offsetting direction—require of course fewer assumptions.

At any rate, taking account of the stronger as well as of the weaker analytical results, it seems reasonable to suggest that we are on our

significance. My own reaction is that his results are of considerable significance, though I agree that a number of *caveats* need to be added.

The strong proposition, or theorem, in question is derived in the following way. Innovations are assumed to be of the factor-augmenting variety; that is to say, post-innovation conditions can all along be represented with the aid of an unchanging dynamic production function, which in this case is a linear homogeneous function for two inputs. Persistent innovating activity expresses itself in the fact that in order to obtain the correct output we need to "feed" into the production function not merely the actual physical labor input, but in addition also a continuously growing percentage of the physical labor input (so that we feed into the function a labor input which results from an innovational augmentation of the physical input); and/or we need to feed into the production function not merely the actual physical capital input but an augmented one.[3] The fact that in reality the physical composition of the capital stock does not remain constant causes a well-known difficulty, but here I will not take up a discussion of the question on what assumptions various statistical makeshifts for measuring changes in the size of physical inputs are more or less acceptable. The theorem assumes the availability of methods for measuring physical changes—an assumption which generally underlies the concept of the production function—and the theorem assumes that the elasticity of substitution along the production functions is less than unity, in which case primarily labor-augmenting innovations are labor-saving, and primarily capital-augmenting innovations are capital-saving. Innovating entrepreneurs are represented as being faced with an innovation possibility frontier, or boundary of a possibility set. The frontier implies a given amount of cost or effort used for acquiring innovation possibilities.[4] The abscissa of any point on this possibility frontier shows a possible (feasible) proportionate labor-augmentation rate per period and the ordinate shows the highest-possible capital-augmentation rate that can be achieved in conjunction with that labor-augmentation. The numerical value of the slope of the innovation possibility frontier expresses at any point a marginal trade-off rate; i.e., the marginal opportunity cost of additional labor-augmentation in terms of the capital-augmentation that must be sacrificed to obtain a small additional unit of labor-augmentation. The slope of the frontier thus being the marginal opportunity cost of labor-augmentation in terms of capital-

[3] However, outside the first quadrant one of the two augmentation rates is negative. Samuelson's graphs do not extend the curve beyond the first quadrant. Generally speaking, the dynamic production function here used may be written as $Q(t) = F[L(t)g(t), K(t)h(t)]$, where the time-rate of change of g and h express the augmentation in each period.

[4] It may be added here that the private optimum effort is not the same thing as the social optimum effort since the external effects involved in the acquisition of new knowledge are considerable.

The theorem recently proposed states that in economies with a rising capital-labor ratio in which—assuming a less than unitary elasticity of substitution—labor's relative share tends to rise along with the wage-rental ratio, innovations tend to become just enough biased toward the labor-saving effect to suppress the rise in the share of labor. In other words, the induced bias of the innovations will offset just enough of the rise in the wage-rental ratio to keep the distribution of income unchanged. If the capital-labor ratio were declining (rather than rising), then an induced capital-saving bias would stabilize the income-distribution; if the capital-labor ratio stayed unchanged, innovations would be neutral. But for Western economies the interesting case is that of a rising capital-labor ratio, a case for which my weak but, I think, secure proposition led merely to the presumption that the rise in the wage-rental ratio would to some extent be counteracted by a labor-saving bias, while the strong proposition leads to the presumption that the labor-saving bias will suppress precisely that part of the rise in the wage-rental ratio which needs to be suppressed to keep the relative shares in income unchanged.

Samuelson is not at all assertive about the realism of the assumptions on which the foregoing strong proposition is valid though he obviously feels that analysis based on these assumptions is of potential
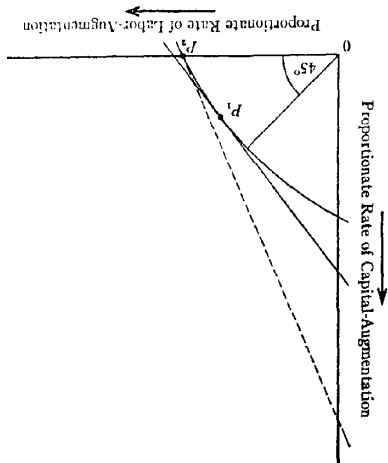
FIGURE 1

The numerical values of the slopes of the two straight lines express the ratio of labor's to capital's share in income, for different income distribtions. The two tangencies with the curvilinear innovation possibility frontier are marked P_1 and P_2. The innovations are Hicks-labor-saving in both points. They are Harrod-neutral in P_2.

wage rates relative to rentals will make the more labor-saving of the two innovations more profitable (and vice versa for a rise in rentals relative to real wage rates), so that an expected rise in real wage rates relative to rentals tilts innovations more toward the labor-saving effect. It can be established rather securely also that if the rate at which some factors are getting macroeconomically scarce relative to other factors is such that the scarcer factors cease to be in infinitely elastic supply to the individual firm, then such unavailability to the firm of factor-units of unchanging quality at the going factor-price tilts innovations toward saving the factors which are getting macroeconomically scarce. Propositions such as these belong in a class which no doubt contains more members than the two I have suggested. In a particular sense, propositions of this sort are admittedly "weak," because while they can indeed be derived from assumptions that are fairly generally acceptable, they merely point to the existence of offsetting tendencies which reduce the probability of historically diminishing returns. These propositions do not establish a tendency toward equilibrium levels of variables or of rates of change, though even these weak propositions suggest that failure of the distributuion of the factor-saving effects to adjust to relative factor costs keeps the rate of innovation below the maximum attainable (and does so on grounds of productivity considerations, even aside from the oversaving difficulties which may arise from an excessively capital-saving character and the Marxian social-political difficulties which may develop from an excessively labor-saving character).

Yet, recently stronger propositions have been developed. These point not only in the same direction as the weaker ones, but, on specified assumptions, they point to definite equilibrium positions which are located in that direction. I would like to call attention particularly to Paul Samuelson's contribution to the subject in the November, 1965, issue of the *Review of Economics and Statistics*. Among the immediate antecedents of Samuelson's article, the contributions of the German economist Christian von Weizsäcker and those of the English economist Charles Kennedy are noteworthy, and it should be added that the Yale economists Emmanuel Drandakis and Edmund Phelps presented an interesting paper on the same subject at a session of the September, 1965, meetings of the Econometric Society in Rome.

Samuelson discusses thoroughly the assumptions on which it is possible to deduce a theorem relating to distributive-shares equilibrium with innovations whose character adjusts in such a way as to assure this equilibrium. I will turn to these assumptions (or conditions) subsequently, and will begin by briefly indicating what sort of equilibrium this is; i.e., what the nature is of the strong propositions concerning an innovational adjustment process.

ods of short duration. On the whole, the record of the West bears out the contention that in economies with a significantly rising capital-labor ratio innovations have offset the tendency toward diminishing returns to investment, and for the rest they have contributed to the observable significant rise in real wage rates. Even in the past, the distribution of the factor-saving effects of innovations have tended to satisfy certain desirable conditions, and one is tempted to conjecture that this has not been entirely the result of chance. Indeed, this is precisely what Sir John Hicks has conjectured in 1932 in his pioneering work on the subject.

Following Hicks, we shall call an innovation labor-saving, capital-saving, or neutral, according as it decreases, increases, or leaves unchanged the relative share of labor when the innovation is macroeconomically adopted for the capital-labor ratio that is given to the economy as a whole. We should, however, stay aware of the fact that the corresponding microeconomic definition, which implies given factor prices, makes the direction of change of the individual firm's capital-labor ratio the relevant criterion for the first impact of a labor-saving or a capital-saving innovation.

Any hypothesis concerning an adjustment mechanism that directs the character of innovations—any theory of the induced bias—implies that firms may choose from among alternative innovation possibilities and this in turn implies that the preference of firms for engaging in more labor-saving or in more capital-saving innovation tends to call forth inventions that enable them to engage in innovation of a more labor-saving or of a more capital-saving kind. These implications seem acceptable, but the question remains to be answered as to the circumstances in which firms are apt to develop a preference for the one or for the other kind of innovation possibility. On assumptions that are not severely restrictive it is possible to justify statements which point to a preference for innovations of a more labor-saving kind when the wage-rental ratio shows a rising trend, and for innovations of a more capital-saving kind when the wage-rental ratio shows a falling trend. To illustrate what I have in mind, I will repeat briefly the content of two propositions which I presented in an earlier note.[2] I believe that the argument supporting these statements is strengthened by the increasingly systematic exploration of innovation possibilities; that is, by the same process which also tends to increase the rate of innovation.

In particular, it can be established rather securely that if at present factor prices two innovations are equally profitable, then a rise in real

[2] *Econ. J.*, June, 1961, pp. 305-308. This note elaborates on propositions formulated in my *Trends and Cycles in Economic Activity* (Holt and Rinehart, 1956).

judgment on a specific innovation project, without knowing what he may do next in the case of its miscarriage, then his risks are appreciably greater than if he stakes his fortunes on the average outcome of a group of projects all of which have been worked out sufficiently well to carry high return-expectations. The dispersion around expected returns is inevitably high for the individual projects belonging in the group but is much lower for the group of projects as a whole, unless the covariance is great. More systematic exploration of innovation possibilities is likely to make technological-organization progress more rapid and more continuous (less "jerky").

The foregoing remarks relate directly to the rate of innovation—to its product-raising effect—rather than to the distribution of its factor-saving effects. In the recent statistical materials of some countries the positive effect of the more systematic research and development effort on the rate of innovation may have become partly covered up by the adverse consequences of a combination of fixed exchange rates with autonomously set money wage rates. But this is a different question, which I will not discuss here.

II. *The Question of the Induced Bias*

What needs to be added now is that a high rate of innovation could not continue for very long if it became associated with a sufficiently pronounced maldistribution of the factor-saving effects. Given the downward rigidity of money-wage rates, overshooting in the capital-saving direction could bring the maladjustments of which the stagnationists of the 1930's were afraid. Overshooting in the labor-saving direction could worsen the trend in real wage rates appreciably, or, in the event of an institutional floor to real wages, it could bring the kind of unemployment which nowadays is usually referred to as "non-Keynesian" (i.e., the kind of maladjustment which Ricardo feared in his more pessimistic moods and which Marx predicted with great assurance). Periods of intense technological change could become periods of significant overshooting in one of these two directions, and such overshooting would interfere with continued economic growth in the Western institutional framework. However, I do not believe that this is likely at all to happen, because it seems to me that the same changes which result in accelerated innovating activity are apt also to direct this activity into channels consistent with the macroeconomic resource positions of Western economies.

In this regard, even the past record is reasonably good. With the possible exception of early stages of economic development, adverse consequences on the demand for labor or capital have stayed limited to immobile factor-units with distinctive characteristics and/or to peri-

Turning now to the problem of technological-organizational innovation, we may state that innovating activity increases the dispersion around the expected value of monetary returns. Perhaps the main reason why it has proved rewarding to draw a distinction between moving along given production functions and moving onto new production functions is that the results of moves of the former kind are more predictable than the results of moves of the latter kind. It is true that recently the distinction between given production functions and new ones has become somewhat blurred. Considerations of analytical convenience have led many investigators to using dynamic production functions which (though they are viewed as staying "given") incorporate the results of innovating activity; that is, incorporate the results of what would be regarded as shifts of statically formulated production functions. But even in the analysis of the limited range of problems which can be fitted into dynamic production functions of unchanging form, we should remember that decision-makers will consider the output results more uncertain—they will consider the dispersion around expected values greater—if the parameters expressing the dynamic character of the function are assigned high values than if the function is more nearly reminiscent of a static production function. The further one gets away from the circumstances with which one has had experience, the more diffuse do one's subjective probability distributions become—i.e., the wider does the range of values become in which all subranges of equal width have similar probabilities—and hence the more unattractive does, other things equal, the situation become for a risk-averter. The decision to adopt an innovation therefore typically rests on the judgment that the expected returns of the innovated technology are high enough, as compared with those of the old technology, to justify the move in spite of the higher dispersion around the expected post-innovation values.

Increasingly methodical concern with innovation possibilities in Western societies is likely to lead to the discovery of a larger number of projects which will carry sufficient return-expectations to overcome the reluctance of decision-makers to face the dispersion. But this is not the only point deserving emphasis in the present context, and it is perhaps not even the main point. The main point, I think, is that increasingly methodical concern with innovation possibilities is almost certain to increase the number of innovation projects which at any time are within the horizon of each important decision-maker, with the result that each decision-maker gets to be a more diversified innovator and hence a more willing innovator than he used to be. It is well known that diversification reduces dispersion and hence increases the willingness of risk-averters to take action. If a decision-maker needs to pass

PROFIT MAXIMIZATION, UTILITY MAXIMIZATION, AND THE RATE AND DIRECTION OF INNOVATION

By WILLIAM FELLNER
Yale University

I. *The Rate of Innovation*

By the rate (or intensity or "quantity") of innovation I will mean here its product-raising effect, given the factor inputs. I will distinguish the rate of innovation from the distribution of its factor-saving effects by labor-saving and capital-saving. The first section of my paper will be focused on the influence which the increasingly systematic exploration of innovation possibilities—a phenomenon reflecting itself partly in the rapid postwar rise in research and development expenditures—may be expected to exert on the rate of innovation. Yet it will be argued in this paper that any satisfactory discussion of this problem must lead into the question whether the distribution of the factor-saving effects of innovation is likely to meet the needs of our economies reasonably well.

The decision-makers in our firms will be thought of as individuals who are attracted by the prospect of monetary returns—"the higher, the better," from the firm's point of view—but who, in most of their significant decisions, are repelled by dispersion around expected values. Such individuals can usually be interpreted as maximizers of the sum of weighted utilities of the potential monetary payoffs; the weights they assign to the utilities of the potential payoffs may be genuine (undistorted) probabilities—i.e., these weights may satisfy the rules of probability theory—but it is not necessary to assume that the relationship between the weights and probabilities is that of identity.[1] While our decision-makers are maximizers of a sum of weighted utilities rather than of a sum of weighted money values of potential payoffs, they should nevertheless be described as individuals guided by the pure profit incentive. They act in awareness of the fact that when the results of their acts will become known their satisfaction will prove to be a monotonically increasing function of their monetary returns. This remains true even though they are aware also of the fact that $2,000 would give them a degree of satisfaction which is not equal to twice the satisfaction yielded by $1,000.

[1] However, to make utility an operational concept we need to assume that the decision weights are probabilities proper in specific risky processes (say of the coin-tossing or card-drawing variety). Elsewhere I called these standard processes.

sumption technology to include the activities associated with the consumer's sale of labor or other resources. Since labor as an activity may have some characteristics associated with it that are shared by goods, the particular work a consumer performs may partly determine his choice of goods. A taxi driver may spend less of his budget on taking weekend drives than the social norm; yet traditional theory would find no connection between his consumption and his occupation.

New occupations and even new work conditions can be considered as changes in consumption technology. They may also lead to changes in production technology, but this is not necessarily the case.

It would be possible to follow through the kind of analysis we have been making here at very much greater length than is available, but I think the point has been made. There is a technology of consumption. It is the subject of continual change and innovation, just as is the production technology. This change does lead to increased welfare, but the direction from which change comes, the incentives for change, and the analysis and measurement of change differ considerably between production and consumption.

teristics by taking half a Cadillac and half a Volkswagen, so that, although the points A and B are on the frontier, points on AB are not. Then a variant priced to give point C'' might be preferred by some consumers to either A or B, and the convexity of the price-characteristics relationship is not a necessary condition for marketability in this case.

New goods and differentiated products may not simply add to the spectrum of consumption activities; they may replace previous goods. This replacement will occur when the characteristics and price properties of the new product push the frontier forward in such a way that some existing good is no longer part of the efficient set. This will, of course, happen if the new good, for the same outlay, gives more of all characteristics in approximately the same proportions as the old. Such a change seems to correspond to what is often meant by an "overall improvement in quality." In other cases a quality improvement may correspond rather to an increase in some characteristics, with the others unchanged.

Although the introduction of a new product or a new variant can be expected to increase welfare in the simple Paretian sense if the new product is actually purchased and if the existing product is still available at the old price, this may not be the case if the seller takes the old product off the market as he puts the new one on. If the new product, however much of some characteristics it may offer per dollar of outlay, offers less of some other characteristic than the old, then some consumer may be deprived of part of the efficient technology relevant to his particular tastes.

The distinction between the technology of production and that of consumption is a great convenience in analysis but is not based on an absolute criterion of any kind. The ultimate constraints on the system are resources; the ultimate products are characteristics. Some resources may be used to first produce goods which are all intermediate goods in the final analysis, and these goods may then be used in the consumption technology to produce characteristics. But some resources may directly enter the consumption technology without the production of goods as intermediates. As the technologies of both production and consumption change, activities may move back and forth between the consumption and production sectors. This is particularly true of the service and distribution phases of production.

Ultimately the supply of resources, particularly labor, is determined by characteristics. A particular job will have associated with it several characteristics: some will be, in relation to characteristics derived from goods, of a negative kind, but some may well be of a positive kind. The traditional idea of "nonmonetary advantages" has been an attempt to face this obvious fact. We can expand the idea of the con-

It may not always be clear whether we should classify a new good as ın innovation on the production or the consumption side, but it cer-:ainly seems most useful to regard a variant of an exist-product, in-/olving no fundamental change in the technical nature of the produc-:ion process, as an innovation in consumption technology. In terms of ɔur model of consumption, the difference between a new product and a ɔroduct variant is only the degree to which the characteristics mix of .he new product differs from that of existing products. We have, in this nodel, a satisfactory technique for analyzing product differentiation.

Consider a simple model with two characteristics, derivable in lifferent proportions from two goods. We can use the same diagram as ɔefore and suppose A, B to represent the two goods. If the goods are livisible and can be used in combination, the attainable characteristics :ollections for a consumer, given the budget constraint, correspond to .he line AB. The introduction of a third good, C, whose characteristics /ector lies between those of A and B can be regarded as a product /ariant, and this good will sell if its price is low enough to bring the :haracteristics vector to point C' or beyond in the diagram. Given .his product variant, further variants lying between A and C, and C and B would, if suitably priced, expand the efficiency frontier ɩnd therefore be sold. If the relationships between the technical prop-:rties of the product variants and their relative prices is such as to ;ive a convex frontier with every variant represented by a corner of he frontier, then all variants will be in demand, provided consumers ›references are well distributed.

If we consider the situation from the production end and look hrough the consumption technology, we see that a producer is ulti-nately selling characteristics collections rather than goods. The de-;ree of product differentiation will depend on the possibilities, at the ›roduction end, of producing variants with characteristics, and at ›rices, that gives a convex frontier.

A producer with some monopoly power (and we might note that he theory of product differentiation presented here does not require mperfect competition as a prerequisite) will seek the profit maxi-ıizing price and differentiation policy. A theory of imperfectly com-›etitive behavior can be built up by pursuing the above analysis, but t is not proposed to do this here.

If products cannot be utilized in combination, the analysis of prod-.ct differentiation is somewhat different. Consider a highly simplified ıodel of automobiles as consumption activities, expressed in terms of wo characteristics, transportation per dollar of gas and comfort. Let wo variants, Cadillacs and Volkswagens, be represented by A and B in he diagram. Now one cannot obtain a combination of these charac-

so that a consumer suddenly thrust from a wage income to welfare payments may take some time to discover efficient methods at the new income level, although at this level efficiency may be crucial.

One suspects that there may be great scope for increasing consumption efficiency in the kind of changing situations outlined above. These include the transition from peasant to market economies and from rural to urban societies in developing countries and, within countries, among social groups migrating from one region to another or from one income level to another.

Because the market system does not place pressure on consumers to be efficient, this aspect of technical progress has been stressed more than it might be in discussing production. But innovation in the true sense occurs in the consumption technology, and this takes place primarily through the introduction of new goods or new variants and product differentiation.

Traditional consumer theory is at its most unenlightening when confronted by the problem of new goods. Introduction of a new goods requires either that the preference function defined on n goods is thrown away, and with it all the knowledge of behavior based on it, and replaced by a brand new function defined on $n + 1$ goods, or the fiction that the consumer has a potential preference function for all goods present and future and that a new good can be treated as the fall in that good's price from infinity to its market level. Neither approach gets us very far.

In the present model, it may be that the good is so revolutionary that its characteristics are not possessed by any existing goods. We are no better off, in this case, than in the traditional one. But most new goods can be regarded as simply giving rise to existing characteristics in new proportions, and we have available an operationally meaningful way of approaching the problem. A new good of this kind—and this probably covers nearly all new goods and certainly all product variants—adds a new activity to the technology and is, in the proper sense of the word, an innovation in that technology Whether the innovation is efficient depends entirely on the price of the new product. If the price is too high, its characteristics correspond to a point within the efficiency frontier and it will not be purchased by efficient consumers, except perhaps initial experimentation to discover whether it is efficient or not. If the price is sufficiently low, however, the new good will push part of the efficiency frontier forward and will enter the efficient technology. Unless that particular part of the frontier happens to contain no consumer's preferred characteristic collection, the new good will sell. Furthermore, the introduction of a successful new good will result in an increase in welfare, if other prices are unchanged.

ing, designed to increase knowledge of the available consumption technology.

We can use our model to demolish the old argument, favored by sellers of established products, that, since consumers "reveal" their preference for the product already, labeling laws are unnecessary. Traditional theory may seem to lend some weight to this argument, but the present theory does not, since actual choice by consumers can no longer be regarded as revealing their preferences for characteristics—they may merely be making an inefficient choice.

The consumption technology, in a society like that of the United States, is very complex. Efficient consumption, even in the presence of adequate information concerning the technology, involves some managerial skill. As any social worker will testify, many households are noticeably deficient in this skill. Conventional consumer theory leads to a presumption that the family which spends its income on an eccentric collection of goods is simply revealing its preferences for that collection. Of course, this might be true, but it may also be that the family is consuming inefficiently. If the consumer's desired characteristics collection could be ascertained even in a very general way, some type of advising might lead to more efficient consumption.

A crucial difference between the production and consumption sectors is that the market mechanism does not tend to guarantee efficiency in consumption in the same way it does in production. In a society at subsistence level, the inefficient consumer may not survive. In a more affluent society he will survive, but will remain at a lower welfare level than that potentially available to him. Again, this leads to the presumption that public consumer education would be socially valuable.

A relatively static technology, in consumption as in production, will, if coupled with stable relative prices, probably lead to a situation in which the efficient activities become generally known and traditional. Traditional consumption patterns will be efficient only within a relatively unchanging choice situation and only optimal for consumers whose preferences on characteristics approximate the society mode. Tradition will be less useful when the technology is changing rapidly, when relative prices are changing considerably, or when the consumer's preferences diverge from the mode. Furthermore, the typical consumer will inherit his traditions from his social background, and they may not serve him at a radically different income level. We are all aware that the *nouveau riche* may consume differently from persons already established in the higher income group. This analysis suggests that it is at least possible that the desired characteristics of the new and old rich need not be different: the newcomers may be less efficient in achieving their aims. The same considerations may work in reverse;

In the case of production technology, considered in activity analysis form, changes in that technology can be regarded in one or more of the following ways: (1) "Magic wand" effects, in which a particular input combination that gave a certain output in 1965 gives a greater output in 1966. (2) Shifts from actual capabilities, or the upgrading in efficiency of those firms whose productivity is below the known technological potential. Strictly speaking, this is not a change in technology but it will manifest itself in aggregate data in a similar way. (3) An identified technical change arising from the introduction of specified new activities. (4) A change in the nature of inputs such as the introduction of new capital goods, new labor or management skills.

In analyzing production technology, output can be measured with relative ease, as can the input of broadly defined factors. This places much emphasis on magic wand effects, such as unexplained residuals. On the other hand, information concerning the detailed nature of inputs is more difficult to discover, so that the effects of changes in the nature of inputs are less emphasized. In consumption technology the situation is reversed; we have information concerning the changes in the goods which form the inputs, but little information concerning the outputs. We have no interest, therefore, in magic wand effects, but the other three effects can be important.

Since our model of consumer behavior provides scope for efficient choice and hence for the possibility that not all consumers are efficient, there is scope for technical progress in the special sense of increased consumption efficiency, even with no change in the nature of goods or consumption activities.

In consumption, as in production, the prime reasons for inefficient use of the existing technology are ignorance and lack of managerial skill. The consumer may not be aware that a certain good possesses certain characteristics or that certain goods may be used in a particular combination to give a specified bundle of characteristics. Producers or sellers may use advertising to ensure that no characteristics of their product regarded as particularly desirable should go unnoticed by consumers. They will go to less pains to ensure that consumers are aware of some other characteristics of their product.

Organization such as the Consumers Union exist to provide more objective information on the characteristics of goods than is easily available elsewhere. Some consumers are willing to pay for information which assists in attaining efficient points on their characteristics possibility sets and, on the model presented here, are rational to do so. However, since efficient choices are the same for all consumers, there is a clear argument in favor of public information on these matters and in favor of legal requirements, such as composition and contents label-

budget is spent on goods A, B, respectively. By spending the whole budget on combinations of A and B, characteristics vectors represented by points along the line AB can be attained. Now consider the third good, C, which gives rise to the characteristics in proportions represented by the line OC^*. The price of C will determine how far out alonge OC^* the consumer can get by spending all his income on C. If this price is low enough, this point might be represented by C. All the attainable bundles of characteristics for the given price-income situation are given by the points A, B, C and their linear combinations, which are the points in and on the triangle ABC.

The consumer chooses his preferred characteristics bundle from the attainable set. Note that efficiency considerations arise—a radical departure from traditional theory—since, for any bundle of characteristics attainable by combinations of A and B, a larger bundle with the same proportions can be attained by C or by combinations of A and C or B and C. An efficient consumer will choose combinations on ACB, the efficiency frontier for characteristics. Just which point he chooses will depend entirely on his preferences. If consumers have well-distributed preferences and are efficient, we can expect to find that all three goods are sold, but that no single consumer consumes both A and B.

In this model, the consumer faces a double choice. He makes an efficiency choice in rejecting goods combinations which do not enable him to reach the efficiency frontier and a private choice in finding his preferred point on the frontier. If the markets are competitive so that all consumers face the same prices, and given the linearity of the consumption activities, the shape of the efficiency frontier is the same for all individuals. Income differences appear only as scalar enlargements or reductions of the typical frontier. Thus efficient choice is objective and common to all individuals in a given price situation.

The efficiency frontier changes with relative prices, however. In the example given, if the price of C should rise so that the characteristics vector attainable by spending the whole income on that good moved to C'', AB would now be the efficiency frontier. No combination using C would be efficient and C would no longer have any buyers at that price. Price changes may give rise to a substitution effect between goods rising wholly from efficiency effects and unrelated to any convexity of the preference structure. This efficiency substitution effect has been discussed in detail in "A New Approach. . . ."

The general nature of the consumption technology has now been established, and the remainder of the paper will be devoted to answering the question, can we have change, innovation, and technical progress in consumption technology, just as we have in production?

is usually also the supplier of the service; the whole transaction is recorded only in the internal accounts of individual economic units. The obstacles to extracting this information for purposes of economic accounting are almost insuperable; the information must be obtained by a relatively lengthy chain of indirect inference. First, the calculation begins with data on the values of transactions in new investment goods. These values must be separated into prices and quantities of investment goods. Second, the quantity of new investment goods reduced by the quantity of old investment goods replaced must be added to accumulated stocks. Third, the quantity of capital services corresponding to each stock must be calculated.[1]

Paralleling the calculation of quantities of capital services, beginning with quantities of new investment goods, the prices of capital services must be calculated beginning with the prices of new investment goods. Finally, a quantity index of total capital input must be constructed from the quantities of each type of capital service, using the relative shares of the implicit rental value of each capital service in the implicit rental value of all capital services as weights. The implicit rental value of each capital service is obtained by simply multiplying the quantity of that service by the corresponding price. At this final stage the construction of a quantity index of capital input is formally identical to the construction of a quantity index of labor input. The chief difference between the construction of price and quantity indexes of capital input and any other aggregation problem is in the circuitous route by which the necessary data are obtained.

To represent the capital accounts that provide the basis for measuring capital input, we introduce the following notation:

I_k—quantity of output of the kth investment good,
K_k—quantity of input of the kth capital service,
q_k—price of the kth investment goods,
p_k—price of the kth capital service.

We assume that the proportion of an investment replaced in a given interval of time declines exponentially over time. A theoretical justification for this assumption is that replacement of investment goods is a recurrent event. Under this assumption the cumulated stock of past investments in the kth capital good, net of replacements, satisfies the well-known relationship:

$$I_k = \dot{K}_k + \delta_k K_k, \tag{1}$$

[1] Here we assume that the "quantity" of a particular type of capital as an asset is proportional to its "quantity" as a service, whatever the age of the capital. If this condition is not satisfied, capital of each distinct age must be treated as a distinct asset and service. Output at each point of time consists of the usual output plus "aged" capital stock.

where δ_k is the instantaneous rate of replacement of the kth investment good. Similarly, in the absence of direct taxation the price of the kth capital service satisfies the relationship:

$$p_k = q_k\left[r + \delta_k - \frac{\dot{q}_k}{q_k}\right], \tag{2}$$

where r is the rate of return on all capital, δ_k is the rate of replacement of the kth investment good, and $\dot{q}_k/q_k$ is the rate of capital gain on that good. Given these relationships between the price and quantity of investment goods and the price and quantity of the corresponding capital services, the only data beyond values of transactions in new investment goods required for the construction of price and quantity indexes of capital input are rates of replacement for each distinct investment good and the rate of return on all capital. We turn now to measurement of the rate of return.

First, to measure the values of output and input it is customary to exclude the value of capital gains from the value of input rather than to include the value of such gains in the value of output. This convention has the virtue that the value of output may be calculated directly from the value of transactions. Second, to measure total factor productivity, depreciation is frequently excluded from both input and output; this convention is adopted, for example, by Kendrick [12]. Exclusion of depreciation on capital introduces an entirely arbitrary distinction between labor input and capital input, since the corresponding exclusion of depreciation of the stock of labor services is not carried out.[2] To calculate the rate of return on all capital, our method is to subtract from the value of output plus capital gains the value of labor input and of replacement. This results in the rate of return multiplied by the value of accumulated stocks. The rate of return is calculated by dividing this quantity by the value of the stock.[3] The implicit rental value of the kth capital good is:

$$p_k K_k = q_k\left[r + \delta_k - \frac{\dot{q}_k}{q_k}\right]K_k. \tag{3}$$

To calculate price and quantity indexes for total capital input, the prices and quantities of each type of capital service are aggregated, using the relative shares of the implicit rental value of each capital service in the implicit rental value of all capital services as weights.

We have already noted that direct observations are usually available

[2] This point is made by Domar [6].
[3] Domar's procedure [6, p. 717, fn. 3] fails to correct for capital gains. Implicitly, Domar is assuming either no capital gains or that all capital gains are included in the value of output, whether realized or not.

only for values of transactions; the separation of these values into prices and quantities is based on much less complete information and usually involves indirect inferences. An error in the separation of the value of new investment goods into the price and quantity of investment goods will result in errors in measurement of the price and quantity of investment goods, of the price and quantity of capital services and of total factor productivity.[4] To measure the bias in the rate of growth of the quantity of investment goods, we let Q^* be the relative error in the measurement of the price of investment goods, I^* the "quantity" of investment goods output, calculated using the erroneous "price" of investment goods, and I the actual quantity of investment goods output. The bias in the rate of growth of investment goods output is then:

$$\frac{\dot{I}^*}{I^*} - \frac{\dot{I}}{I} = -\frac{\dot{Q}^*}{Q^*}. \tag{4}$$

The rate of growth of this bias is negative if the rate of growth of the error in measurement of the price of investment goods is positive, and vice versa. If we let K^* be the "quantity" of capital calculated using the erroneous "price" of investment goods and K the actual quantity of capital:

$$K^* = \int_{-\infty}^{t} e^{-\delta(t-s)} I^*(s)ds = \int_{-\infty}^{t} e^{-\delta(t-s)} \frac{I(S)}{Q^*(s)} ds.$$

The bias in the rate of growth of the quantity of capital services is then:

$$\frac{\dot{K}^*}{K^*} - \frac{\dot{K}}{K} = \frac{I}{Q^*K^*} - \frac{I}{K}, \tag{5}$$

$$= \frac{I}{\int_{-\infty}^{t} e^{-\delta(t-s)} \frac{Q^*(t)}{Q^*(s)} I(s)ds} - \frac{I}{\int_{-\infty}^{t} e^{-\delta(t-s)} I(s)ds}$$

which is negative of the rate of growth of the error in measurement of the price of investment goods is positive, and vice versa.

To calculate the error of measurement in total factor productivity, we let C represent the quantity of consumption goods and L the quantity of labor input; second, we let v_I represent the relative share of the value of investment goods in the value of total output and v_C the relative share of consumption goods; finally, we let w_K represent the relative share of the value of capital input in the value of total input and w_L the relative share of labor. The rate of growth of total factor productivity

[4] The following analysis summarizes a more detailed argument in [12].

may be represented as:

$$\frac{\dot{P}}{P} = w_I \frac{\dot{I}}{I} + w_C \frac{\dot{C}}{C} - v_K \frac{\dot{K}}{K} - v_L \frac{\dot{L}}{L}.$$

If we let P^* represent the measured rate of growth of total factor productivity using the erroneous "price" of investment goods:

$$\frac{\dot{P}^*}{P^*} = w_I \frac{\dot{I}^*}{I^*} + w_C \frac{\dot{C}}{C} - v_K \frac{\dot{K}^*}{K^*} - v_L \frac{\dot{L}}{L}.$$

Subtracting the first of these expressions from the second we obtain the bias in the rate of growth of total factor productivity:

$$\frac{\dot{P}^*}{P^*} - \frac{\dot{P}}{P} = w_I \left[\frac{\dot{I}^*}{I^*} - \frac{\dot{I}}{I}\right] - v_K \left[\frac{\dot{K}^*}{K^*} - \frac{\dot{K}}{K}\right].$$

Substituting expressions (5) and (4) for the biases in measured rates of growth of capital input and the output of investment goods, we have:

$$(6) \quad \frac{\dot{P}^*}{P^*} - \frac{\dot{P}}{P} = - w_I \frac{\dot{Q}^*}{Q^*} - v_K \left[\frac{I}{\int_{-\infty}^{t} e^{-\delta(t-s)} \frac{Q^*(t)}{Q^*(s)} I(s)ds} - \frac{I}{\int_{-\infty}^{t} e^{-\delta(t-s)} I(s)ds}\right].$$

If investment and the error in measurement are growing at constant rates, the biases in the rates of growth of the quantity of investment goods produced and the quantity of capital services are equal, so that the net effect is equal to the rate of growth in the error in measurement of the price of investment goods multiplied by the difference between the capital share in total input and the investment share in total output.[5]

III. *The Measurement of Capital Input*

We have demonstrated that an error in the measurement of investment goods prices results in errors in the measurement of investment goods output, capital input, and total factor productivity. Roughly speaking, a positive bias in the rate of growth of the investment goods prices index results in a positive bias in the rate of growth of total factor productivity, provided that the share of capital in the value of input exceeds the share of investment in the value of output. This condition is fulfilled for the U.S. private domestic sector throughout the period,

[5] Domar [7, p. 587, formula 51] considers a special case of this problem in which capital "is imported from the outside." This specialization is unnecessary, as suggested in the text.

1929–64. Hence we must examine the indexes of investment goods prices that underly our measurement for possible sources of bias.[6]

First, except for the price index for road construction the published price indexes for structures that underly the U.S. national accounts are indexes of the cost of input rather than the price of output. The use of input prices in place of output prices for structures results in an important error of measurement. To eliminate this error it is necessary to use an output price index in measuring prices of both investment goods output and capital services input. The Bureau of Public Roads prices index for a "standard mile" of highway construction is such an output price index. The corresponding input price index behaves in a manner very similar to that of the input price of all of new construction.[7] Second, the price indexes for equipment that underly the U.S. national accounts are based primarily on data from the wholesale price index of the Bureau of Labor Statistics. Since expenditures on the wholesale price index are less than those on the consumers' price index, adjustments for quality change are less frequent and less detailed. To eliminate this source of bias, we replace the implicit deflator for producers' durables by the corresponding deflator for consumers' durables. In view of the upward bias in the consumers' durables price index this adjustment is conservative.[8] Third, the price indexes for change in business inventories from the U.S. national accounts contain year-to-year fluctuations that appear to be highly implausible. To represent these movements more accurately, we replace the implicit deflator for change in inventories by the deflator for private domestic consumption. The level of this index generally coincides with that of the implicit deflator for change in business inventories; however, the fluctuations are much less than those for the inventory investment deflator.

To study the relationship between the structure of capital and measured productivity change we begin by constructing indexes of investment goods output and capital input without correcting for errors of measurement. As an initial index of investment goods output we take U.S. private domestic investment in constant prices as measured in the U.S. national product accounts. As an index of capital input we take the stock of capital in the private domestic sector of the U.S. economy. The stock of capital is the sum of land, plant, equipment, and inventories employed in this sector. To study the effects of errors in measurement on measured productivity change we weight the rate of growth of investment by its share in U.S. private domestic output and the rate of growth

[6] All data except those for the stock of land are taken from *U.S. Income and Output* [15] and subsequent revisions of the data given there. Data on the stock of land is from Goldsmith [9] and subsequent revisions.

[7] See Dacy [1] on this point and for discussion of the Bureau of Public Roads index.

[8] See Griliches [11].

TABLE 1

ERRORS OF MEASUREMENT IN THE PRICES OF INVESTMENT GOODS AND MEASURED PRODUCTIVITY CHANGE
(Annual Percentage Rates)

	1	2	3	4	5	6	7
1930	−47.93	2.10	−6.49	−49.77	2.50	−6.84	.36
1931	−63.31	.23	−5.11	−62.14	.64	−5.15	.04
1932	−264.28	−1.22	−4.42	−290.46	−.72	−5.03	.61
1933	14.92	−2.85	1.26	−2.15	−2.30	.63	.64
1934	42.68	−2.64	3.26	56.53	−2.26	3.91	−.66
1935	47.75	−1.97	5.31	50.19	−1.47	5.38	−.07
1936	24.94	−.69	3.07	23.72	−.19	2.76	.32
1937	20.07	.33	2.73	26.90	.73	3.56	−.83
1938	−76.58	1.04	−6.82	−72.69	1.73	−6.73	−.09
1939	31.18	−.78	3.77	30.30	−.13	3.44	.33
1940	24.89	.36	3.44	28.82	.98	3.78	−.34
1941	21.16	1.48	2.74	18.46	2.34	1.98	.75
1942	−94.83	2.55	−7.50	−124.59	3.21	−9.81	2.30
1943	−67.36	−.25	−2.24	−92.99	−.12	−3.18	.93
1944	8.61	−1.51	.92	17.23	−1.52	1.27	−.35
1945	29.63	−1.26	2.26	31.74	−1.14	2.34	−.08
1946	61.96	−.46	10.30	65.06	−.38	10.77	−.48
1947	−1.73	4.07	−1.68	−.50	4.29	−1.56	−.12
1948	15.02	3.55	1.62	20.18	3.84	2.51	−.89
1949	−26.04	4.26	−5.46	−27.40	4.98	−5.92	.46
1950	30.74	2.27	5.49	36.26	2.84	6.42	−.93
1951	.99	4.53	−1.50	−4.64	5.74	−3.07	1.57
1952	−15.72	4.13	−4.09	−17.86	4.75	−4.67	.58
1953	1.17	2.79	−.79	3.40	3.21	−.59	−.21
1954	−3.07	2.61	−1.40	2.41	3.13	−.73	−.67
1955	21.23	2.16	3.17	24.68	2.97	3.52	−.35
1956	−1.49	3.60	−1.51	−4.54	4.77	−2.48	.96
1957	−8.11	3.18	−2.48	−7.44	3.96	−2.63	.16
1958	−12.82	2.37	−2.78	−10.69	3.08	−2.70	−.07
1959	17.24	1.45	2.46	20.60	2.15	2.79	−.33
1960	−1.61	2.51	−1.14	.73	3.49	−1.09	−.05
1961	−4.80	2.22	−1.52	−4.57	3.25	−1.85	.33
1962	12.99	1.76	1.51	11.43	2.66	.93	.58
1963	3.38	2.51	−.36	3.26	3.28	−.66	.30
1964	4.86	2.55	−.12	5.95	3.24	−.19	.07

1. Investment output.
2. Capital input.
3. Contribution of capital.
4. Investment output, corrected for errors in the measurement of prices.
5. Capital input, corrected.
6. Contribution of capital, corrected.
7. Contribution of errors in the measurement of prices.

of capital by its share in U.S. private domestic input. The difference is the contribution of capital to the measured rate of growth in total factor productivity.

Rates of growth of investment goods output, capital input, and the

rate of growth of the contribution of capital to total factor productivity are given in columns 1–3 of Table 1. Since errors of measurement and cyclical and transitory phenomena induce large and negatively serially correlated fluctuations in measured annual percentage changes, we will analyze here only the average rates of changes for the whole 1929–64 period. During this period the average rate of growth of the contribution to changes in total factor productivity was −.12 percent per year. To eliminate errors of measurement in the prices of investment goods, we replace our initial index of investment goods output by a new index of U.S. gross private domestic investment in constant prices. This index is calculated with the Bureau of Public Roads price index for structures, the consumers' durables price index for equipment, and the private domestic consumption price index for change in inventories. We replace our initial index of capital input by the stock of capital calculated with these same price indexes. Columns 4–6 of Table 1 present rates of growth of the alternative indexes of investment goods output, capital input, and the rate of growth of their contribution to total factor productivity. The average rate of growth of their contribution to changes in total factor productivity is −.25 percent per year. Finally, column 7 gives the rate of growth of the contribution of errors in measurement of the prices of investment goods to measured productivity change. These errors account for .14 percent per year of the measured rate of growth of total factor productivity.

We have outlined a method for computing the price of capital services in the absence of direct taxation of business income. In the presence of direct taxes we may distinguish between the price of capital services before and after taxes. The expression (2) given above for the price of capital services is the price after taxes. The price of capital services before taxes is:

$$p_k = q_k \left[\frac{1 - uv}{1 - u} r + \frac{1 - uw}{1 - u} \delta_k - \frac{1 - ux}{1 - u} \frac{\dot{q}_k}{q_k} \right] \tag{7}$$

where u is the rate of direct taxation, v the proportion of return to capital allowable as a charge against income for tax purposes, w the proportion of replacement allowable for tax purposes and x the proportion of capital gains included in income for tax purposes.

We estimate the variables describing the tax structure as follows: The rate of direct taxation is the ratio of profits tax liability to profits before taxes. The proportion of the return to capital allowable for tax purposes is the ratio of net interest to the total return to capital. Total return to capital is the after tax rate of return, r, multiplied by the current value of capital stock. The proportion of replacement allowable for tax purposes is the ratio of capital consumption allowances to the cur-

rent value of replacement. The proportion of capital gains included in income is zero by the conventions of the U.S. national accounts. Given the value of direct taxes we estimate the after tax rate of return by subtracting from the value of output plus capital gains the value of labor input, replacement, and direct taxes. This results in the total return to capital. The rate of return is calculated by dividing this quantity by the current value of the stock of capital. Given data on the rate of return and the variables describing the tax structure, we calculate the price of capital services before taxes for each investment good. These prices of capital services are used in the calculation of indexes of capital input.

For the U.S. private domestic economy it is possible to distinguish four classes of investment goods—land, structures, equipment, and inventories. Although it is also possible to distinguish a number of subclasses within each of these groupings, we will employ only the four major groups in calculating an index of total capital input. For each group we first compute a before tax service price analogous to (7). We then compute an index of capital input as a Divisia index[9] of the services of land, structures, equipment, and inventories. This index may be compared with our initial index of capital input. In this index the relative share of the asset value of each investment good in the value of all assets is used as a weight. Our index eliminates the conceptual error arising from the implicit assumption that service prices are proportional to asset prices for different investment goods.[10] Similarly, we compute an index of investment goods output as a Divisia index of the output of structures, equipment, and inventories; we assume that the output of land as an investment good is zero for the U.S. private domestic economy.

Rates of growth of Divisia indexes of capital input, investment goods output, and the rate of growth of the contribution to total factor productivity with errors in the measurement of the prices of investment

[9] See Divisia [3-5] and Wold [16]. The basic formula for capital stock employed throughout is:

$$K_{t+1} = I_t + (1 - \delta)K_t,$$

where I_t is the value of gross private domestic investment for each category in constant prices. The initial (1929) value of capital stock in constant prices of 1958 and the depreciation rates employed are as follows:

	National Accounts Deflators		Alternative Deflators	
	K_{1929}	δ	K_{1929}	δ
Land	270,000	0	270,000	0
Structures	347,622	0.0437	214,287	0.0443
Equipment	69,587	0.1394	48,065	0.1290
Inventories	48,504	0	48,504	0

[10] For examples, see Kendrick [14, p. 106] and Denison [2, pp. 97-8]; see the comments by Griliches [10, p. 129].

TABLE 2

ERRORS OF AGGREGATION AND RATE OF UTILIZATION AND MEASURED PRODUCTIVITY CHANGE
(Annual Percentage Rates)

	1	2	3	4	5	6	7	8
1930	−50.54	2.90	−7.07	.23	.16	2.45	−6.92	−.15
1931	−63.13	.88	−5.30	.16	−7.52	.43	−5.16	−.14
1932	−377.24	−.40	−6.69	1.66	−15.43	−.85	−6.56	−.13
1933	−5.99	−5.39	1.44	−.81	13.89	−5.84	1.57	−.13
1934	54.13	−2.76	3.94	−.03	11.44	−3.21	4.09	−.15
1935	50.21	−2.15	5.61	−.23	5.31	−2.60	5.77	−.15
1936	23.65	−.09	2.71	.04	11.99	−.54	2.87	−.16
1937	26.97	.57	3.62	−.07	9.57	.12	3.78	−.16
1938	−76.72	2.25	−7.25	.52	5.02	1.80	−7.10	−.15
1939	30.68	−.02	3.45	−.01	9.99	2.13	2.71	.74
1940	29.54	1.48	3.70	.08	12.72	3.63	2.92	.78
1941	18.85	3.17	1.73	.26	21.31	5.32	.90	.82
1942	−124.08	4.32	−10.19	.39	27.77	6.46	−11.01	.82
1943	−89.79	−.54	−2.91	−.27	21.08	1.61	−3.71	.80
1944	17.36	−2.43	1.62	−.35	12.78	−.28	.80	.82
1945	31.21	−1.62	2.49	−.15	15.06	.53	1.68	.81
1946	64.04	.17	10.41	.36	22.19	2.32	9.66	.75
1947	−.03	5.84	−2.03	.46	23.44	7.99	−2.77	.74
1948	19.82	5.90	1.70	.80	20.65	8.05	.94	.77
1949	−26.80	6.63	−6.42	.50	9.31	8.78	−7.19	.77
1950	35.98	5.28	5.46	.96	13.40	7.43	4.66	.79
1951	−5.19	6.32	−3.39	.32	23.07	8.46	−4.20	.81
1952	−16.76	5.68	−4.82	.15	12.33	7.83	−5.60	.77
1953	3.64	4.18	−.89	.30	8.33	6.33	−1.64	.75
1954	2.24	4.71	−1.31	.58	8.63	6.86	−2.06	.76
1955	24.98	4.31	3.10	.43	11.58	5.63	2.62	.48
1956	−4.62	5.11	−2.61	.13	16.90	6.43	−3.06	.45
1957	−7.38	4.94	−2.96	.33	13.60	6.26	−3.42	.46
1958	−10.69	4.18	−3.09	.39	9.67	5.50	−3.56	.47
1959	20.77	3.07	2.49	.30	9.53	4.39	2.03	.47
1960	.75	4.22	−1.34	.25	9.67	5.54	−1.80	.46
1961	−4.78	3.81	−2.08	.23	11.00	5.12	−2.54	.47
1962	11.69	2.81	.91	.01	12.88	4.13	.44	.48
1963	3.23	3.58	−.77	.11	12.08	4.90	−1.25	.48
1964	5.85	3.56	−.32	.14	11.52	4.88	−.80	.48

1. Investment output, Divisia index.
2. Capital input, Divisia index.
3. Contribution of capital, Divisia index.
4. Contribution of errors of aggregation and the conceptual error of assuming that service prices are proportional to asset prices.
5. Rate of return after taxes.
6. Capital input, corrected for variations in the rate of utilization.
7. Contribution of capital, corrected.
8. Contribution of the conceptual error of assuming that capital services are proportional to capital stock for each type of asset.

goods eliminated are presented in Table 2. These indexes eliminate errors of aggregation and the conceptual error of assuming that service prices are proportional to asset prices. With these errors removed the average

rate of growth of the contribution of capital to changes in total factor productivity is −.49 percent per year. Thus such errors contribute .23 percent per year to the measured rate of growth of total factor productivity. A by-product of the computation of the Divisia index of capital input is an estimate of the rate of return after taxes in the U.S. private domestic economy. For completeness this rate of return is presented in column 5 of Table 2.

Up to this point we have assumed that capital services are proportional to capital stock for each type of asset. Adjustment of data on capital stock for variation in rates of utilization is essential if comparability of data on labor and capital services is to be preserved. Fortunately, relatively good data on the trend of hours per machine are available for electric motors in the manufacturing sector.[11] The relative utilization of electric motors provides an indicator of the relative utilization of capital in the manufacturing sector, since electric motors are the predominant source of direct power. We assume that the relative utilization of capital for the U.S. private domestic economy is the same as that for the manufacturing sector. When better data become available one should be able to improve upon this assumption.

The rate of growth of investment goods output is unchanged in correcting capital input for variations in the rate of utilization of capital stock. The rate of growth of capital input after this correction is presented in column 6 of Table 2. The rate of growth of the contribution of capital to changes in total factor productivity after correction is presented in column 7. The estimated average rate of growth of the contribution of capital is now −.94 percent per year, the conceptual error of assuming that capital service is proportional to capital stock accounting for .45 percent per year of the measured rate of growth of total factor productivity.

IV. *Summary and Conclusion*

We have studied the relationship between the structure of capital and measured productivity change in the U.S. private domestic economy, 1929–64, and found that errors in measurement of the contribution of capital account for a substantial proportion of measured changes in total factor productivity. The total error in the measurement of total factor productivity from this source is .82 percent per year. Of this total .14 percent per year is due to errors in the measurement of prices of investment goods. Errors in aggregation and the conceptual error of assuming that service prices are proportional to asset prices account for .23 percent per year. The conceptual error of assuming that capital ser-

[11] See Foss [8]. Foss provides estimates on relative utilization only for 1929 and 1954. These have been updated to 1962 for this study. Thus, they catch only the trend in relative utilization and make no allowance for the shorter run cyclical fluctuations.

vices are proportional to capital stock for each type of asset accounts for .45 percent per year.[12]

The total error of measurement of the contribution of capital of .82 percent per year may be compared with Denison's average rate of growth of output per unit of input for the U.S. economy, 1929–57, of .93 percent per year. Denison's figure is corrected for errors in the measurement of labor services, but not for errors in the measurement of the contribution of capital. For the 1929–57 period, Denison's corrections for errors in the measurement of labor services amount to .77 per year. The effects of errors in the measurement of the contribution of capital we have studied are, if anything, slightly greater. Of course, our figures are not strictly comparable with those of Denison either in coverage or time span. Nevertheless, our results suggest that the residual, our ignorance, or advance of knowledge has been substantially overstated, even by Denison.

In explaining economic growth we suggest greater reliance than heretofore on the twin pillars of human and nonhuman capital, each supporting an important part of the capital structure. Perhaps the day is not far off when economists can remove the intellectual scaffolding of technical change altogether.

[12] Since some of these corrections are based on fragmentary evidence, all of these numbers should be taken only as indications of the probable orders of magnitude of the various errors. We need more research and better data for more precise estimates.

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TOWARD A THEORY OF INVENTIVE ACTIVITY AND CAPITAL ACCUMULATION*

By KARL SHELL
Massachusetts Institute of Technology

I. *Introduction*

In at least two recent models of economic growth, the rate of technical change depends upon other economic variables. The first, a model introduced by Kaldor [3] [4] [5], assumes a positive relationship (the technical progress function) between relative changes in productivity per worker and relative changes in gross investment. The technical progress function is an eclectic amalgam summarizing basic technical and institutional forces in a free enterprise economy. Kaldor takes the Schumpeterian view that the creation of new ideas largely occurs at an autonomous rate, but that the implementation of these new techniques by entrepreneurs can be explained by economic phenomena. Obviously, if the implementation of a new technique requires new capital equipment as opposed to mere organizational change, increased productivity can only be transmitted through new (gross) investment. In addition, Kaldor argues that for a capitalist economy the higher the relative rate of gross investment the higher the degree of "technical dynamism." Technical dynamism is a mass measure of entrepreneurial psychology including the readiness to adopt new methods of production.

In the second model with endogenous technical change, Arrow [1] concentrates upon the relation between learning and experience. Economic learning results in higher productivity, while cumulative gross investment is the measure of economic experience. Therefore, in refining the technical progress function, Arrow explicitly postulates that productivity per worker is determined by accumulated gross investment. The production of new technical knowledge (invention) and the transmission and application of that knowledge (innovation) are treated as by-products in the production and adoption of new capital goods.

While it is doubtlessly true that technical change is related to gross investment both as a by-product of capital goods production and as a vehicle for embodying new techniques in new capital equipment, it is also true that the rate of production of technical knowledge can be increased by increasing the allocation of economic resources explicitly devoted to inventive activity.

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At least two peculiar properties of technical knowledge require special study. First, technical knowledge can be used by many economic units without altering its character. Thus, for the economy in which technical knowledge is a commodity, the basic premises of the classical welfare economics are violated, and the optimality of the competitive mechanism is not assured. Typically, technical knowledge is very durable and the cost of transmission is small in comparison to the cost of production. Second, at least on the microeconomic level, the inventive process is characterized by extreme riskiness.[1]

II. *The Model*

I have argued that increases in technical knowledge are fundamentally related to the amount of resources explicitly devoted to inventive activity. In order to study the respective roles of invention and investment in economic growth, I assume that current aggregate output $Y(t)$ is determined by the relation

$$Y(t) = F[A(t), K(t), L(t)], \tag{1}$$

where $A(t)$ and $K(t)$ denote the current levels of the stocks of technical knowledge and physical capital, respectively; $L(t)$ denotes the current size of the labor force inelastically offered for employment.[2]

The growth in the stock of technical knowledge satisfies the differential equation

$$A(t) = \sigma\alpha(t) Y(t) - \rho A, \tag{2}$$

where $0 \leq \alpha(t) \leq 1$ is the fraction of output currently devoted to invention and $0 < \sigma \leq 1$ is the fraction of inventions that are "successful." For the case where ρ is positive, equation (2) should be understood as a long-run approximation to processes not explicitly treated in the model. For example, decay in technical knowledge is observed because of the imperfect transmission of technical information from one generation of the labor force to the next.

If capital is subject to evaporative decay at the given technical rate $\mu > 0$, then

$$\dot{K}(t) = s(t)(1 - \alpha(t)) Y(t) - \mu K(t), \tag{3}$$

where $0 \leq s(t)(1 - \alpha(t)) \leq 1$ is the fraction of output currently devoted to investment.

[1] Cf., e.g., [8], especially Arrow's contribution on pp. 609-25.

[2] I treat the one-sector model for ease of exposition. This implies that the production possibility frontier is a hyperplane in the consumption-investment-invention space. If the model is disaggregated to two or three sectors, then the frontier can possess greater curvature. Also notice that increases in efficiency are shared by all vintages of capital and labor.

III. *A Stylized Economy*

In the United States, intervention in behalf of inventive activity has taken two basic forms: (1) the establishment of a legal device, the patent, designed to bestow property rights on certain of the outputs of the inventive process; (2) direct nonmarket support of research and development. The universities and the Department of Agriculture, for example, have contributed to our economy in the second role. Recently the Department of Commerce has initiated industrial research programs modeled after the programs of the agricultural research stations, while the Department of Defense favors the device of contracting research tasks to private enterprises on a cost-plus-fixed-fee basis.

Consider a model economy in which production is undertaken by many individual firms. The level of technical knowledge enters each firm's production function as a pure public good of production. Hence, the competitive price of invention is zero—suggesting the desirability of social intervention in the market process.[3] Assume that the only form of intervention is the imposition of a tax upon output at a given constant rate $0<\alpha<1$, the proceeds of which are used to support invention. The private sector saves (and invests in capital accumulation) the constant fraction $0<s<1$ of disposable income.

I assume that the production function given in (1) exhibits constant returns in capital and labor and consequently increasing returns in all three factors. Then if the special assumption is made that there are constant returns to (Hicks-neutral) technical knowledge,[4] then (1) can be rewritten as

$$y = Af(k), \tag{4}$$

where lower-case letters denote quantities per worker and $f(k)$ is shorthand for $F(k, 1)$. $f(\cdot)$ is twice-continuously differentiable with $f(k)>0, f'(k)>0, f''(k)<0$ for $0<k<\infty$; $f(0)=0, f(\infty)=\infty, f'(0)=\infty$, $f'(\infty)=0$. For simplicity, I assume that there is no change in the labor force, thus setting $L=1$ and writing

$$A = \alpha\sigma y - \rho A, \tag{5}$$

$$k = s(1-\alpha)y - \mu k. \tag{6}$$

From (4) and (5), $\dot{A}=0$ if and only if

[3] In practical applications, the distinction between private and public goods is fuzzy. What is considered to be a public good under one set of legal and social arrangements may be considered to be a private good under a different set of arrangements. In choosing among differing arrangements, society should include in its calculation the buying and selling costs that they imply.

[4] Qualitative long-run behavior does not depend upon this special assumption. It does allow a simple aggregation congenial to the competitive hypothesis: $Y=\Sigma_j Y_j = A\Sigma_j F(K_j, L_j)$ where, for example, K_j is the quantity of capital employed by the jth firm.

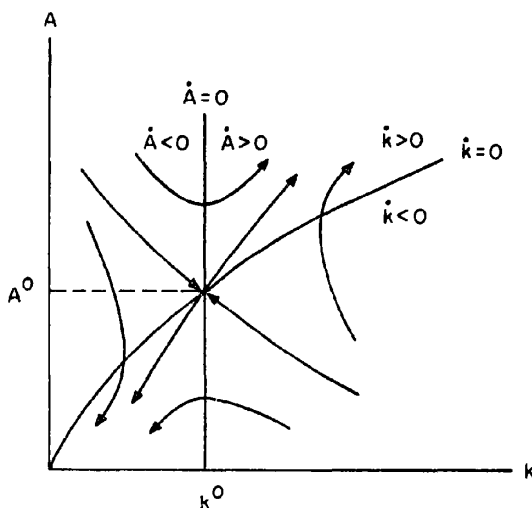
$$f(k) = \frac{\rho}{\alpha\sigma} \cdot \tag{7}$$

Call the unique solution to (7) k^0. But from (4) and (6), $\dot{k}=0$ if and only if

$$A = \frac{\mu k}{s(1-\alpha)f(k)}, \tag{8}$$

which yields

$$\left(\frac{dA}{dk}\right)_{\dot{k}=0} = \frac{\mu[f(k) - kf'(k)]}{s(1-\alpha)[f(k)]^2} > 0.$$

The laws of motion for the stylized economy are shown in the phase diagram below. To verify that (A^0, k^0), the unique solution to (7) and

(8), is a saddlepoint, solve the characteristic equation for the linear Taylor approximation to (5) and (6) about (A^0, k^0). The characteristic roots are real and opposite in sign, guaranteeing that (A^0, k^0) is a local saddlepoint. Twice-continuous differentiability of $f(\cdot)$ then guarantees that (A^0, k^0) is a global saddlepoint. Notice that for initial endowments sufficiently large (small), the stylized economy explodes (decays).[5]

[5] In the multisector model, it is possible that there are many equilibrium points. Some would be saddlepoints; the others would be stable or surrounded by limit cycles. See [10]. Global stability is an interesting property for a descriptive growth model but should not be thought to be essential. In fact, Maruyama [7] argues that social systems are basically morphogenetic rather than morphostatic.

IV. *The Controlled Economy*

In what follows, it will be convenient to assume that equation (1) can be rewritten as

$$y = g(A, k), \tag{9}$$

where $g(\cdot)$ is an increasing strictly concave function of A and k. This assumption is made in order to avoid complications by insuring that the usual necessary conditions for optimality are also sufficient conditions. Suppose that the economic planning board desires to maximize the sum of future discounted utility (of per capita consumption)

$$\int_0^\infty U[c(t)]e^{-\delta t}dt. \tag{10}$$

subject to given initial endowments $A(0)=A_0$, $k(0)=k_0$, and technology given by (5), (6), (9). $U[c(t)]$ is the utility of consumption at time t, with $U'[c]>0$, $U''[c]<0$ for $0<c<\infty$, $U'[0]=\infty$, $U'[\infty]=0$. $\delta>0$ is the (constant) pure rate of social discount. For simplicity, set $L(t)\equiv 1$.

Because the marginal utility of consumption is infinitely large when consumption is zero, consumption must be everywhere positive on the optimal program. Further assume that the initial endowments (A_0, k_0) are sufficiently small that the optimal program will never be specialized to consumption. A feasible consumption program $\{c(t):0\leq t\leq\infty\}$ satisfying (5), (6), and (9) is optimal[6] if, and only if, there exist continuous functions $q(t)$ and $v(t)$ such that

$$\dot{q} = (\delta + \mu)q - \max(q, v\sigma)g_k(A, k), \tag{11}$$

$$\dot{v} = (\delta + \rho)v - \max(q, v\sigma)g_A(A, k), \tag{12}$$

$$\lim_{t\to\infty} q(t)e^{-\delta t} = \lim_{t\to\infty} v(t)e^{-\delta t} = 0, \tag{13}$$

where $0\leq\alpha(t)\leq 1$ and $0\leq s(t)\leq 1$ are chosen at each instant to maximize

$$U[(1-s)(1-\alpha)g(A, k)] + \{qs(1-\alpha) + v\alpha\sigma\}g(A, k). \tag{14}$$

If utility is the numeraire, then q and v are the social demand prices for investment and invention, respectively. Conditions (11) and (12) imply that the planning board has perfect foresight with respect to marginal products. Transversality condition (13) requires the present value of a unit of future investment or invention to become small as the future date becomes distant. Expression (14) is simply the certainty

[6] Cf. [9], especially theorem 7, p. 69, and pp. 188-91, 298-300.

equivalent of the imputed value of gross national product. Maximization of (14) implies that if $U'=q>v\sigma$ then $\alpha=0$, and if $U'=v\sigma>q$ then $s=0$. Further if the certainty equivalent net marginal products are equal, $\sigma g_A-\rho=g_k-\mu$, then $q=v\sigma$.

Notice that (11) and (12) have a special interpretation in a decentralized economy in which factors are rewarded by their marginal products. From (11), for example, the change in the demand price of investment should be such as to compensate the representative rentier for "abstinence" and depreciation loss net of rewards from the employment of his capital. Of course, for a simple decentralization of the economy treated above, condition (11) will not necessarily hold. If output is taxed to support invention, then private factors will not be paid their full marginal products. At any rate, if an optimal program exists in the fully controlled economy, the stocks of technical knowledge and physical capital will approach limiting values (A^*, k^*) such that $\sigma g_A(A^*, k^*)-\rho =g_k(A^*, k^*)-\mu=\delta$. The limiting value of consumption c^* is given by $c^*=g(A^*, k^*)-\mu k^*-\rho A^*/\sigma$.

It may be that the planning board treats the process of private saving and investment in physical capital as institutionally given leaving the choice of $\{\alpha(t):0\leq t\leq\infty\}$ as the remaining policy instrument. Assume, for example, that private capital accumulation follows (6) with savings $0<s<1$ a given fixed fraction of disposable income. The planning board desires to maximize (10) subject to given initial endowments and subject to technological and behavioral relations (5), (6), (9). It is implicit in this formulation that the free play of the private capital market does not necessarily yield a socially preferred result. There could be several reasons for this divergence, including the existence of conventional externalities and certain intrinsic impediments to borrowing and lending in a risky world.[7]

In the partially controlled economy, maximization of (14) implies that $(1-s)U'+qs\geq v\sigma$, or with equality if $\alpha>0$. The optimal consumption program $\{c(t):0\leq t\leq\infty\}$ in the partially controlled economy is such that the stocks of technical knowledge and physical capital approach limiting values (A^{**}, k^{**}) where $\sigma g_A(A^{**}, k^{**})-\rho=\delta$. Notice that asymptotically the marginal products of technical knowledge are identical for the partially and fully controlled economies. This is a (long-run) dynamic generalization of the Rule of the Second Best. If, for example, long-run private savings are too low, then long-run inventive activity will be greater in the partially controlled economy than it would be in the fully controlled economy.

[7] Arrow in [8] refers to such impediments as "moral hazards." I have found an exposition very similar to what follows in an unpublished paper by Arrow [2].

V. *Concluding Comments*

I have argued that in the study of aggregative models it is useful to think of technical knowledge as a public good of production, while the level of inventive activity (the process of production of knowledge) is dependent upon the amount of economic resources devoted to that activity. Of course, invention is a particularly risky form of social investment. In my model, a given fraction of inventions is "successful," thus removing the difficult decision problems associated with uncertainty. Perhaps this is a legitimate approximation in a macroeconomic model. On the other hand, differential equation (2) represents the most unsatisfactory simplification in the model. A complete theory should explicitly treat the problems of transmission of knowledge, e.g., education, book publishing, etc., and its effect upon the efficiency of different generations of the labor force and different vintages of capital. Finally, the recent contribution of Kennedy [6] warns us that additions to technical knowledge should not be thought of as increasing efficiency in any specified way. That is, the "bias of technical progress," whether in a stylized economy or in a planned economy, should be a subject for economic decision.

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INVESTMENT IN HUMANS, TECHNOLOGICAL DIFFUSION, AND ECONOMIC GROWTH

By RICHARD R. NELSON, *RAND Corporation*
and EDMUND S. PHELPS, *Yale University*

I. *Introduction*

Most economic theorists have embraced the principle that certain kinds of education—the three R's, vocational training, and higher education—equip a man to perform certain jobs or functions, or enable a man to perform a given function more effectively. The principle seems a sound one. Underlying it, perhaps, is the theory that education enhances one's ability to receive, decode, and understand information, and that information processing and interpretation is important for performing or learning to perform many jobs.

In applying this principle we find it fruitful to rank jobs or functions according to the degree to which they require adaptation to change or require learning in the performance of the function. At the bottom of this scale are functions which are highly routinized: e.g., running a power saw or diagnosing a malfunction in an automobile. In these functions, the discriminations to be made and the operations based on them remain relatively constant over time. In the other direction on this scale we have, for example, innovative functions which demand keeping abreast of improving technology. Even a highly routinized job may require considerable education to master the necessary discriminations and skills. But probably education is especially important to those functions requiring adaptation to change. Here it is necessary to learn to follow and to understand new technological developments.

Thus far, economic growth theory has concentrated on the role of education as it relates to the completely routinized job. In its usual, rather general form, the theory postulates a production function which states how maximum current output depends upon the current services of tangible capital goods, the current number of men performing each of these jobs, the current educational attainments of each of these jobholders, and time. To simplify matters, some analysts have specified a production function in which output depends upon tangible capital and "effective labor"; the latter is a weighted sum of the number of workers, the weight assigned to each worker being an increasing function of that worker's educational attainment. This specification assumes that highly educated men are perfect substitutes for less educated men (in the technical sense that the marginal rate of substitution between them is constant). Actually, it is possible that educated men are more sub-

stitutable for certain capital goods than for other labor; they permit production with less complex machines. However, the exact specification of the production function does not concern us. The pertinent feature of this kind of production function is this: The "marginal productivity" of education, which is a function of the inputs and the current technology, can remain positive forever even if the technology is stationary. In the models we shall later introduce, education has a positive payoff only if the technology is always improving.

We shall consider now the importance of education for a particular function requiring great adaptation to change. We then propose two models which these considerations suggest.

II. *The Hypothesis*

We suggest that, in a technologically progressive or dynamic economy, production management is a function requiring adaptation to change and that the more educated a manager is, the quicker will he be to introduce new techniques of production. To put the hypothesis simply, educated people make good innovators, so that education speeds the process of technological diffusion.

Evidence for this hypothesis can be found in the experience of United States agriculture.[1] It is clear that the farmer with a relatively high level of education has tended to adopt productive innovations earlier than the farmer with relatively little education. We submit that this is because the greater education of the more educated farmer has increased his ability to understand and evaluate the information on new products and processes disseminated by the Department of Agriculture, the farm journals, the radio, seed and equipment companies, and so on.[2] The better educated farmer is quicker to adopt profitable new processes and products since, for him, the expected payoff from innovation is likely to be greater and the risk likely to be smaller; for he is better able to discriminate between promising and unpromising ideas, and hence less likely to make mistakes. The less educated farmer, for whom the information in technical journals means less, is prudent to delay the introduction of a new technique until he has concrete evidence of its profitability, like the fact that his more educated friends have adopted the technique with success.

This phenomenon, that education speeds technological diffusion, may take different forms outside of agriculture. In large, industrial corpora-

[1] See E. M. Rogers, *Diffusion of Innovations* (Free Press, 1962), especially Chap. 6.

[2] To be sure, some of the correlation described between education and diffusion may be spurious. Some farmers are undoubtedly both progressive and educated because they come from progressive and prosperous farming families that could afford to give them an education. But there is no question that educated farmers do read technical, innovation-describing literature more than do less educated farmers—and presumably because they find it profitable to do so.

tions, in which there is a fine division of labor, the function of keeping abreast of technological improvements (though perhaps not the ultimate responsibility for innovation) may be assigned to scientists. In this case, their education is obviously important; but so too is the education and sophistication of top management which must make the final decisions.[3]

So much for our broad hypothesis and the evidence supporting it. We shall consider now two specific models of the process of technological diffusion and the role of education.

III. *Two Models of Technological Diffusion*

We shall adopt a postulate about the factor-saving character of technical progress which permits us to speak meaningfully about the "level" or "index" of technology. Specifically, we suppose that technical progress is Harrod-neutral everywhere (i.e., for all capital-labor ratios), so that progress can be described as purely labor-augmenting. This means that if output, Q, is a function of capital, K, labor, L, and time, t, the production function may be written

$$Q(t) = F[K(t), A(t)L(t)] \tag{1}$$

In (1), the variable $A(t)$ is our index of technology in practice. If we interpret (1) as a vintage production function in which $K(t)$ is the quantity of currently purchased capital, $L(t)$ the labor working with it, and $Q(t)$ the output producible from it, then $A(t)$ measures the best-practice level of technology, the average technology level "embodied" in the representative assortment of capital goods currently being purchased. Alternatively, we could suppose that all technical progress is wholly "disembodied" and that (1) is the "aggregate" production function for the firm, industry or economy and $A(t)$ is the average index of technology common to all vintages of capital, old and new.

In addition to this concept, we introduce the notion of the theoretical level of technology, $T(t)$. This is defined as the best-practice level of technology that would prevail if technological diffusion were completely instantaneous. It is a measure of the stock of knowledge or body of techniques that is available to innovators. We shall suppose that the theoretical technology level advances exogenously at a constant exponential rate λ:

$$T(t) = T_0 e^{\lambda t}, \qquad \lambda > 0 \tag{2}$$

[3] For an interesting essay on science policy, in which it is argued that Britain's growth has suffered from a shortage of scientists in management, that too small a fraction of scientists are engaged in using (rather than adding to) the existing stock of knowledge, see C. F. Carter and B. R. Williams, "Government Scientific Policy and the Growth of the British Economy," *The Manchester School*, Sept., 1964.

First model. Our first model is as simple a one as we can invent. It states that the time lag between the creation of a new technique and its adoption is a decreasing function of some index of average educational attainment, h, of those in a position to innovate. (We may think of h as denoting the degree of human capital intensity.) Letting w denote the lag, we can represent this notion as follows:

$$A(t) = T(t - w(h)), \qquad w'(h) < 0. \tag{3}$$

The level of technology in practice equals the theoretical level of technology w years ago, w a decreasing function of h.

Substitution of (2) in (3) yields

$$A(t) = T_0 e^{\lambda[t-w(h)]} \tag{4}$$

If h is constant, two results follow from (4). First, the index of technology in practice grows at the same rate, λ, as the index of theoretical technology. Second, the "level" or path of the technology in practice is an increasing function of h, since an increase of h shortens the lag between $T(t)$ and $A(t)$.

An important feature of this model is that, *ceteris paribus*, the return to education is greater the faster the theoretical level of technology has been advancing. As equation (5) shows, the effect upon $A(t)$ of a marginal increase of h is an increasing function of λ, given $A(t)$, and is positive only if $\lambda > 0$.

$$\frac{\partial A(t)}{\partial h} = -\lambda w'(h) T_0 e^{\lambda[t-w(h)]} = -\lambda w'(h) A(t). \tag{5}$$

The same property is displayed by the "marginal productivity of educational attainment." Using (1) and (4) we have

$$Q(t) = F[K(t), T_0 e^{\lambda[t-w(h)]} L(t)] \tag{6}$$

Hence,

$$\frac{\partial Q(t)}{\partial h} = \lambda T_0 e^{\lambda[t-w(h)]} L(t)[-w'(h)]F_2 = -\lambda w'(h) \times \text{Wage Bill}. \tag{7}$$

Thus the marginal productivity of education is an increasing function of λ, given the current wage bill, and is positive only if $\lambda > 0$. This feature is not found in the conventional treatment of education described at the beginning of this paper.

This first model is not altogether satisfactory. It is unreasonable to suppose that the lag of the best-practice level behind the theoretical

level of technology is independent of the profitability of the new techniques not yet introduced. Further, it is somewhat unrealistic to suppose that an increase of educational attainments instantaneously reduces the lag. In these respects, our second model is somewhat more realistic.

Second model. Our second model states that the rate at which the latest, theoretical technology is realized in improved technological practice depends upon educational attainment and upon the gap between the theoretical level of technology and the level of technology in practice. Specifically,

$$\dot{A}(t) = \Phi(h)[T(t) - A(t)] \tag{8}$$

or equivalently

$$\frac{\dot{A}(t)}{A(t)} = \Phi(h) \left[\frac{T(t) - A(t)}{A(t)}\right], \qquad \Phi(0) = 0, \qquad \Phi'(h) > 0. \tag{8'}$$

According to this hypothesis, the rate of increase of the technology in practice (not the level) is an increasing function of education attainment and proportional to the "gap," $(T(t)-A(t))/A(t)$.

Some results parallel to those in the first model can be obtained if we again postulate exponential growth of $T(t)$, as in (2), and constancy of h. First in the long run, if h is positive, the rate of increase of the level of technology in practice, $\dot{A}(t)/A(t)$, settles down to the value λ, independently of the index of education attainment. The reason is this: if, say, the level of h is sufficiently large that $\dot{A}(t)/A(t) > \lambda$ initially, then the gap narrowed; but the narrowing of the gap reduces $\dot{A}(t)/A(t)$; the gap continues to narrow until, in the limit, $\dot{A}(t)/A(t)$ has fallen to the value λ at which point the system is in equilibrium with a constant gap.

Another result is that the asymptotic or equilibrium gap is a decreasing function of educational attainment. Thus increased educational attainment increases the path of the technology in practice in the long run.

Both these results are shown by Figure 1 and by (9), which is the solution to our differential equation (8), given (2):

$$A(t) = \left(A_0 - \frac{\Phi}{\Phi + \lambda} T_0\right) e^{-\Phi t} + \frac{\Phi}{\Phi + \lambda} T_0 e^{\lambda t}. \tag{9}$$

As both (9) and Figure 1 imply, the equilibrium path of the technology in practice is given by

$$A^*(t) = \frac{\Phi(h)}{\Phi(h) + \lambda} T_0 e^{\lambda t}; \tag{10}$$
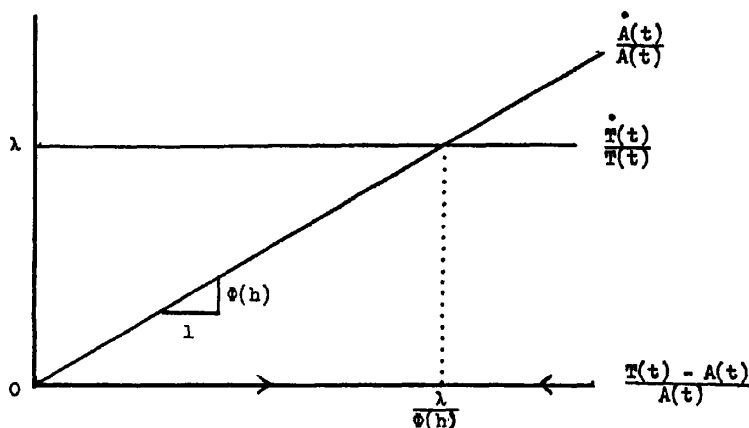
FIGURE 1

the equilibrium gap is given by

$$\frac{T(t) - A^*(t)}{A^*(t)} = \frac{\lambda}{\Phi(h)} . \tag{11}$$

In a technologically stangnant economy ($\lambda = 0$), the gap approaches zero for every $h > 0$. In a technologically progressive economy ($\lambda > 0$), there is a positive equilibrium gap for every h and λ. The equilibrium gap is increasing in λ and decreasing in h.

In the first model it was seen that the marginal productivity of educational attainment is an increasing function of λ and positive only if $\lambda > 0$. That is also true of the second model in the long run (once the effect of an increase of h has had time to influence the level of $A(t)$ as well as its rate of change). Equation (12) shows that the elasticity of the long-run equilibrium level of technology in practice, $A^*(t)$, with respect to h is increasing in λ:

$$\frac{\partial A^*(t)}{\partial h} \frac{h}{A^*(t)} = \left[\frac{h\Phi'(h)}{\Phi(h)}\right] \left[\frac{\lambda}{\Phi(h) + \lambda}\right] \tag{12}$$

This indicates that the payoff to increased educational attainment is greater the more technologically progressive is the economy.

These are only partial models and excessively simple ones. No machinery has been given for determining educational attainment.[4] The

[4] This is done in a paper by Phelps which develops a Golden Rule of Education. It is shown that Golden Rule growth requires more education the more technologically progressive is the economy.

theoretical level of technology has been treated as exogenous. Finally, it might be useful to build a model which combines elements of both the first and second model: the rate of technical progress in practice may depend both upon the length of time during which a new technique has been in existence and upon its profitability. But we hope that these two models may be a useful starting point.

IV. *Concluding Remarks*

The general subject at this session is the relationship between capital structure and technological progress. Recalling that the process of education can be viewed as an act of investment in people that educated people are bearers of human capital, we see that this paper has relevance to that subject. For, according to the models presented here, the rate of return to education is greater the more technologically progressive is the economy. This suggests that the progressiveness of the technology has implications for the optimal capital structure in the broad sense. In particular, it may be that society should build more human capital relative to tangible capital the more dynamic is the technology.

Another point of relevance for social investment policy may be mentioned. If innovations produce externalities, because they show the way to imitators, then education—by its stimulation of innovation—also yields externalities. Hence, the way of viewing the role of education in economic growth set forth here seems to indicate another possible source of a divergence between the private and social rate of return to education.

Finally, the connection between education and growth which we have discussed has a significant implication for the proper analysis of economic growth. Our view suggests that the usual, straightforward insertion of some index of educational attainment in the production function may constitute a gross misspecification of the relation between education and the dynamics of production.

DISCUSSION

EDWARD F. DENISON: Professors Griliches and Jorgenson presented more material than they could adequately explain within their time allocation and more than I can discuss within mine. I must omit merited praise for originality. I ignore several questions of general methodology. I can only assert, without explanation, that analysis of gross instead of net product necessarily conveys a deceptive impression of the importance of capital in growth.

My remarks are confined to five modifications of capital input that the authors introduce. However, one or two of these would count as contributions of capital what I consider contributions of advances in knowledge. I shall mention these when I come to them, but a prefatory remark about the part to be assigned in growth analysis to advances in knowledge is necessary. Since advances in knowledge cannot increase national product without raising the marginal product of one or more factors of production, they of course disappear as a source of growth if an increase in a factor's marginal product resulting from the advance of knowledge is counted as an increase in the quantity of factor input. This must be what Jorgenson and Griliches mean when they suggest that with appropriate input measures they could dispense with advances in knowledge in explaining growth. But such a classification is uninformative and inconvenient; it does not correspond to the changes that have produced growth nor to the variables that would have to be influenced to accelerate it. Knowledge does advance, national income is higher than it would be if nothing had been learned since 1929, and there may be variables like the amount of research or like patent law that can influence the pace of advance.

The first three modifications of capital input are simple substitutions of one price index for another in deflating capital formation. The authors state that the three together reduce the growth rate of output per unit of input by .14.

1. O.B.E. deflators for private construction are rejected as biased because most of them price construction inputs and do not reflect changes in productivity in on-site construction work. Various people have examined this bias, and their conclusions as to its size are divergent. Substitution of the Public Roads index without radical reweighting of its components introduces a rather extreme adjustment. It appears dubious because operations required for building construction, which is the bulk of private construction, have so little in common with operations required for highway construction.

2. To deflate producers' durables expenditures the authors reject price indexes for producers' durables. They substitute consumers' durables prices. No reason is given for this choice.

An argument that producers' durables prices are biased upward must be entirely different from that used for construction. The price data available to O.B.E., though deficient and possibly significantly in error, measure prices, not input costs. Whether the deflator on balance can be assumed to have an upward bias rather than random error depends on the criterion adopted for judging appropriate behavior. I think there is no such presumption if the cri-

terion is the same as for other price indexes, including those for consumers' durables, the crucial characteristic being that a new or different product is counted as the same quantity of product as existing products with the same cost or selling price at a common date.

But the authors may want new types of capital goods to be equated with old types by their marginal products when they are put to use. If a price index conforming to this criterion did or could exist, its use would lead to classifying output gains from the invention of new or improved capital goods as contributions of capital rather than of advances in knowledge. Regardless of classification questions, there may be interest in estimating the amount by which this kind of technical progress raises the marginal product of capital. But substituting a price index of consumers' durables for a price index of producers' durables in no way furthers the pursuit of this will-of-the-wisp.

3. Griliches and Jorgenson deflate inventory change by the consumption price index. Their belief that because the implicit deflator for inventory change is erratic it is "highly implausible" is simply wrong. It is rightly erratic, because one year the change in inventories may consist mainly of wheat and the following year of half-built airplanes. The deflator for the stock of inventories is not erratic.

4. I commend the authors' addition of depreciation to net earnings in the weights used to combine components of the stock. Inclusion of depreciation in the weights raises the growth rate of capital input because the stock of short-lived equipment has increased more than the stocks of structures and inventories. The reweighting appropriately classifies as a contribution of capital the increasing double counting of investment in GNP. Inclusion of depreciation in the weights would, of course, be entirely wrong for analysis of the growth of net product. The authors also refer to taxes and capital gains, but I could not see how these changed their results. Dwellings, never mentioned, were apparently given either no weight or the same weight per dollar of stock as business structures. Dwellings should be included, but weighted with a lower net rate of return than business capital.

5. The largest adjustment stems from the important and ingenious article in which Foss used data on electric power consumption to show that power-driven equipment in manufacturing establishments was used one-third to one-half more hours per year in the mid-1950's than in 1929. Foss mentions at least four possible reasons. Improvement in management practice smoothed out production peaks during the year, better balanced equipment on the floor in accordance with needs, and extended continuous operation of machinery. Changes in equipment itself reduced downtime for repairs and also contributed to continuous operation. Shift work may have increased; it could not, however, explain the major part of the increase in machine hours that Foss found. Finally, the weight of industries in which continuous operations are important increased. This last cause would not contribute to growth since rates of return at a point in time are assumed equal in different industries regardless of machine hours.

Longer machine hours were not at all the consequence of increased saving and investment. They look to me like the result of capital-saving organization-

al and technical innovation, with perhaps a dash of improved quality of management thrown in. But however we classify their effects, it is interesting to isolate them. Griliches and Jorgenson did so by extending the Foss series to 1964, and treating an increase in average machine hours as equivalent to an equal percentage increase in capital stock. They did not eliminate the industry-shift effect, and they implicitly assumed that increased use of machinery neither required more expensive machinery nor more maintenance nor more rapid replacement; at least they mention no offsetting reductions in their estimates of the growth of capital input.

Power-driven machinery in manufacturing, to which the data refer, is so small a component of total capital input that an increase in the hours it is used would have only a minor effect on the growth rate. To assume an increase in hours worked by power-driven machinery in nonmanufacturing industries equal to that in manufacturing might be reasonable, although Foss gives enough information to permit a better estimate to be attempted. But the authors' estimate of a contribution of .45 to the growth rate results from letting the tail wag the dog. They assume, with no attempt at justification, that the average hours worked by inventories, by structures, and by all producers' durables, including such components as office furniture and restaurant equipment, increased in all industries in proportion to the increase in hours worked by manufacturing machinery driven by electric motors.

Let me now try to summarize. The authors provide no estimates of the contribution of capital to growth, despite careless wording at several points in the paper. But their procedures imply that their adjustments would raise this contribution from an unstated base by amounts that are even larger, stated in percentage points, than the magnitudes they do present, which refer to the net effect of their changes in both GNP and capital input upon productivity. I believe the paper conveys a misleadingly large impression of the contribution of capital because GNP rather than net product is analyzed, because at least the last of their adjustments probably is much too big, and because capital is credited with gains resulting not from saving and investment but from technical and organizational progress. I regard the paper's chief contributions as inclusion of depreciation in the weighting scheme for GNP analysis and the attempt to exploit the Foss breakthrough.

KAZUO SATO: Technical progress has been a sort of orphan in capital theory. Even in recent theoretical developments, technical progress has remained largely an exogenous variable. Some have attempted to introduce technical progress functions to rectify this state of affairs. Now Professor Shell attempts to make technical progress an endogenous variable by making the rate of technical progress an increasing function of current expenditure on inventive activity. A specific amount of scarce resources must be directed toward inventive activity if the stock of technology is to be expanded. The stocks of technology and physical capital enter into the production function in comparable capacities. The close analogy between

capital accumulation and augmentation of technological knowledge is the most interesting feature of the model.

Shell derives implications from his aggregative model with respect to decentralized and controlled economies. A decentralized, free enterprise economy which follows the law of motion prescribed by (4), (5), and (6) has a stationary equilibrium with a saddlepoint instability. Hence, this economy either explodes or decays. On the other hand, a centrally-planned economy with the maximization of discounted utilities as its target chooses an optimal program of capital accumulation and technical progress, the time path of which would approach another stationary point.

I have felt somewhat uneasy about these conclusions, though I agree with major premises of the model. As for the free enterprise economy, we have not seen a runaway growth process of the type predictable from Shell's model. There has always been some modest upper limit on the rate of technical progress in any historical environment. As for the planned economy, it sounds strange for any planning board to accept a stationary state as its optimum, in particular when foreign economies continue to pursue a growth policy, either planned or unplanned.

Let us have a careful look at some essential parts of Shell's model; namely, his hypothesis on the generation of technical progress and the character of technical progress.

For the former, it is clear that Shell has in mind a mechanism of accelerating knowledge expansion. Equation (5) shows that the growth rate of A is given by

$$\frac{A}{A} = \sigma\alpha f(k) - \rho.$$

With continuing capital deepening, technology grows at an increasing rate without any upper bounds because $f(\infty) = \infty$ by assumption. If this view is taken, we need not worry about the specter of stagnation due to the exhaustion of new ideas. A more correct formulation must be

$$A = \sigma\alpha A - \rho A.$$

What it states is that $100\alpha\%$ of total output spent on inventive activity would shift the production function by the amount equal to the right-hand side of the equation. σ is the efficiency of current expenditure on inventive activity. Though Shell takes σ as a constant, it is presumably a function of α and A. More expenditure on inventive activity may yield a larger shift of the production function only with diminishing rates in the short run. Hence, $\partial\sigma/\partial\alpha < 0$. The effect of A on σ is less clear, but we may conjecture that a rise in A, associated with the general improvement in education, tends to raise σ, probably with an upper limit on it for fairly high levels of technology. It is necessary here to consider the cost-benefit comparison of inventive activity even if it may be undertaken exclusively by the public sector. If the benefit is less than cost, economic welfare is higher with no in-

ventive activity at all. Assume a stationary state: one dollar spent now on inventive activity would shift the production function permanently by σ\$. The discounted present value of this gain less depreciation is equal to $\sigma[1/\delta-\rho]$, which must exceed one for inventive activity to be economical. This condition determines the feasible limit of α in the short run because of the assumption of diminishing returns. If the effect of A on σ is positive, the limit on α is smaller, the lower the level of technology is. If the maximum feasible value of α is less than ρ/σ, technology would in fact retrogress. Thus, low A, low σ, and low α tend to reinforce themselves; a backward economy cannot escape from its underdeveloped state without strong technical aid.

Even with this modification, however, the stationary equilibrium is still unstable though the rate of technical progress is now bounded. Let us recall now the following property of a growth model: there is a stable balanced-growth path if and only if technical progress is Harrod-neutral or labor-augmenting. In Shell's case of a decentralized economy, we can easily see that the assumption of Hicks-neutral technical progress is responsible for its explosive property. If technical progress is purely labor-augmenting, there exists no stationary solution (because of its overdeterminateness). Instead, his stylized economy would approach a balanced growth path. Is there any mechanism which ensures such a transformation?

With respect to a controlled economy, Shell prescribes an optimal program which makes the stocks of technology and physical capital approach some limiting values. I suspect that this proposition breaks down once the utility function takes into explicit account the negative influence of more advanced economies' consumption levels. If income levels abroad continue to rise, is there any sane planner who can deny continuing growth and propose a stationary income level as a target? More serious from a technical point of view is that the entire result depends on the character of technical progress. Here again we find that if technical progress is Harrod-neutral, there is a balanced-growth state in which the stocks of technical knowledge and physical capital grow at the same rate and which satisfies the maximizing conditions (11)–(14) with constant equilibrium prices. In this case, there is no stationary solution. (Let the production function be $g(A, k) = A\phi(k/A)$. As both g_A and g_k are now expressed in terms of k/A, we can readily see that the stationary solution from (11) and (12) is now overdeterminate.) It is clear that the maximand should be higher if the economy keeps on growing than if it converges to a stationary state, however high the latter level may be. If the planning board can control the degree of bias of technical progress, it should definitely encourage to develop labor-augmenting technology.

Capital theory has had not much to say on how technical progress is generated within the economy. It has much less to say on how the bias of technical progress is determined. Is it entirely technological, or can it be influenced by deliberate economic decisions? Shell's analytical findings depend essentially on this aspect of technology. Hence, I completely agree with Shell's concluding remarks on these points.

JACK HIRSHLEIFER: As a number of economists have pointed out, the concept of a "golden age"—in which current ouput and capital grow steadily at the same proportionate rate over time—is a generalization of the familiar idea of the stationary state, in which these growth rates are zero. The great appeal of the generalization is that, viewed from a certain perspective, the Western world in recent centuries has indeed experienced something like a steady rate of proportionate growth. It is therefore natural to employ golden-age models in dealing with problems of a long-range nature about technical progress and capital structure.

If, however, we consider a somewhat longer sweep of history, we may well conclude that models of steady progress represent a naïvely optimistic view of the prospects for mankind. What has become of fabled Babylon, of ancient Egypt, Greece, and Rome? Leaving antiquity aside, the Black Death of the mid-fourteenth century, the decline of Spain from the height achieved in the sixteenth century, and the cessation and reversal of economic advance in Ireland after 1850, confirm in a variety of ways that progress can indeed be forced to halt. And looking into the future, the rosy pictures of continued growth that we project today all lie under the threat posed by the even more dramatic advance in the powers of destruction. Viewing the problem historically, then, progress may seem an ambitious goal indeed. We may be doing well just to survive and to conserve our heritage.

These somewhat melancholy reflections were set in motion by reading the Nelson-Phelps paper, my assignment for this session, about the time of the great power blackout of this year. The central point of the Nelson-Phelps paper is that education promotes growth, not only or mainly by amplifying the capacity of ordinary labor, but rather by accelerating the rate at which entrepreneurs adapt technology-in-use to the best-practice-technology. Best-practice-technology, it is assumed, advances autonomously through theoretical researches and invention. Thus, "education speeds the process of technological diffusion." The rationale is that the educated entrepreneur is better able to discriminate between promising and unpromising ideas. He will therefore lead in the adoption of good new practice; the less well-educated entrepreneur will find it more prudent to wait and observe the results before acting himself.

The appeal of these arguments, for me, was somewhat shaken by the events of November 9. On that date, you will recall, we experienced the collapse of a system embodying what was certainly regarded as "best-practice-technology" in electricity production and distribution. Only a few backward enterprises, presumably run by undereducated managers, kept their inefficient operations going.

One might be inclined to infer from this experience that inaction due to ignorance may have a useful social function! There may be considerable peril, in a world of uncertainty, from too fast a rate of technological diffusion. The clearest example, perhaps, is in the adoption of new drugs: here a considerable waiting period is called for, so that undesirable side-effects upon experimental subjects will have time to reveal themselves.

But rather than give ignorance any credit, I would prefer to argue that wise

management has two functions: the progressive and the conservative. The merely ignorant manager may seem conservative because, as Nelson and Phelps indicate, he finds it prudent to resist change. In effect, for him the subjective variance for the outcome of a new proposal swamps any expected gain. But by the same token, having learned only from limited personal experience, the uninformed manager tends to have excessive confidence, too small a subjective variance, as to the possible outcome of present policies. (Hearing of the blackout in the northeast, such a person is likely to say, "It couldn't happen to me!") It is precisely the role of education to provide vicarious experience of a broader world than the individual can personally encounter. Education thus presents to the mind alternatives, of environment and of policy; it will suggest opportunities for progress, but also hazards against which protection is required.

The above can be summarized in two conjectured hypotheses. First, in analogy with the lag between technology-in-use and best-practice-technology, there is a gap between the actual and the appropriate provision for the unusual or rarely experienced event. (The clearest instance today, certainly, is the almost total failure of individuals and societies to adapt to the hazard of nuclear war). And second, since ignorance is at the heart of both types of failure, we can expect education to narrow both the progress lag and the protection gap. As one example, it is not surprising that the Jews—who over their history have placed extraordinary emphasis upon education, the formation of human capital (which is, of course, the least expropriable form of wealth)—have been champions at survival through every kind of hazard, as well as highly successful innovative entrepreneurs.

To close these thoughts on a modestly optimistic note, although the record of human existence does show the *ex ante* failure to adapt to hazard, the gap in protection of which I have been speaking, it also reveals a rather remarkable *ex post* ability to adapt to the new conditions generated once the blow actually falls. A variety of historical studies with which I have been concerned have confirmed the impressive capability of human societies to recover from disasters, which John Stuart Mill had observed and commented upon. In this resilience to shock, it is reasonable to assign a considerable role to the relative invulnerability of the stock of knowledge and to the enlarged scope for application of informed intelligence brought into being by the catastrophe itself. This enhanced adaptability constitutes a hidden benefit, both private and social, of human capital.

ECONOMIC DEVELOPMENT: ADVANCED TECHNOLOGY FOR POOR COUNTRIES

TRANSPORT TECHNOLOGIES FOR DEVELOPING COUNTRIES

By JOHN R. MEYER
Harvard University

The problems of picking appropriate factor proportions and a suitably modern or ancient technology for productive operations in less developed countries have been extensively discussed. The list of those participating is impressive: Kuznets, Hayek, Gerschenkron, Kindleberger, Hirschman, and Mahalanobis, to mention only a few. In abbreviated form the issue has been stated as that of "labor intensive versus up-to-date technology" [4, Chap. 10]. It is, of course, conceivable, and in a few cases perhaps even practically possible, to use relatively more labor advantageously with a modern technology. Also, an older piece of capital equipment may not necessarily embody an outdated technology, may little affect labor requirements, and may be worthy of consideration mainly because of savings in capital cost. Similarly, a useful distinction can be drawn between the problems of choosing between industries with different capital requirements and the problem of choosing between more or less capital-oriented technologies for performing a specific industrial or other economic function. In short, the problem is considerably more complex than choosing between a labor intensive or an up-to-date technology. At a minimum, it would seem appropriate to distinguish between: (1) old versus new technologies; (2) old versus new equipment; (3) labor-oriented versus capital-oriented industries; and (4) labor-oriented versus capital-oriented technologies for a given industry.

All except the third of these issues would appear relevant for transport planning in less developed countries. Even outside of transport, the importance of the third or "industry choice" issue can easily be exaggerated for countries that are truly in their initial stages of development. For such countries the planning choices are usually quite limited: almost any conceivable or sensible plan will include some considerable emphasis on development of agriculture, transportation, construction, and public utilities (not to mention education and other social services). Furthermore, if the country is endowed with good natural resources, there are usually compelling reasons for developing such

natural resources as a means of earning badly needed foreign exchange. At any rate, most (though by no means all) less developed countries need more transportation capacity if they are to develop economically. The questions, therefore, are not whether to provide more transport, but how much and how.

Finding answers to these questions quite clearly involves both engineering and economics or issues very commonly formulated today under the headings of "operations research" or "systems analyses." Furthermore, a consideration of the engineering and economic details strongly suggests that it is difficult to generalize the findings. In transportation, at least, some support can be found for any and all of the general positions that have previously been assumed on these issues of "labor intensity versus up-to-date technology." Examples and counterexamples abound.

In some applications, very modern technology would indeed seem to be justified—technology in some cases beyond the normal range of existing commercial application in the industrialized countries. Thus, helicopter and vertical (or, more accurately, short-distance) take-off aircraft have very real applicability in certain less developed countries as devices for opening up new territory or uniting geographic areas separated by difficult water or other geographical barriers [5]. The role of any relatively "exotic" technology, however, is likely to be limited to an initial period until more permanent and less costly conventional transportation, usually on the ground, becomes available. There is, of course, nothing startlingly new about this: it has been recognized for some while in Alaska, the Canadian North, and more recently in Algeria, Colombia, Pakistan, and Peru, among other places.

Air transport, in fact, generally provides strong support for those who adhere to the position that a modern technology is best for less developed countries [5] [7]. Air transport of a utilitarian type often may have a greater relative role to play as a basic passenger mode. This will be particularly true if the distance between major cities is relatively great or the cities are separated by rugged geographic barriers, as in Colombia, for example. Furthermore, if a decision has been made to use air transport in some application, it very often is advantageous for a less developed country to use modern jets rather than used piston aircraft. This should be particularly true as the new generation of two-engine jets become more available. Because of much higher speed and simple maintenance which permits more hours of daily utilization, a modern jet normally can produce many more seat miles of transportation for a given dollar of initial investment than older propeller driven aircraft were able to do when new. Of course, if the propeller craft are available today at heavily depreciated prices, the capital

cost advantage may shift to the used propeller plane. However, any capital cost saving on depreciated piston aircraft is likely to be so small as to be offset by other savings. For example, the simpler maintenance of turbine engines is a particular source of appeal and cost reductions in less developed countries. Furthermore, while flight crew personnel required for aircraft operation are normally available at lower salaries in less developed countries than in Europe or North America, these salaries are still not inconsequential, so that laborsaving remains attractive. In short, jet aircraft appear to be thoroughly dominant in a technological sense. Piston craft will be attractive in less developed countries only if opportunity costs on capital and foreign exchange are extremely high (which, of course, they sometimes can be) or if the route structure has a very low density and many short hops. (Actually, two-engine planes of post-World War II vintage reequipped with turbo-prop engines—a bit of old and new—can be the most economic in such circumstances.)

In the important road versus rail choice, by contrast, the adherents of a more labor intensive approach are likely to receive comfort. However, to the extent trucking is considered the more up-to-date as well as labor intensive technology, there is some support for both views. Specifically, road transport would seem to have a potentially greater economic role (relative to railroads) in less developed countries than in Europe or North America [3] [5] [6]. The choice, though, is only very partially explained by the labor inputs required. Any advantage of trucks or buses over railroads in less developed countries largely derives from two factors. First, low volume densities commonly occur on traffic corridors in less developed countries, particularly in South America and Africa, and highway vehicles tend to be more economic than railroads under such circumstances. Second, railroading generally requires much more centralized and sophisticated management and control than trucking, and the required sophistication is not always found in the less developed countries. As one major consequence, the quality of rail services in some parts of Asia, Africa, and South America can be markedly inferior to the quality of such services available in Europe or North America, while the quality of truck services usually is not. Slow deliveries, of course, increase inventory costs which, in turn, further increase the total relative capital requirements of a rail system.

Needless to say, roads will not supplant all need for railroads in less developed countries. Where heavy volumes of bulk commodities, particularly minerals, are to be moved and no water transportation is available, the railroad almost invariably will be the most economical choice. For these bulk commodities, service considerations usually do not loom large. Furthermore, if the country is very dependent upon

imports for petroleum and is short of foreign exchange, railroads can have an attraction over highways. This would be particularly true if the country has coal and its railroads are steam driven or has considerable hydroelectric power and an electrified rail system. Any foreign exchange advantage of railroads, however, can easily be exaggerated even in circumstances where the country has no indigenous petroleum reserves. It is usually easier to conceive of a country developing import substitution in the supply of vehicles and parts for a highway system than for rail.

Many intriguing possibilities for factor and technological substitution occur when contemplating different highway transport systems [2]. For example, it is tempting to conclude a priori that American and European highway engineers have been entirely too prone to apply the engineering and specifications developed within the context of North American and European experience to less developed economies. As Kindleberger has aptly put it: "All too frequently it is the technologically culture-bound expert from the developed country, rather than the politicians of the underdeveloped, who need . . . an . . . understanding of elementary economics" [4, p. 183]. To be specific, many (including myself) have challenged whether less developed countries really need roads engineered to modern North American standards, say with twelve-foot lanes, six- to ten-inch pavements, six- to ten-foot shoulders, etc.

On closer inspection, however, it would appear that highway engineers in applying the standards and specifications of Europe and North America to less developed countries may well have been doing approximately the right thing—albeit sometimes for the wrong reasons. (This is not to deny that in many instances they may have overbuilt, at least partially or temporarily.) Whenever volumes achieve a rather nominal level, reasonably good roads seem to be justified in less developed countries for much the same reason as modern airplanes. Good roads permit higher operating speeds, and thus conserve on vehicular capital required per unit of output. Also, they reduce tire wear, vehicular maintenance, and the rate of physical depreciation of the vehicle as well as saving on operating labor. Furthermore, as often noted, good roads reduce road maintenance requirements, and road maintenance can be a somewhat difficult function to organize in some less developed countries.

In general, possibilities for achieving economies in highway transportation in less developed countries would seem to lie more with the vehicles than with the highways. Specifically, there has been only limited transfer of used cars, trucks, and buses from Europe and North America to the less developed countries of the world. This contrasts

with the rather well-developed tradition of sending any and almost all used airplanes, ferry boats, ships, and, to a lesser extent, railroad and urban transit equipment. The logic of transferring more used vehicles would be to transfer automobiles and trucks from high to low labor cost economies at some point in their life when the labor required for maintenance becomes relatively substantial. Buses, trucks, and autos are examples *par excellence* of technologies that have had relatively few recent innovations that effectuate important capital savings and thus would appear to be particularly suitable for transfer to less developed countries when in a used condition. To put the matter differently, most recent improvements in commerical highway vehicles, particularly in the United States, have been mainly aimed at saving labor.

For those who would object that vehicular maintenance requires a degree of skill not commonly encountered in less developed countries, it should be observed that many of these countries do an uncommonly good job of such maintenance today. Indeed, a considerable ability to make "creative adaptations" in European and American vehicles to make them more suitable for Asian, African, and South American conditions has been displayed. Of course, spare parts may pose a problem. But as a rule the manufacture of spare parts should be considerably simpler than manufacture or fabrication of complete automobiles, as many of the larger less developed countries now attempt. At a minimum, specialized rehabilitation of vehicles and manufacture of some spare parts would seem to be considerably more industrially feasible for the smaller of the less developed countries than development of a complete automobile industry. Furthermore, cannibalization of scrapped vehicles for spare parts would be marginally more attractive in less developed countries than in North America because of lower labor costs; thus, as the less developed countries accumulated a larger stock of vehicles the spare parts problem would be partially self-correcting. Indeed, by making cannibalization marginally more attractive, some reduction in the number of very old vehicles might be induced which, in turn, would reduce overall fleet maintenance costs.

Another argument commonly tendered against employment of used highway vehicles in less developed countries is the possibility that a considerable expansion of highway fleets would generate an intolerable increase in petroleum imports in some cases. This would seem to be a general argument against use of any highway vehicles in such countries or an argument for purchase of used European rather than American vehicles. Actually, employment of used rather than new vehicles should have only a very slight effect upon total fuel consumption per unit of output. Furthermore, used vehicles might even reduce fuel

needs by reducing average operating speeds and by placing reliance on a fleet with less total horsepower. Similarly, if cars are not manufactured within the country and must be imported, the transport costs per vehicle should not be greatly affected by substituting used for new vehicles and, if anything, might be slightly reduced. The transport costs to be amortized per unit of output would, though, normally increase.

At any rate, an initial assessment of the cost relationships (though one that could use refinement) strongly suggests that for a given expenditure of foreign exchange on used vehicles a considerably greater total potential volume of transportation output often should be purchasable. The exact extent of the advantage will vary, of course, from one area to another and will depend on the level of ocean freight rates, the existing indigenous capability for vehicle parts manufacture, and the extent to which existing maintenance practices on new vehicles reduce maintenance requirements as vehicles age. Given that a certain level of transport capability is to be achieved, the essential choice is between large initial expenditures of foreign exchange (on new vehicles or new auto manufacturing machinery) with lower vehicular maintenance costs later, or smaller initial outlays of foreign exchange (for used vehicles) but with higher subsequent outlays for maintenance. The high discount rates commonly recommended for evaluating foreign exchange expenditures by the less developed countries implies an advantage for the timing of outlays associated with the second of these two alternatives. Furthermore, to the extent that auto manufacture is marked by more scale economies than parts manufacture and vehicle rehabilitation, some advantage, at least initially, should reside with used vehicles.

The question arises, of course, of why this substitution of used for new vehicles has not already occurred on a greater scale if these economies are indeed achievable. The answer would appear to lie in a number of considerations, the most important of which seems to be the naure of excise taxes and import quotas imposed by the less developed countries. In large measure, these policies discriminate against used vehicles. Since a common justification for the quotas and taxes is conservation of foreign exchange, perhaps one way out of the tangle is for the United States to inaugurate a "cars for peace" program parallel to its "food for peace" program. The objective would be to accept payment for used cars in soft currency. As a by-product, disposal of some of our used vehicles would help reduce the flow over time to our junkyards—eyesores which have attracted much attention of late.

The potential returns from substituting used for new vehicles in less

developed countries could be substantial. If we concentrate attention on those countries of South America, Asia, and Africa that would seem to be most in need of help in developing their vehicular capacity and least likely or least inclined to develop internal production of vehicles, we find that the available supply of vehicles is shockingly small. Certainly it is less than will be required if these countries are to make full or best use of highways now being constructed or planned. More individual highway transport also would seem useful in achieving the expansion of market agriculture now sought in many development plans.

To be specific, the total automobile population of Africa, Asia, and Latin America (exclusive of Communist countries, the Union of South Africa, Rhodesia, Israel, Japan, Brazil, Argentina, and Mexico) can be estimated as little over 3.5 million [1] [5]. The equivalent figure for trucks and buses is approximately 2.0 million. By way of contrast, total vehicular registrations in the United States today are just short of 90.0 million. The United States scraps over 6.0 million vehicles per year of which approximately 800,000 are trucks and buses and somewhat under one million more are station wagons that might be expected to have considerable value for commercial transportation in less developed countries [1]. Of course, some four-door sedans might also be useful (as taxis, jitneys or for light-freight hauling). On a conservative estimate, therefore, today's scrappage rate in the United States would be sufficient to almost double the present stock of commercial vehicles in these less developed countries in one year.

The major point of this paper, however, is not to recommend any specific solution to the transport problems of the less developed countries. Indeed, it seems fairly safe that one cannot recommend one universal panacea that will meet the needs of all of these countries equally well. A fairly detailed analysis, involving both engineering and economics and incorporating an evaluation of the total system effects, seems required in each case. That detail, moreover, would seem to be a prerequisite to intelligently analyzing the questions so often raised about "labor intensity versus up-to-date technology." Actually, the very dichotomization of "labor intensive versus up-to-date" may be rather irrelevant in many instances. In transport, "capital saving" rather than "up-to-date" or "labor intensive" would seem to be the key consideration, though even this generalization is subject to significant exceptions. Up-to-date equipment generally will be attractive if recent improvements have led to considerable capital saving per unit of output, as well as laborsaving or service improvements. By contrast, if recent technological change has not induced any significant changes in

capital requirements per unit of output, used or otherwise, less up-to-date equipment may be the proper economic choice for a less developed country.

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THE CAPACITY TO ASSIMILATE AN ADVANCED TECHNOLOGY

By Robert Solo
Princeton University

We observe one set of societies where the norm of productivity is very low compared to that of another set. The former have been called "developing," the latter "advanced." The complex of techniques, including the equipment in use and the organization of operations in the advanced which, presumably, accounts for their high productivity, will be termed "advanced technology." The question is, what determines the capacity of the developing society for incorporating this advanced technology into its own operations and, thereby, itself achieving high productivity?

It may simply be a question of financing investment. There need not be the resources to cover the high initial costs of reorganizing operations. Foreign exchange to purchase key producer durables may be lacking. Indeed the provision of investable resources through foreign loans or grants or through local savings to cover the costs of transformation seems hitherto to have been the principal concern of development economists and agencies who have taken their cue from post-Keynesian growth models. But simply to have the resources available for investment is not a guarantee of development. Resources must be matched by the opportunity to use them in transforming operations—an opportunity which will depend in part on the social capacity to assimilate advanced technology.

Nontransferable Components of Advanced Technology

One reason why advanced technologies are not used in low-productivity economies may be because they are not usable there. They have evolved in and consequently are adapted to a social and a physical environment which differs significantly from that of developing societies. On account of these differences, their use in the developing society will sometimes be uneconomic and technically retrogressive.

The most evident difference in the contexts of technical operations between low- and high-productivity economies is in the physical environment. Characteristically, advanced technologies have developed in temperate climates while low productivity societies are found, for the most part, in tropical and subtropical zones. For that reason different vegetation, different fish and fowl, different animals flourish in each. The structure of the soils and the practices appropriate to soil conser-

vation will differ. There are different crops, differently cultivated, with different problems in their preservation and processing. Different diseases attack men, animals, and plants. When the diseases are the same, their vectors are likely to differ. Correspondingly, it is not possible to transfer the technologies and the sciences of agriculture, horticulture, animal husbandry, medicine and public health developed in temperature conditions directly to low-productivity societies in tropical and subtropical zones. As in the physical, similarly differences in the social and economic context of technical operations may bar the direct transference of advanced technologies.

Inasmuch as advanced technologies are not directly transferable, their assimilation requires social action and social competence in: (1) recognizing what can be transferred directly and what might be adapted for transference, then in (2) adapting advanced technologies for application in the low-productivity economy, or in (3) restructuring the context of operations to provide a more hospitable environment for the advanced technology.

These three—the capacity to recognize the feasibility of attempting directly to transfer or adapt advanced technology, the capacity to adapt technology to the physical, social and economic context, and the capacity to adapt social and economic conditions to the requisites of technology—together constitute the capacity to assimilate advanced technologies.

Adapting the Advanced Technologies

Consider the adaptation of advanced technologies to the context of operations in low-productivity economies—which can be understood as taking place at three different levels.

At the simplest level advanced technologies are adapted on the spot by adjusting or modifying machines which are in use elsewhere or processes and techniques which are practiced elsewhere to a particular need or circumstance. However, a machine or technique only expresses and partially embodies a corpus of knowledge. The mastery of that knowledge and its use to design the appropriate mechanisms and techniques can be understood also as an adaptation of advanced technology and one that conveys a wider range and a greater power of assimilation than is possible through on the spot adjustments or modifications of already designed and practiced techniques.

Advanced technologies are based, in considerable part, on the knowledge of science. Hence, to exploit the corpus of information upon which advanced technology rests requires the competence of the scientist or the science-trained engineer who, in this instance, must also be deeply familiar with the circumstances and needs of a low-productivity

society. But even the mastery of the knowledge of technology by those who also understand the circumstances of the low-productivity economy need not suffice.

The knowledge underlying advanced technology is neither infinite nor static. It has itself been produced and is continuously being produced through experience and research. Since it has chiefly evolved under the circumstances of high-productivity societies in the effort to solve the problems of high-productivity societies, correspondingly it will be less applicable, less adequate to solve the problems of low-productivity societies or to design a technology suited to their circumstances.

Inasmuch as existing knowledge does not suffice, what must be acquired is the capacity to produce knowledge that does. It is the problem-solving, information-producing apparati that must be mastered, adapted, and applied. Science itself must be development-oriented. To adapt and to focus the analytic concepts of science and its research method on those problems which arise at the nexus of need and circumstances in a developing society means to exploit the most dynamic component of advanced technology. It is to adapt advanced technology's very mechanism for adaptation and for further advance.

An instance of development-oriented science, familiar to this audience, is that of development economics. The conventional social technology for economic control and policy determination could not be directly transferred to low-productivity societies. And the accumulation of concept, theory, and information—constituting economics—underlying that technology was likewise unsuited. Therefore, the research focus of our discipline was redirected and its problem-solving apparatus adapted so that it might produce the needed knowledge on the basis of which viable policies and controls for economic development can be designed. This suggests another fact. The capacity to adapt advanced technology to the requisites of low-productivity societies need not be wholly indigenous to those societies. Nor can it be entirely outside, since, to be effective, that capacity at each of its three levels must be integrated into the system of choice and power.

To recapitulate, with an illustration of the three levels of adaptation, take the hypothetical need to control insects on a tropical plantation. There might be equipment in use elsewhere which can be used for spraying insecticides, but it would have to be adapted on the spot to climate and terrain, to the shape of the infested plant, and to the locus of its infestation or to the skill of indigenous labor. Such on the spot adjustment of already designed equipment would be adaptation at the first level. It might then be necessary to compound and produce an appropriate insecticide which would require a knowledge of the chemis-

try of pesticides and of the habits and vulnerabilities of the insect to be controlled. To design this new component of technology by reference to existing knowledge would be a second-level adaptation. And if, as is frequently the case, not enough is yet known about the habits and vulnerabilities of the pest or the pesticidal efficacy of locally available materials or the possibilities of control through the use of the pest's own parasites, then a conceptual and analytic apparatus of science must be turned to search for the new information which is required. This development-orientation of science is the third level in the adaptation of advanced technology.

The greater the differences in the context of technical operations, the more difficult it will be to adapt a technology that has evolved in a high-productivity society to the needs and circumstances of a low-productivity society. This may account for the seemingly paradoxical emphasis on industrial development by predominantly agricultural low-productivity societies; since the ineradicable differences in the natural parameters of agriculture (and also the very deep differences in the social circumstances of agriculture) in low-productivity vis-à-vis high-productivity societies probably makes it more difficult to adapt advanced agricultural technologies than to adapt advanced industrial technologies for assimilation.

Whether or not it will be economic to use a particular technology will depend on the cost pattern of materials, of equipment, of energy sources, of the various skills of labor, and of access to markets. Differences in cost patterns will check the transference of advanced technologies. Particularly significant are cost differences which reflect the relative lack of skilled labor in low-productivity economies. An adaptation of advanced technology to this lack may call for the use of a less costly accumulation of producer's durables per unit of output, i.e., for "low capital intensity" techniques, than is usual in high-productivity economies—or it may call for exactly the opposite. Thus Norbert Weiner, while working in India, observing the lack of ordinary industrial skills there, quite correctly deduced that this would make it impossible to transfer the technology practiced in high-productivity societies directly to India, and that the costs of training the masses to the requisite skills would be very great. He also observed that there existed in India a number of highly trained and very able mathematicians and scientists who might be mobilized to design and introduce fully automated industrial processes where the need for ordinary industrial skills would be minimal. Therefore, Weiner supposed that the opportunity costs of automation in India would be less than in, say, the United States and that it would be more economic to take the path of high automation in a low-productivity society such as India than it

would be in a high-productivity society such as the United States. I am told that Kaleki took the same position in regard to economic development in Poland. I doubt that a conclusive general case could be made that technology adapted for assimilation into low-productivity economies should be more or less capital intensive than the norm. Suffice that since the pattern of comparative costs is likely to be different, the transferred technology should in some way be reshaped. Perhaps the same could be said for many transfers of technology between high-productivity societies or even for transfers between sectors in the same economy.

Social and Economic Organization as a Variable

An advanced technology must be adapted also to prevailing forms of social and economic organization. Or conversely, forms of social and economic organization might be changed to exploit the potential benefits of an advanced technology. For example, data processing and other computer-based techniques which constitute a spectacular component in the technology of advanced societies are significant more or less specifically as a support to the planning and control functions of very large organizations. Where decision-taking is decentralized and operations are carried on by large numbers of discrete entities in competitive markets, even as in the agricultural or in the small business sector of the American economy, these elements of technology are not significant. Therefore the assimilation of these components of advanced technology presumes the existence or at least the desirability of large organizations. The capacity to control a multitude of activities under centralized political direction or through the quasi-voluntary association of great corporate enterprise itself requires particular cultural proclivities.

Creating a Hospitable Context for Advanced Technology

Nor is it only the adaptation of technology to a particular context of operations which is at issue. The context may be adapted to the available technology. Thus new and complex techniques using concrete as a material in residential construction have been developed for certain low-productivity economies, because the lack of skilled craftsmen precluded the technology of construction as it is conventionally practiced in high-productivity economies. Alternatively, carpenters and bricklayers could have been trained. New and complex techniques of aerial survey for mapping and for the inventorying of natural and social resources have been developed more or less specifically for the low-productivity economies. Alternatively, roads and internal transportation facilities could have been built which would have made it possible

to use the survey techniques conventional in high-productivity economies. Not only can advanced technology be adapted to the environment of the low-productivity economy; the environment of the low-productivity economy can be adapted to facilitate the transference of advanced technologies. Certainly efforts are made thus to reshape the context of operations, particularly in the development of the infrastructure in the building of roads, harbors, airfields, and in making available transport facilities, in providing protection against contagion, violence, theft, fraud, flood, hurricane, in offering the services of banks, and equity markets, etc. Especially relevant to the capacity to adapt technology or to adapt to advanced technologies are those science-based components of the infrastructure which enable decision-makers to test, measure, quantify the variables related to economic choice, or precisely to determine and precisely to specify the performance characteristics and requirements of materials, products, structures, or more accurately to forecast the context or consequences of decision and policy—thereby raising the capacity to control and to innovate in any sphere of technology.

The Capacity to Assimilate and the Motivation to Innovate

The environment may be hospitable to the incorporation of an advanced technology. The advanced technology may be transferable or there may be available in the developing society all the skills required to adapt it for transference. Yet it may fail to be incorporated into operations. Nor are such failures to realize upon opportunities for technological progress confined to low-productivity economies. They occur everywhere and are part of the story of nearly every ultimately successful innovation. For transformation to an advanced (or to any new) technology, there must not only be the capacity to transform, which has been the concern of this paper, but also there must be a gearing-together of the competence to evaluate the technical feasibility and economic benefits of change with the power to effectuate change and with the motivation to transform an existing organization of operations, including sometimes a structure of privilege.

The Structure of Cognition

A society's capacity to adapt itself to the requisites of advanced technology and to adapt the advanced technology to its own circumstances and objectives, as well as its capacity to innovate, will depend in part on the intellectual skills, the acquired knowledge and know-how, the problem-solving competencies—in a word, on the cognitions possessed by those who constitute that society.

The required structure of cognition might be conceived as a pyramid. At its base, pervading the whole society, would be simply a sense

of the machine, of its logic, of its manipulability, of its limits, of its demands upon its caretakers, of its values in extending the individual's power; i.e., a cognition of mechanism. Merging with and emerging out of this cognition of mechanism, carrying it into vocation and practice, would be the cognitions implicit in mechanical and technical skills. A mass cognition of mechanism at its base, a middle mass of mechanical and technical skills occupying the center part of the pyramid, gives any society its capacity to respond to the signals of technical leadership and to adapt to and to adjust an advanced technology around the whole periphery of its operations.

Merging with and emerging out of the middle mass of technical skills is another sort of cognition which comprehends the interrelation of machines, materials, labor, and information in processes of producing goods and services. This cognition of process is necessary to set in motion, to control, or further to transform an advanced technology.

And at the apex of the pyramid, interacting with the cognition of process, are the cognitions of science, of a development-oriented science, and a science-based development engineering. These are necessary in order to "apply" research method and the world accumulations of scientific knowledge to problems impeding technical advance, or to evaluate the feasibilities of transferring science-based technology or to adapt science-based technologies for assimilation.

This structure of cognitions is not the only precondition of development. For example, an enlightened political leadership or an aggressive entrepreneurship, as traditionally conceived, may also be needed. But the wisest, most benign political leadership and the slickest, quickest, wheeling-dealingest entrepreneurship will not produce a stream of technical transformations unless they act within this structure of cognition.

In Western Europe and the United States, the cognition of mechanism and the skills of mechanics and technicians and the cognition of process can be acquired—and for the most part are being acquired through daily observation—through apprenticeships, through *ad hoc* training or through managerial experience on the job—all outside the system of formal education. Formal education, inasmuch as it has had a functional objective, has produced the scientific and technical elites at the apex. But in low-productivity societies which have not yet crossed the threshold of industrialization, the mass cognition of mechanism, the skills of a middle mass of mechanics and technicians, and the essential cognition of process cannot be acquired spontaneously, outside the system of formal education. Rather the gigantic task of inculcating them needs to be planned and programmed. This suggests that a quite different policy and system of formal education is needed in developing societies than is traditional in the West.

NOTES ON INVENTION AND INNOVATION IN LESS DEVELOPED COUNTRIES

By RICHARD S. ECKAUS
Massachusetts Institute of Technology

I. *Introduction*

Invention and innovation in the less developed countries present particular problems for analysis because of the special characteristics of the organization of their economies, their resource proportions and their relations with more advanced economies and partly because of the intensity with which their economic growth is being pursued. In this paper I hope only to illustrate rather than resolve the difficulties in understanding the role of technical change and in making policy for it.

The distinction which I shall adopt between invention and innovation is between the processes by which new products or processes are created and the actual implementation of the new processes and production of the new products. I shall not try to maintain the distinction which has been made much of in the past, especially by Schumpeter, between the first use of a new technology and its subsequent imitation. In some instances that distinction may be profound but I think those cases are exceptional. The limited literature with which I am familiar on the sociology of innovation suggests to me that the distinction is usually a matter of degree and that the psychological and cultural barriers to imitation of a technological change are often no less significant than those to the first use.[1] The innovation-imitation distinction may also be given an economic interpretation in terms of the relative risks involved. It might be argued that except where monopolization is complete the first innovator creates a true externality which reduces risk: the knowledge of his success or failure. But success or failure is not always quickly identified, except in extreme cases, and even in the case of success in less developed countries neither the original innovator nor his potential imitators may be fully aware of how much is due to the new seed or new product, for example, or the special attention and favors of government officials.

The role of process innovation in directly increasing factor produc-

[1] Professor E. Hagen's *On the Theory of Social Change* (1962) has influenced my views, though I cannot find a particular supporting quotation. The frequent references to the difficulties of dispersing the technical changes demonstrated in pilot projects may also be adduced in partial support of this view.

tivity is only one of its important functions in the less developed areas. It is, however, that function in which economists have been most interested and which will be the main subject of this paper. Product innovations, by which new goods are created, offer a different set of analytical challenges including the one of rigorously maintaining the process-product innovation distinction itself. "New" goods in less developed countries may be "old" goods in the more advanced countries. Their production raises questions of the relative effectiveness of investment in import substituting versus export gaining industries which I do not wish to take up here.

Process innovations in the less developed countries, as in the more advanced, are generally taken to mean a change in "technology" with a corresponding change in factor productivity. Engineers know what a technology is and identify processes by the character of the physical and chemical transformations involved or the equipment which produces them. Economists on the other hand do not need to know about physical and chemical processes and identify technologies by the amounts of productive factors used and the associated outputs. The production function is simply a summary of all the alternative physically efficient combinations of productive factors which are necessary for different output levels. Because of the theoretical convenience, economists are fond of representing production functions by smooth and continuous analytical functions. Technical change substitutes for one set of factor inputs, which is required for a particular output, another set, which is superior in the sense that it uses less of at least one factor. It is often represented as a change in all the possible factor combinations associated with each possible level of output but need be only a change in a particular factor combination.

Economists have customarily distinguished the actions of firms in changing their input combinations because of changes in the relative prices of the inputs, i.e., factor substitution, from those changes due to innovation of new technologies. This distinction has its origins in the different sources of the causal factors. Changes in input prices are given a straightforward economic explanation in terms of changes in demand and supply conditions of the inputs. The deeper reasons for such changes are in turn the subject of some of the more fundamental analyses in economics. Even with constant factor prices, optimal input proportions can change with the scale of output unless the production function has the particular property of being homogeneous of the first degree. The ascription of this property to production relations is usually a matter of theoretical convenience rather than one for which there is a strong empirical basis.

Though the innovation-factor substitution distinction is certainly

significant, from an overall point of view it need not be important to an individual firm engaged in the choice of a new set of input and output proportions. The firm is concerned only with its revenues and costs. Perhaps it might be argued that there is greater uncertainty associated with innovation as compared to factor substitution which justifies the distinction at the level of the firm. But this need not be true at all for the imitators of the first innovator, and, depending on the circumstances, need not be true even for the first innovator. Factor substitution may also be exploration of relatively unknown territory for the individual firm, especially when it is associated with substantial changes in the overall level of its output.

At any moment a number of different factor combinations might simultaneously be actively in use in producing a given commodity even if all firms are behaving optimally with respect to their choice of techniques. This is because fixed capital, labor, and material inputs are often limited in their capacity to be adjusted to new factor combinations and "old" combinations may rationally continue to be used as long as the variable or current costs of the old are less than the total costs of the "new," all properly discounted. Thus it will ordinarily take some period of time for a new process innovation to become predominant and for a change in input prices to work itself through an economy. The speed of dispersion depends in part on the potential flexibility of the existing productive factors in entering into new input combinations. Disembodied technical change, which is ordinarily taken to mean that it is not uniquely associated with a particular type of capital, may or may not proceed more quickly than embodied innovations. Technologies are also embodied in labor skills and materials, and these too have fixed cost elements which should be ignored in calculating the current costs of the old technology but not in such calculations for the new one.

The above discussion restates the various types of changes which may lead to use of different input-output combinations in the production of already known goods and services: input substitution resulting from changes in their relative prices, a more efficient adjustment to given relative factor prices at preexisting output levels, the effects of changes in the scale of output with given factor prices or the innovation of a new technology. In turning to a discussion of technical change in less developed countries I shall try to observe these distinctions and ask whether there are special features of less developed countries which act to impede the achievement of the most effective input combinations.

There are economic as well as noneconomic theories of invention to account for the appearance of new technologies and new products. The

theories are partial and incomplete but nevertheless I believe they will provide some insights at a later point in this paper.

II. *Innovation in Less Developed Countries*

There are theories of the condition of the less developed countries in which technical change is not mentioned but these should not be taken as implying a subsidiary role for such change. Economic development is seldom envisaged as a scalar expansion of all productive factors and outputs in some original set of proportions. Thus, when the "vicious circle of poverty" or the need for "infrastructure" or the arguments about "balanced growth" are adduced, the omission of discussion of technical change is usually only a convenient device for the purpose of obtaining greater insight into the role of other factors. The omission is to some extent, however, a reflection of the paucity of our understanding and practical knowledge. For example, it is easy to criticize most of the various quantitative development planning models on the grounds of their omission of technical change. Yet, in many of the models there is in principle no barrier to the embodiment of changes in technical coefficients. The difficulty is in the lack of a theoretical and empirical basis to predict such changes.

We may make headway in appreciating the barriers to achievement of the most effective factor combinations in less developed countries by examining some of the customary assumptions, starting with that of rational profit maximization. There is an abundant literature with the theme that lack of development is characterized by the absence of purposive, profit maximizing rationality. This is often accompanied by the argument, as in some theories of the "rise of capitalism," that the appearance of such rationality will as a natural consequence lead to economic growth. Though the examination of situations characterized by the absence of profit maximizing rationality would be of general interest, the only object here is to indicate the possible effects on the technology used.

Irrationality may take the form of acceptance of new technologies simply because of a faith in modernity as well as an uncritical perseverance with traditional methods. This point has been made before. Though there is an underground of stories which are told of technological "mistakes," a judgment as to their quantitative significance is difficult. For example, on the one hand we have Professor Theodore Schultz, who argues that peasants in less developed countries have made a more or less optimal adjustment to their circumstances.[2] At the same time there is an abundant literature which argues that there are often tech-

[2] *Transforming Traditional Agriculture* (New Haven, 1964), Chap. 3.

nical changes easily within the reach of such peasants which would increase their output if only their psychological and cultural blockages could be removed.[3]

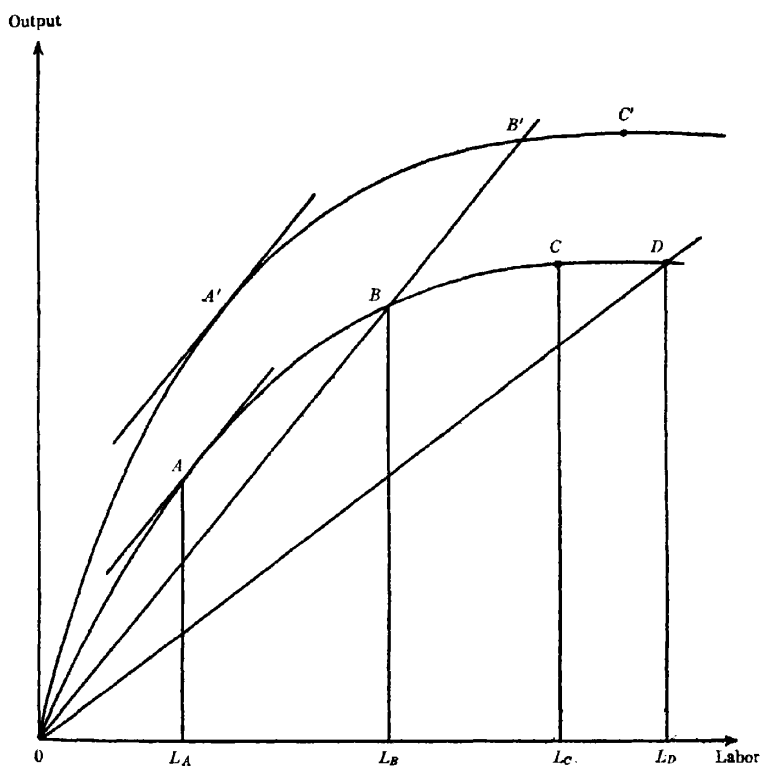
It is difficult in any case to distinguish the effects of irrationality from the absence of the customarily assumed profit maximizing motivations. In many of the less developed countries it would be unusual and socially not acceptable for a productive enterprise to be organized as a firm employing labor and other resources by impersonal contracts in order to maximize profits. The predominant form of social organization for work is often the extended family or the tribe or the government firm with no inclination to maximize its profits. A brief analysis of one such case will help to indicate some of the difficulties in identifying the sources of technical change and the reasons for the use of factors in different relative intensities. Suppose in Figure 1 the line *OABCD* represents the conventional total product curve for a family farm. In many countries the farm simply accommodates as many producers and consumers as there are in the extended family.[4] It is rational to treat the subsistence requirements of the available laborers as a fixed cost and to maximize the output which can be achieved with that labor.[5] If the family grows in a Malthusian way and subsistence requirements are indicated by the slope of the line *OB*, the size of the family will grow until L_B laborers are available and the family will produce at *B*.

Families of size less than L_B will be able to save and invest and in this way be able to shift the total product curve upward to, say, *OA'B'C'*. With perfect capital markets even the Malthusian family at *B* could always borrow, invest, and obtain more output but there is no reason to suppose such perfection in rural, subsistence farming. On the other hand, such behavior should not be surprising, and, in fact, there appears to be evidence that it does occur. It may be noted that families with a lower level of subsistence, as indicated by the slope *OD*, might expand until L_D amount of labor was available, even though the marginal productivity of labor beyond L_C is zero. In a family-organized, subsistence agriculture, it would not in fact be surprising to find farms at various points along such *OABCD* or *OA'B'C'D'* curves, since the ordinary competitive pressures would not exist.

[3] As a fairly typical example see Kusum Nair's *Blossoms in the Dust* (London, 1961).

[4] Though the particular implications to be drawn from the analysis have not been stressed before, I believe, it is related to that of Professor A. K. Sen, *Choice of Techniques* (Oxford, 1960), and recently, Professors J. Fei and G. Ranis, *Development of the Labor Surplus Economy* (Yale, 1964), as well as some of my own previous work.

[5] If the ratio of work force members to nonmembers is fixed, the discussion can be conducted, as here, in terms of the number of laborers in the work force and their subsistence defined to include the requirements for their proportionate share of those in the family who are not members of the working force. Otherwise it becomes slightly more complicated to make the simple point of the argument presented.

In contrast with this type of family organization of the firm, a profit maximizer in this situation who could hire labor at the subsistence rate would use only L_A amount of labor, operate at A, where the marginal product of labor equaled the subsistence wage, and make profits. Consider, further, the effect of vigorous, competitive, non-Malthusian entrepreneurship in such a sector. Since the use of inputs by rational, profit

maximizing employers is more efficient, their output would have lower costs and they could systematically displace the Malthusian family firms which have no cushion of surplus above subsistence. In the process, resources would be drawn out of family firms into the firms of the profit maximizing employers and a group of "landless laborers" would be created. Even families not at the extreme Malthusian limit may not be able to resist the superior competitive position of firms operating at A if the latter firms reduce prices to such an extent that the inefficient firms

cannot maintain their capital. The profit maximizer might even b
to hire "part-time" labor from family farms at less than subsistence
if their marginal productivity on these family farms is lower than s
tence. This, of course, depends on the ability of a part-time labo
maintain his associations with the family so as to obtain the rem
of his subsistence requirements.[6]

The point of this analysis is that an outside observer looking
industry composed of family firms and profit maximizing emp
would find a number of different factor intensities and factor pı
tivities in existence. It would not be hard to think of institution
havior in which such a varied pattern would be maintained for a
siderable period. In other situations more efficient factor combin
might slowly replace less efficient ones. The replacement may con
atively swiftly, however, with other social changes.[7] Such ch
might well appear to be technological innovation if one could n
below the surface of events when in fact they would represent en
ment of known and conventional technologies and considerable
innovation. On the other hand, technological innovations and
innovations might be closely associated. Process innovations
generate profit opportunities so much more attractive than thos
viously available as to change traditional patterns of behavic
product innovations which create new social positions can fac
new factor combinations which would have been barred by cust
traditional products.

The above analysis provides an example of the influence on
of technology of behavior which does not correspond to the custo
assumptions of rational profit maximization. Other cases could l
veloped at the other end of the spectrum of organization; e.g
modern, government-subsidized firm, staffed from the civil se
whose mode of operation is influenced as much by civil service cı
or income redistribution goals as by the usual criteria of eco
efficiency.

When firms are rational profit maximizers, the analysis of th
persion of technological innovations falls within the body of ex
economic theory.[8] Though the qualitative analysis in this case w
the same for less developed countries as for advanced countrie
particular quantitative results in technical dispersion may vary
stantially due to the different weights of the controlling facto

[6] This analysis can apply just as well to family firms in advanced countries as t
in less developed areas.

[7] This would, I believe, apply, for example, to the Enclosure Movements in Eng
the eighteenth and nineteenth centuries.

[8] See, for example, W. E. G. Salter, *Productivity and Technical Change* (Can
1960), Part I.

priori judgments about these are difficult. For example, risk elements in costs associated with new technologies are often considered to be more significant in less developed than in advanced countries. Even if this is so, such cost differentials may be offset in the less developed countries by a greater assurance as to revenues which derives from government protection or the monopoly position which is often associated with new products or processes in less developed countries.

The extensive debate in the literature on the choice of technology in less developed countries is relevant to the dispersion of technological innovations. The debate has, for the most part, been addressed to the normative question: "What is the optimal choice of technique in the less developed countries?" Though the issues have not been fully resolved, it is now clear that the answer takes the form: "It depends on the content of the development goals and whether there are any relations between their attainment and the technology other than the direct contribution of output to the national product." When the goals include achievement of a certain growth or employment rate and these in turn are related to the savings rates from the different factor shares which in turn depend on the relative input intensities, the optimal choice of technology may well be different than it otherwise would be. The normative discussion has not, however, culminated in practical measures to control technological choice other than the suggestion of subsidies or government exhortation. This is in part because the analytical development in this field has far outstripped the empirical basis which exists for implementing the normative proposals.

It is often assumed that the range of technical choice available to the less developed countries is biased toward laborsaving and capital-using technologies. Some recent developments in the theory of induced invention can be adduced to help clarify this point. Professor William Fellner has been among the most active persons in drawing attention to the possible significance of induced invention for the explanation of some of the characteristics of economic growth.[9] Professor Charles Kennedy[10] and Dr. Christian Weizacker have led in extending the argument, which has long historical antecedents[11] but which was made most explicitly by Professor John Hicks,[12] that there is a tendency for innovations to have a laborsaving bias. The argument briefly, as Kennedy puts it, is that the search by entrepreneurs for cost-reducing

[9] W. Fellner, "Two Propositions in the Theory of Induced Innovations," *Econ. J.*, June, 1961; and *Trends and Cycles in Economic Activity* (New York), Chap. 9.

[10] C. Kennedy, "Induced Bias in Innovation and the Theory of Distribution," *Econ. J.*, Sept., 1964, pp. 541-48.

[11] M. Blaug provides a useful brief evaluation of the arguments in his article, "A Survey of the Theory of Process-Innovations," *Economica*, Feb., 1963, pp. 26-30.

[12] J. Hicks, *Theory of Wages* (London, 1932), Chap. 5.

inventions is given a capital- or laborsaving bias depending on the relative share of the factors. Since labor's share is the larger, there will be a tendency for innovations to be laborsaving. To demonstrate this, Kennedy makes use of a transformation relationship between reductions in capital cost and reductions in labor cost which is implicitly assumed to be symmetric. Professor P. A. Samuelson has shown that if relative shares are not technically determined, as in the Cobb-Douglas production functions, the argument leads to the paradoxical result that in equilibrium labor and capital shares will be equalized.[13] With other strong assumptions about the nature of the production functions and the relative growth rate of capital and labor, Samuelson is able to deduce a laborsaving bias for inventions. However, against such arguments for an inherent laborsaving bias, Samuelson and Salter have argued that entrepreneurs in seeking out inventions are interested only in reducing total costs. In pursuing this goal, and without knowledge as to the relative ease with which labor- or capital-saving cost reductions can be achieved, there is no reason to believe they will have a tendency toward either labor- or capital-saving innovations. Finally, the once widely held belief that, for whatever reason, inventions were as a matter of factual record for the most part laborsaving is more generally challenged.[14]

These recent arguments do not mean that the menu of technologies in advanced countries from which less developed countries may choose has no particular bias as compared to those already in use. Rather they indicate that the existence of such a bias cannot be inferred from an economic rationale alone. It is also true that if advanced countries are in fact characterized by relative capital abundance, they will take advantage of the substitution possibilities which may exist in order to use relatively less labor. It might then be argued that whatever the factor bias in the invention which created the technology, the relatively capital intensive factor combinations resulting from input substitution are "frozen" into the design of standard equipment. In turn, the less developed countries may face the alternative of either buying the standard equipment or paying higher prices for equipment especially designed for their own factor intensities. In either case the input substitution which potentially exists in a technology does not become as fully available to the less developed countries as to the advanced countries, due to economies of scale in manufacturing equipment for the latter.

[13] P. A. Samuelson, "Notes on the Weizacker-Kennedy Theories of Induced Invention," to appear in *Rev. of Econ. and Statis.*

[14] See M. Blaug, *op. cit.*, pp. 22-24.

III. *Invention in Less Developed Countries*

Issues in the theory of induced invention were raised above with reference to the question of the character of the technologies available from the advanced countries for use in the less developed. In approaching directly issues related to invention in the less developed countries, it might be useful to begin at a somewhat more general level. Richard Nelson has contrasted a "demand" theory of inventions and what I shall call a "supply" theory to indicate the predominant influences in each type of explanation.[15]

The demand theory, which may be described as a "necessity is the mother of invention" theory, as Nelson summarizes it, argues that: "Social need, usually manifesting itself through perceived opportunities for private profit, not chance, is the cause of inventions."[16] In this theory, to oversimplify somewhat, inventions are for the most part produced to order by step-by-step refinement of the known "state of the art." The rate of "production of invention" depends on the profitability of the inventive activity. In the "supply theory" it is science and the "social heritage of knowledge and technique which is the real mother of invention."[17] Inventions occur independently of any social need and are explained by the momentum of scientific progress. It is the innovation or implementation of inventions which is dependent on social and economic conditions.

As Nelson says, neither the demand nor supply theories appear adequate in themselves to explain many of the case histories available to us which appear to " . . . illustrate the interplay of moving frontiers of knowledge and growing need. . . ."[18] Neither of the theories provides any a priori basis for believing that inventions will have a particular bias in saving one or another of the productive inputs. The theory of induced invention as developed by Samuelson leads to the conclusion that "induced invention has no systematic bias and the drift of relative shares depends on the drift of exogenous technical changes and upon the change of factor proportions (as affected by the relevant elasticity of substitution)."[19]

This discussion is relevant to the rationale of research and development activities in less developed countries. There is a widely held view that research and development activities in less developed countries considered as investments have a rate of return so high as to warrant

[15] R. Nelson, "The Economics of Invention: A Survey of the Literature," *J. of Bus.*, Apr., 1959, No. 2, pp. 101-27.
[16] *Ibid.*, p. 103.
[17] *Ibid.*, p. 106.
[18] *Ibid.*, p. 107.
[19] P. A. Samuelson, *op. cit.*

a much larger effort than anything now under way. This opinion is usually supported by the customary references to the studies which have indicated that technical change has been a major contributor to economic growth in advanced countries and to the case studies in which the rate of return to research appears to have been enormous. However, even if these studies are accepted at face value, since technical change can come about by transfer as well as indigenous development, a separate argument is required to justify research and development programs in the less developed areas. One such argument would be that the uniqueness of their resources limits the value of technological transfers though previous results indicate the potential of research directed specifically at those unique features. This appears to be the view of Professor T. Schultz with respect to agriculture in less developed countries but it may have a wider application. A supporting argument would be that monopoly positions in advanced countries permit them to extract most of the benefits of technological transfers. Research has also been justified as an activity which is necessary to achieve higher education of the quality desired in less developed countries.

It cannot be assumed that both the costs and benefits of research and development in the less developed countries are the same as in advanced countries. Research and development and education are to some degree competitive in their demands for scarce personnel and other resources. If the shortages of educated personnel and, therefore, of teachers are relatively greater in the less developed countries than in advanced countries, that would suggest the desirability of research and education proportions weighted more toward the latter.

There is relatively little systematic information about the relationships between "inputs" and "outputs" in the research process but there have been suggestions that there are important economies of scale. These might place a diversified research program beyond the reach of most of the less developed countries though it would still be feasible to carry out specialized research programs at an appropriate scale. Yet the size or endowments of many of the less developed countries might not permit them to obtain benefits from the research to the same degree as in the case of larger and more diversified economies.

Recognition of the tremendous benefits which have accrued from research should, moreover, not be transformed into the assumption of a short and predictable connection between research and growth. There is ample testimony to the uncertainty and high failure rates involved.[20] There is little organized information on the gestation periods between research expenditures and their achievement of substantial economic benefits. But in numerous case studies the delays stretch over time

[20] R. Nelson, *op. cit.*, pp. 112-15, esp.

spans several times longer than ordinary investment gestation periods. The evidence on the relation between research and growth in *Some Factors in Economic Growth in Europe During the 1950's*[21] may or may not bear directly on this point but it should lead to some caution. It is pointed out there that "there is no correlation between the rates of growth of output and research outlays during the period studied,"[22] for twelve countries from 1950 to 1960. This conclusion may be the result of using a period which is too short to permit the benefits to be achieved, the existence of a relationship which is too complex to be found by simple statistical correlation, or, in fact, the lack of any relationship.

The policy issues of what type and how much activity to support in research and development are particularly difficult for the less developed countries. The divorce of economic from other criteria is less warranted; yet the relationships are less clear among the economic and other influences which have determined the fields of relative scientific advancement. A tentative answer to the policy question on economic grounds, taking into account the risk and scale elements mentioned above and the possibility of technological transfers, would advise concentration on problems directly related to each country's natural endownments and growth requirements. Another type of answer would be to emphasize whatever fields in which some success has already been achieved. This would recognize effects of economies of scale and be more closely related to the political and educational arguments for scientific research in less developed countries. Such qualitative answers are obviously not satisfactory to budget-makers: they do not tell "how much" and as to type of research they may even point in different directions. Much more analytical and empirical effort is needed, however, before better answers can be given.

[21] Economic Commission for Europe, United Nations, 1964.
[22] *Ibid.*, Chap. 5, p. 7.

DISCUSSION

Barend A. de Vries: The maintenance or acceleration of growth in developing countries depends vitally on the transfer of technology as well as on the generation of an indigenous technology in the underdeveloped countries. Flows of external capital are of little avail if they are not accompanied by or, if necessary, linked with an improvement in productivity. Such linking is particularly important in financing of those countries which have as yet made little progress in utilizing new technology. This is suggested by experience with the absorption of capital inflows, on which I will make a few remarks. In doing so, I am mindful of T. W. Schultz's observation that technological change is often used as a name for a set of unexplained residuals.

Application of new technology depends on the availability of various factors. Thus, we should allow for the differences in factor availability among developing countries and, perhaps even more important, the differences in costs to be incurred in changing these availabilities. The analysis of the labor surplus economy has only limited application where there is an abundance of land—as in parts of Latin America and Africa.

For our subject it is essential that we do not group all underdeveloped countries under the general heading of "low-productivity economies." Production centers using more advanced methods assume an increasingly important place in many underdeveloped economies, especially those of relatively large size (say, with a GNP over $4 billion) or with relatively higher per capita income (say, over $250). Thus, in agriculture we find both family-type subsistence farming and commerical farmers seeking to maximize their profits (Eckaus). The advanced centers, both of manufacturing industry and commercial agriculture, have many of the preconditions (social and business organization, education, etc.) needed to assimilate new technology. In these centers, the problems of assimilating new technology for complex industry or temperate zone agriculture are, in principle, similar to those existing in developed economies. The new industries in these centers have predominantly capital intensive technology: the choice of technology is, in practice, narrower than the range of choice considered in theoretical discussions; moreover, in making the choice the relevant fact may not be the abundance of unskilled labor so much as the scarcity of skilled labor. In many developing centers skilled labor, although available, is becoming expensive for a variety of reasons. Thus, there may be both technical and economic reasons for capital intensive production in important sectors of the underdeveloped economy. This is also borne out by the experience in transportation economics discussed by Meyer.

More difficult problems arise where the lessons of experience in developed countries cannot be applied. Especially in tropical agriculture, developing countries must often break new ground. The potential contribution of basic research or "third-level assimilation" (Solo) may be greater in this field than

in any other. Even where better methods are known, either through relevant experience in other developing countries or through research findings, they often cannot be applied until greater availability (i.e., lower cost) of one or more factors makes possible the necessary shifts in factor combination. This will require that the country overcome a backlog in investment in infrastructure and education which in rural areas tends to be more pronounced than in the industrial centers. The crucial factor which can set the stage for wider application of technology varies in different situations. In arid regions it is often improved water supply which will set in train a process of applying other factors (seeds, fertilizer, machines) and utilizing new technology. Where water and land are available at low cost, the crucial factor may be a better trained and informed farm manager. In the broadest sense, it may be necessary first to acquaint the farmer with examples of higher living standards before he will utilize factors in different combinations and strive to earn more through better production methods. Where there is an abundance of land, it may be the farmer's attitude toward conservation which will be the greatest obstacle to improved technology.

Development of better farm management may have implications for the size of the farm—and thus for the country's social conditions. The minimum farm unit needed to accomplish technological improvements may exceed the traditional unit; e.g., where management has to be highly specialized. It is worth while to observe that in Latin America productivity has often been improved on the larger units by landowners making better use of their lands with modern methods.

Developing countries also may have to break new ground when technological advance comes in greater spurts than was experienced in the developed countries. This may have an advantage: in establishing new industries the developing countries may not need to cope with the costs of disposing of old plants or old skills. But the unavoidably large spurt had disadvantages in terms of organization and capital costs. For example, the developed countries gradually increased the weight of road transport vehicles and the corresponding improvements in road standards and design could also be introduced gradually. But the developing countries now start using heavy trucks without first experimenting with lighter vehicles and their new roads must therefore be built to suit the heavy trucks. Inherent in the large technological spurt is the diminished role of the factor which is most abundant: unskilled labor. Thus, in the construction of these higher standard roads, the extent to which labor can substitute for machinery is severely limited by technological considerations, as well as the cost of delays and the low quality associated with labor intensive construction.[1] Lighter vehicles designed for the underdeveloped economy would bring capital saving, both on roads and vehicles. But this requires development-oriented research, not now conducted by those qualified and able to achieve economies of scale: the large manufacturers in developed countries.

[1] "Substitution of Labor for Equipment in Road Construction" (IBRD, mimeographed, May 6, 1965).

In the transfer of technology—and the creation of a "hospitable context" (Solo)—the recipient operating unit is strategic. I have already mentioned the size of the farm unit and the need for specialized management in tropical agriculture. In infrastructure, the construction and operation of new facilities must be undertaken by organizations independent of traditional political and social influence. The same is true for the large and complex new industry, but there it is easier since industry is farther removed from the political center than is infrastructure. In practice, these units and their technicians will have close links with their foreign counterparts. The joint (foreign-domestic) venture—in construction, engineering, or manufacturing—is a proven vehicle for the transfer of technology. Waiting for the establishment of suitable operating units and of joint ventures may seem to slow down the pace of investment and to reduce capital utilization, but over the longer run the economies achieved (and the training obtained) will pay off.

The relatively small scale of production limits the application of advanced technology in underdeveloped countries. Often the larger scale activities are found in the public sector which therefore can play a key role in promoting the use of more productive methods, frequently by granting the operating units maximum autonomy. The fact that public administration is often on the lower side of the productivity scale, points to both the difficulties to be overcome in the center of government and the potential for improvement. Governments can induce a growth pattern receptive to advanced technology through appropriate expenditure planning (e.g., in technical education, or by preference for the larger-sized infrastructure over smaller and less economical units). Equally important, governments can help by favoring industrial development through units producing for the larger markets. In the case of many countries, this means export orientation and sometimes special subsidies in the initial stage.

Lloyd G. Reynolds: There is little in these papers with which one might quarrel. Nor is there any point in restating what the speakers have said so effectively. Let me rather comment on two or three of the central issues, referring to one or other of the papers where this is pertinent.

At a factual level, we still know remarkably little about the international transmission of know-how. How does technical knowledge in fact find its way from the advanced to the less developed countries? One can think of various channels: branch plants or joint ventures initiated by companies from the developed countries; "turnkey projects" in which a foreign company builds plant and manages it through the "teething stage" of development; technical assistance by engineering and management consultants from the developed countries; the missionary work of salesmen for modern machinery and other inputs. (The work of these people should not be underestimated. I have heard it said that in American agriculture the work of the machinery salesmen, the hybrid corn salesmen, and the rest may have been as important as the activities of the extension agents.) Then there are the slower processes of higher education, of sending students abroad from the less developed countries for

technical training, importing teachers and research workers from the advanced countries, and so on.

Each of these channels makes some contribution. But what is their quantitative importance? Are some of them more efficacious than others in overcoming resistance to innovation and getting new knowledge transferred, adopted, and applied? As one of the speakers remarked, these questions arise within our own discipline. Assuming that economics is a useful technique, that it is desirable to raise the level of economic competence throughout the world, and that the resources available for this purpose in the developed countries are limited, what do we do? How do we deploy our university facilities and our professional manpower so as to make the greatest contribution? One hears much casual discussion of these matters; but I have not seen economic analysis applied systematically to this kind of optimizing problem.

A second strand in our discussions is the question whether the technologies used in the advanced countries are well adapted to the factor supply situation in the less developed countries. There is a familiar argument that in labor-surplus economies, technology needs to be adapted in a labor-using direction. The need is for capital shallowing rather than capital deepening. This argument needs to be viewed with some caution, partly because—as two of our speakers have remarked—unskilled labor and physical capital are not the only ingredients in production. Skilled labor is typically scarce, and competent supervision and management are particularly scarce; and this may indicate greater mechanization than would otherwise be desirable.

There is also the factual question of how far there is an effective choice of technology for specific products. Our examples of labor-using adaptation seem always to come from agriculture, where factor proportions are notoriously flexible, or from textile production. But textile production may be untypical of manufacturing generally, and it may be that for most products the range of choice is quite narrow. This is a research area in which engineers and economists might collaborate to good advantage.

If I understand Professor Meyer correctly, he is saying that the big decisions bearing on factor proportions are the decisions about what industries or sectors to emphasize in a development program; that when one comes down to specific products one technique will usually emerge as most efficient within any plausible range of factor prices; that in any event a case-by-case approach is essential; and that the results will be difficult to generalize. This seems sensible to me. I am impressed also by his suggestion that the important substitution possibilities are not just between labor and capital, but also—say in the case of road transport—between roads and vehicles, between initial costs and maintenance costs, and between vehicles of different vintages. Without having looked at the detailed calculations, it seems reasonable that many countries should concentrate on building up maintenance capacity and spare parts capacity rather than producing or importing the latest-vintage vehicles. Perhaps AID can graft a cars for peace program onto the junkyard-elimination program already on the statute books.

A third question, raised in both Professor Eckaus' and Professor Solo's pa-

pers, is that of the optimal amount of research and development activity in a less developed country. Activity aimed at adapting imported technology to local circumstances must almost always yield positive returns. This is notably true in agriculture, where the need for adaptation has been demonstrated many times over. But in industry also there is usually need to adapt equipment to the peculiarities of raw materials and other local inputs, to the presence or absence of good maintenance and repair facilities, to the shortcomings of plant supervision, and to labor supply conditions. Where low-skilled labor is in excess supply, there will usually be some possibility of squeezing in additional labor around the edges of the main production processes—in materials handling, warehousing and shipping, plant maintenance and repair, and the like.

But the technicians needed for this adaptive activity are scarce; and so are the scientists needed for work at Professor Solo's second and third levels of assimilation of advanced technology. These same people, as Professor Eckaus rightly points out, are needed as teachers and research workers in the educational system. A balance must be struck between short-run research and development output and longer-run educational output. Even in the United States, with our relatively ample professional resources, the system of higher education may well have been weakened over the past generation by the great flood of money available for research. The possibility of a dangerous stripping down of the educational system is even more real in the less developed countries, where college teaching is typically a low-income and low-status occupation.

In this area, as in the others I have suggested, there is no substitute for detailed information about particular situations, for careful analysis, for judgment about priorities and strategy. Hypotheses and generalities are a dime a dozen. We still have very few facts.

WOLFGANG F. STOLPER: The three papers do not leave much room for critical disagreement. Eckaus has given us an analysis of the demand for labor under institutional arrangements which are not necessarily profit maximizing, and a suggestive account of the discussion about the labor- or capital-saving bias of innovations together with a preview of some unpublished views of Samuelson. Meyer takes us to the more specific—though still wide enough problem—of transport planning and concludes not only that detailed analysis of specific cases is essential before anything useful can be said, but that at least some of the dichotomies which are presented to us may be something less than relevant. Solo, in a thoughtful piece, tries to come to grips with the obstacles that have to be overcome in the process of adopting and adapting advanced technologies in underdeveloped countries. Thus the papers dovetail nicely: What can we say in general as to the choice of technologies under certain circumstances (Eckaus)? What can be said on this point in the specific case of transportation (Meyer)? Why have we so much trouble getting technologies to move, if not across frontiers, at least across cultures (Solo)?

The questions that the papers raise in my mind are twofold. The first relate

to fact; the second to matters of interpretation. Both are offered to supplement rather than to disagree with the papers. Both are related.

The first is this: In what sense can it be said that there are cultural or political resistances to the acceptance of modern technologies which are in some sense irrational? It can hardly be denied that Indonesia, say, presents a textbook case of *de facto* resistance by the leadership to modernization. It is likely that the day of reckoning is at hand for one can eat up one's substance only once. But take the case of West Africa, which I know best. With the only major exception of oil palm products, virtually every export good and staple food crop has been brought in from abroad and developed by indigenous peasant agriculture in a most successful manner. The chances are that resistance to new products or new methods has been due to either the fact that they did not work, e.g., one still does not know always just what fertilizer will in fact do, or that they do not pay. The latter will lead to my point of interpretation.

Or take the common assumption that labor, at least unskilled labor, is redundant on the Asian subcontinent, and that therefore the marginal product of labor is zero and that in the institutional arrangement of the subsistance farm and extended family, employment will be greater than in a profit maximizing farm—as elegantly analyzed by Eckaus. But Morton Paglin's analysis of the Indian Farm Management Studies throws at least doubt on the hypothesis of redundant labor.[1] The Indian data may be bad and subject to more than one interpretation, but they make the redundancy hypothesis less than self-evident.

The first fact—and it is a fact—raises questions why some innovations are accepted and others are not: whether an economy can be ripe for some but not for other technologies. It also raises questions about the time horizon: whether the impression that it is difficult to transmit technologies may not be due to an impatience with the time it takes to carry through a radical transformation of an economy. Perhaps we can come to grips with the problem in a slightly different way.

Which brings me to my problem of interpretation. Suppose I look at the problems (and answers) presented by our three authors from a slightly different viewpoint which allows explicitly for a time horizon. How can the problems of the proper technique to be employed be divorced from the general problem of development? How can the problems of the techniques to be used in a particular firm or plant or industry be divorced from the problems of what to produce in the first place? What possible meaning can I give to any particular factor intensity definition? How can I say anything useful about the bias of future innovation? I believe that there is a danger that the questions are, in a real sense, wrongly put. As Robert Solow put it another context: The questions, after bias of innovations or choice of techniques, may or may not have answers, but in a sense they may be irrelevant as put.[2]

[1] Morton Paglin, "Surplus Agricultural Labor and Development: Facts and Theories," *A.E.R.*, Sept., 1965.

[2] Robert M. Solow, "The Great Automation Question," *The Public Interest*, Vol. I, No. 1.

Put yourself into the shoes of a planner who has to allocate resources for growth. He wants to achieve the fastest possible growth through increasing the productive capacity of the economy and making it more flexible. He wants to maximize the resources he can utilize for these purposes and he must allocate them as well as he can. Now notice that the resources consist of a bundle of things: labor of varying skills; land of varying qualities; machines; raw material; some foreign exchange. Some of the things can be used for almost anything, particularly foreign exchange. Others have various specific uses and are difficult to shift. Moreover, the planner can reallocate at any one moment of time only part of the resources, but he can always reallocate some resources at every moment of time. And, finally (for the present purpose), the economy is producing at a certain level of technology and hence has a given income.

I have avoided the word capital. For I do not know what it means in such a context. But I have no doubt what I would do in such a situation. I would try to distribute all my resources in such a manner as to give me the highest social pay-off in the short run. If I were confronted with reasonably accurate prices this would, in fact, save on the scarce ones in the sense of making the best use of them. The scarcest resources are as likely to be managerial talent or skilled labor as foreign exchange or "capital." Such a criterion would get me the most resources in the next period for reallocation for all purposes. It would take into consideration the limited market which would grow at the fastest possible rate. It would automatically select the best techniques and the best "industries," as well as the best factor proportions, and it is even likely to lead as a result (but not as a selection criterion) to import substitution.

I am just putting into blunt everyday words the more sophisticated findings of Robert Solow in his De Vries lectures: "Without any dubious 'measurement of capital,' within whatever technological assumptions instinct and observation lead one to make, it is possible to pose and to answer what I have claimed to be the central question of capital theory. What is the pay-off to society from an extra bit of saving transformed efficiently into capital formation."[8]

If this view of a proper investment criterion is accepted, the problems of technological transference appear, perhaps, in a slightly different light. It still remains true that no good technologies may exist and that a particular problem is not solved as yet. Tropical soils undoubtedly pose such problems. Obviously research and creative adaptation are the painfully slow answer. Second the criterion proposed, despite the variations that can be introduced via shadow prices and other refinements (not to say gimmicks), is reasonably clear. But it also indicates that no miracles can be expected, the brake to miracles coming from the condition that the savings must be "efficiently" transformed. One can accelerate just so much. It is obviously true that in many specific cases a society may skip a whole series of technologies and move from headpan directly to conveyor belt and truck. It is also true that it is a slow process to move a whole society growing at 3 percent rapidly to 7 percent.

[8] Robert M. Solow, *Capital Theory and the Rate of Return* (Rand McNally & Co. 1964), p. 34. (I wish I had had this excellent book when I was a planner to use his high authority where my lowly authority was doubted.)

Third, it can never pay in terms of growth of employment to use more labor than is profitable, if (but only if) the additional profits can be invested and thus create employment. It seems to me there is no logical or factual relation between factor intensities, income distribution and rate of savings. No doubt employment depends in a classical manner on relative prices of factors —many factors—but in a growing economy, in the Keynesian manner, also on the level of investment and, with more than two factors assumed, even in a classical system there are possibilities of complementaries as well as substitution of factors.

From this, it seems to me to follow, that obstacles to the application of advanced technologies will diminish as a society becomes richer, since normally (leaving aside the lucky find of oil or a sudden dramatic and sustained improvement in the price of a traditional export good) growth involves change toward a more sophisticated structure of production and the availability of an increasing number of people capable of applying the technologies as well as creating some of them. If the view is right, most difficulties are growing pains and can be overcome by patient husbanding of resources.

Obviously, there remain many questions to be answered. I find it difficult to believe that underdeveloped countries will achieve sustained growth as long as the adaptation of technologies to their problems is made exclusively in the advanced countries. One thing that is impressive about early American development is how early the creative adaptation of European technologies started: see President Jefferson's plow in the Smithsonian Institute. But I find it also difficult not to believe that African or Asian countries will get to this point if they have not done so already.

KNOWLEDGE, INFORMATION, AND INNOVATION IN THE SOVIET ECONOMY

INNOVATION AND INFORMATION IN THE SOVIET ECONOMY*

By GREGORY GROSSMAN
University of California, Berkeley

In the Soviet view, technological progress is not only virtually synonymous with human progress, but is also an overriding national goal and a major element of legitimation of Communist rule in Russia. From Lenin's "communism equals Soviet power plus the electrification of the whole country," through Stalin's injunction to his subjects to "master technology," to Khrushchev's and Kosygin's "construction of the material-technical base of communism"—each additional machine or kilowatt, every just-trained engineer, has been identified simultaneously with yet another forward step on the direct historical path to social perfection and human bliss as well as with the strengthening of the internal and external power of the Soviet state. The "world-historic victory of socialism over capitalism" is assured, we have been repeatedly told, because socialism (Soviet-style, of course) removes the obstacles to the full productive use of man's ingenuity and creativity and is capable of planning and organizing the productive forces of society for unstinted technological advance and, hence, economic progress.

A consequence of this approach has been the elaboration of a new social technique: the routinization of economic growth. This is to say, virtually everything connected with the process of economic growth—the accumulation of society's saving, capital formation, education and training, invention, research and development, technical modernization and innovation, dissemination of technological information, and (last but not least) the massive take-over of technology from abroad—has been centrally organized and planned and subjected to standardized, repetitive, routine methods and procedures. The technique is not exclusively Soviet or Communist; some of the just-cited functions have also been routinized for some time elsewhere; for instance, research and development in large "capitalist" corporations or the defense and space programs of major Western governments have also been in large

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measure routinized. But its comprehensiveness and extent within the national economy are distinctly Soviet-style.

In the present paper we shall be concerned with only one aspect of the routinization of growth in the Soviet economy: the routinization of innovation and the related problems of information. By innovation we mean something rather broad; namely, the introduction and diffusion of relatively more advanced production techniques, whether indigenously developed or not and whether the given technology is finding its first application in the Soviet Union or is already known there in practice. Innovation, as we use the word, thus comes close to the standard Soviet phrase, *vnedrenie novoi teckhniki,* often translated as "the introduction of new technology."[1] If the phrase "routinization of innovation" has the appearance of a contradiction in terms, it also reflects the dilemmas and paradoxes inherent in the planning of rapid technical progress in any "going" economy and especially within the rigid context of Soviet economic and administrative institutions.

We begin, therefore, with a few words about that context. The economy is centrally managed by means of detailed output and input targets assigned to enterprises and intermediate entities. Nearly every material producer good of importance is centrally allocated. Short-term planning aims at (1) mobilizing production resources to the utmost and (2) achieving minimal consistency between production capacities, output targets, and supply allocations. In the event, this consistency is quite poorly achieved. Labor, however, is deployed largely through a free market (a major exception being that the mobility of collective farmers is administratively restricted). Investment—which has in recent years accounted for something like one-third of GNP—is nearly entirely centrally planned and controlled. Its purpose is not only to propel the steady and rapid expansion of the country's productive capacity in the desired directions, but also to serve as the chief vehicle for the introduction of modern technology into the economy.

Prices for producer goods are administratively set (or, at least, approved) according to complicated principles and procedures which may have their rationale but in any case do not purport to seek our either scarcity or equilibrium levels. Given also some other aspects of the Soviet economic system, especially materials allocation, the result is that for most important producer goods demand exceeds supply and the usual phenomena of a seller's market prevail.

From the early days of the Plan Era until July, 1957, and again since October, 1965, industry has been formally organized according to

[1] This phrase, however, does little justice to its Russian counterpart. The verb *vnedriat'* bears the connotation of an organic process (to implant, to cause to take root), while *tekhnika* embraces both the technique or technology of production and the physical equipment that embodies it.

the "branch" ("product-line," "ministerial") principle; i.e., the enterprises belonging to a given branch of industry have been subordinated to the corresponding ministry (earlier, commissariat), usually regardless of their geographic location. Between 1957 and 1965, the "territorial" principle of industrial organization obtained—most enterprises were subordinated to regional economic councils (sovnarkhozy). The organizational "partitioning" of the Soviet economy is important for our inquiry because it tends to determine (1) the pattern of information flow and (2) the structure of what Peter Wiles has called "subordinate autarky"; that is, the tendency of each administrative level to be self-sufficient. The latter phenomenon, conditioned by the system of success indicators and by the prevalence of the seller's market, appears in the form of either "departmentalism" or "localism," and, together with the tendency of communication lines to coincide with those of authority, creates the malignant situation known in Soviet parlance as "departmental barriers"—the failure of enterprises or higher organs to effectively communicate or cooperate with each other.[2]

As we might expect from the opening observations of this paper, technological information of all kinds and from all sources is avidly gathered, assiduously translated, processed, and compiled, and aggressively distributed. The volume of technical publication is enormous. Throughout the Soviet period, highly organized efforts have been made to import technology from the more advanced countries in the form of stray information, systematic documentation, production processes, prototype equipment, and live advisers. The All-Union Institute for Scientific and Technical Information (*VNITI*) is unique in the world in the range and thoroughness of its collecting and publishing activities, although it is only one of many Soviet organizations devoted to this end.[3] Commerical secrecy as practiced under private enterprise is not supposed to exist (will the current efforts to raise the role of profit in the Soviet economy tend to revive it?) but departmental jealousy and what might be called subordinate secrecy (concealing one's true capacities from one's superiors) are practiced widely. Lastly, much technical information is also dispensed, often in directive manner, in the form of input-output coefficients ("norms") for planning, technical specifications and standards, and the like.

Much more complex is the picture with regard to economic information. As we have noted, producer goods prices are poor representations of the goods' relative scarcities for either the short or the long run.

[2] For further information on the structure and functional characteristics of the Soviet economy and on its prices, the reader is referred to [2] [9] [11] and [16].

[3] See [13, pp. 73-76] for a concise account of such organizations. This study by Peter Knirsch is so far the only systematic inquiry into Soviet planning of technical progress published in the West.

Now that state enterprises are about to begin paying interest on their fixed and working assets, capital will be carrying a price in current cost accounting as well as in planning, but it may be doubted that the rate—or, more correctly, the many "branch" and special rates—will accurately reflect the social marginal opportunity cost of this factor.[4] Land remains unpriced, though some other natural resources will supposedly soon bear rent charges. The exchange rate is of doubtful fidelity. Farm prices, even if somewhat more sensible than before, are still a country-wide crazy quilt. The wage structure does make some sense, but only with reference to supply and demand in the labor market—a circumstance from which Soviet labor was supposedly liberated by the advent of socialism.

For day-to-day production decisions, the most important economic information consists of plan targets, supply allocations, and success indicators. Both short-term and long-term planning rest on an enormous flow of statistical data, most of which is probably superfluous and in any case costly to collect and often of doubtful reliability. More important, while it forms the basis for all the planning in the traditional—centralized and "manual"—way, the information generated either in intrafirm bookkeeping or in hierarchical reporting is poorly suited to either management decisions[5] in the firm or to the prospect of mathematized planning at the top. Surely, at the enterprise level, and even considerably above it, there is little reliable indication from economic (as against administrative) signals of the socially most desirable directions for investment, as things now stand. Another serious defect is the very inadequate state of interfirm communication—lack of technical catalogues, advertising, salesmen—which makes it difficult to know what goods are actually or potentially available for use in production.

Perhaps even more inimical to technological progress and economic growth than the dubious quality of economic information generated by the Soviet economy is the lack of appropriate motivation for innovation at the enterprise level. The problem has been treated in the Soviet literature at the greatest length; we have had occasion to discuss it in this *Review* several years ago.[6] Complex in its many details, the matter is essentially quite simple: given the success indicators and the system of bonuses to management, the "taut" plans, and the "ratchet principle" of raising plan targets on the basis of most recent performance, management has little to gain and much to risk by espousing new products or processes. Besides, any change in the routine runs into in-

[4] Cf. [6, esp. Chap. 11].

[5] Recent analyses of the Soviet use of the rate of return in choosing among alternative technologies will be found in works by Bergson [2, Chap. 11] and Collette [7, Part III].

[6] [10]; see also [16, pp. 167-71] [19, Chap. 9] [3, p. 86].

numerable bureaucratic obstacles at all levels. Hence, widespread aversion below to anything new; i.e., a reluctance to act innovatingly on information that more than offsets the commercial secrecy of the market economy.

It must be noted that machine-building enterprises are also prey to this phobia of innovation. Thus, precisely the industry which in the Western market economy, through its sales campaigns, is responsible for much of the diffusion of innovations, in the Soviet Union has the opposite role of imposing technical conservatism on itself and its clients. Accordingly, it has been machine building that has been the chief beneficiary of various schemes to reward managerial and technical personnel responsible for innovation—especially since 1960—and to relieve the costs (and, thus, prices) of new products of expenses incurred at the development stage.[7] The Soviet literature does not give a strong impression that these schemes have as yet been markedly successful in lessening resistance to innovation.[8]

Of course, much—and in the earlier period, most—of what is technically new enters the Soviet economy by being embodied in newly built plants, where the dead hand of the present has not yet appeared. By now, the Soviet Union possesses an enormous establishment for the design of new (or redesign of old) production facilities. In 1963, there were some 1,300 so-called "project-making" organizations (engineering design bureaus); in late 1965 they employed over 450,000 persons and had an aggregate budget of 900 million rubles (*Pravda*, Dec. 8, 1965, p. 5). For reasons of space we cannot inquire into their operation. Suffice it to say that the project-making organizations have been under much attack in the Soviet literature over the years because of their slowness to innovate on the drawing boards, among other things. Doubtless, the fault is not entirely with the subjective qualities of their engineering personnel; they have had to contend with inadequate economic signals, faulty incentives, and a refractory, overcentralized system.

Insofar as it has been overcome, the resistance to innovation at enterprises and in project-making organizations has been overcome in large measure by continuous administrative pressure from above. Crucial to its success are the communication channels through which it is transmitted—a problem to which we shall return toward the end of this

[7] On these schemes for the "stimulation of new technology" see [1, pp. 98 ff.; V. Markov in *Planovoe khoziaistvo* (hereafter *P Kh.*), 1960] [6, English text in *Problems of Economics*, III] [10; E. Slastenko in *P.Kh.*, 1964] [2; A. Basistov in *Voprosy ekonomiki*, 1964] [5; and *Ekonomicheskaia gazeta* (hereafter *E.G.*), Jan. 20, 1965, p. 37].

[8] But, not unexpectedly, these schemes may have aggravated conflicts of interest between innovators and production personnel within enterprises; cf. Bassitov, *loc. cit.*, pp. 31-32.

paper in the context of the recent reorganization. But now we turn our attention to two of the most distinctive Soviet institutions in the field of technological progress, "uniform technological policy" and "the plan for the development and introduction of new technology."

Uniform Technological Policy

A frequently cited concept, uniform technological (or technical) policy (*edinaia tekhnicheskaia politika*) is also a most elusive one. We know of no rigorous definition of it in the Soviet literature. Often it merely refers to the dictator's whim in technical matters or to the particular technological hobbyhorse that the given author is riding. Insofar as it does have substantive content, the concept of uniform technological policy amounts to the centralized determination and mandatory enforcement of the technological aspects and parameters of a production process. This may refer to the basic characteristics of the process (e.g., thermal or hydraulic power generation, type of railroad traction, automation), its main technological parameters, the types and varieties of required equipment, standardization, typical size of plant, degree of vertical integration in production, etc. An example will be found in the Resolution of the 1959 Plenum of the Central Committee CPSU which selects many dozens of technological processes in virtually all branches of industry and transport for high-priority attention [17, pp. 501 ff.]. No doubt even starker examples could be culled from the early years of Soviet industrialization.

That something like the notion of a uniform technological policy should have evolved in the U.S.S.R. is hardly surprising. It is, first and foremost, a corollary of the central planning of investment, especially where economic advance has been seen largely in technological terms to begin with. Second, investment has to be supported by specific materials allocations, which has also been highly centralized in Soviet practice. That is to say, technological specifications determine precise material requirements, which cannot be honored except at the top. Third, under Soviet conditions, the necessary linkages with other industries can be properly handled only at a very high level. Fourth, centralizing technical decisions at the center is presumably an economical way of utilizing engineering and other scarce skills. Thus, A. Kostousov, chairman of the State Commission for Automation and Machine Building which was created in 1959 expressly to lay down a technological policy in that area, wrote soon after assuming his new post:

> [Technological progress requires] uniformity of technological policy in machine building. There cannot be isolated technological policies in Riasan', Minsk, Moscow, or Leningrad provinces in regard to, say machine-tool building. The coordination of technological policy is a most important task at the present stage. It will allow to save the efforts of

scientists and engineers, to eliminate unnecessary parallelism and duplication, and to create the conditions for the developing the most progressive technology in all branches of machine building.[9]

On the other hand, we may well ask how feasible—under actual Soviet conditions—would have been the decentralization of basic technological decisions given the inadequate motivation, the absence of reliable economic signals, vagueness in regard to decision rules, and—we must add—far from perfect identity between the goals and values at the center and at the economic periphery.

To be sure, insofar as we know, the central authorities have not had the benefit of much better economic parameters or much more precise decision criteria than those available to their subordinates. They, too, have tended—perhaps even more consciously—to mistrust internally generated price-cost information and to put their faith in more strictly technological criteria and into imitation of foreign practice. The latter, particularly, seems to have been an important criterion—both for want of others and as a convenient hedge for the individual decision-maker against the risk of being accused of deciding wrong. After all, American (or German, or British) technological solutions could hardly be wrong! And besides they are already at work. Where the central organs, however, have indeed had a clear advantage over the periphery is in their overview of the physical requirements of the whole investment program. This, of course, has only tended to support the notion of uniform, centrally-determined technological policy—even if in the event the requisite consistency and coordination have apparently been minimal.

Nor is much known as to the roles of various planning bodies and political organs in this connection. There is little doubt, however, that the abolition of the ministerial structure in 1957 adversely affected communication along branch lines and thus militated against innovation insofar as it depends—as it largely does in the U.S.S.R.—on planning and administrative pressure from above. The progressive multiplication of the State Commissions for individual industries between 1957 and 1962, and even the enhancement of their authority in matters of technological policy in 1962-63, seems to have more complicated than strengthened planning along branch lines. These difficulties have provided the main reason for a return to the formerly discredited ministerial (branch) system of industrial organization.[10]

In sum, uniform technological policy has been both reality and mir-

[9] *Kommunist*, 1959:8, pp. 11-12; cf. *idem* in [18, pp. 108 ff.].

[10] The implications of the 1957 reform for economic growth were discussed by us in [10, pp. 70-71]. A useful and concise Soviet analysis of the implications of branch and territorial partitioning may be found in Birman's booklet [4, pp. 49-64]. Knirsch gives a good account of the many organs engaged in planning technological advance in the U.S.S.R. [13, pp. 76 ff.].

age in Soviet practice. It has certainly been a reality in the sense that huge industries have been built up on the basis of particular technologies. But the economic wisdom of these technological solutions has been quite uneven, and often probably insufficiently or inefficiently investigated in the first place. Moreover, the very goal of technical uniformity itself may have been irrational where economic conditions require a variety of technical solutions; witness the patent case of agricultural machinery. But seen on its own terms and with all the technological biases to which it has been prey, uniform technological policy in the U.S.S.R. seems as often as not to have been unattainable. Even within individual industries this has often been so owing to conflicts of personal or departmental interests, bureaucratic empire building, "departmental barriers," materials shortages, resistances to innovations, and all the other facts of Soviet economic life. It has frequently lacked continuity—or has continued for much too long (as in the case of traditional emphases on coal, steel, or railroad steam traction). As we shall presently see, it has not been effectively supported by the expressly-designed "plans for the development and introduction of new technology." And there seems to have been relatively little connection between technological policies in different industries.

Plans for the Development and Introduction of New Technology

Though far from being the only vehicle of technical progress in the Soviet economy, these plans have been the chief formal documents through which the authorities have been trying to carry out technological policy in industry and construction. Begun in the late 1940's, they are constituent parts of annual economic plans (and only of the annual plans) on each level, from the union government down to the individual enterprise. Unlike production plans, at the higher levels these plans do not aggregate the provisions of those at lower levels but rather select items of requisite importance. In recent years these measures have been a rather varied assortment, falling under six main rubrics: (1) Directives for the mechanization and automation of production processes and the introduction of advanced technology; (2) directives for the development and production of prototypes of new, important machines and articles; (3) directives for the most important research, development, and experimental projects; (4) a list of obsolete machinery, devices, etc., whose production is to be terminated; (5) directives for quantity production of new kinds of industrial products; and (6) a statement of the requirements of materials, equipment, etc., for the implementation of this plan.[11] The last provision

[11] The fullest description of the plans for new technology will be found in [8]. A brief historical sketch is in [14, pp. 135 ff.], while the situation in the early years is described by Sokolov in *P.Kh.*, 1951:6.

dates only from 1962; financial requirements were included in the plans for new technology also for the first time only in that year.

Soviet sources leave little doubt that the plans for new technology are among the least effective or successful in the industrial sector of the Soviet economy. Year after year, the nominal fulfillment of the union-level plan for new technology is of the order of 50-60 percent [17, p. 728] [18, p. 264] (Grishin in *Pravda*, Nov. 22, 1962). Even this may overstate the real effect in that the percentages refer to proportions of the number of items (projects) carried out or completed; there is accordingly a tendency to carry out or complete—whatever this may mean—the smaller items (projects) first for the sake of a better record. The reasons for the poor fulfillment record are many. Those most frequently cited are the lack of support with materials allocations (even since 1962), lack of financial backing, and the usual resistances to innovation.

But the plans for new technology seem to be very poorly drawn up to begin with. They are on a strictly annual basis with little if any reference to any long-term projections, let alone plans, of technological development. The selection of items and projects seems to be unsystematic, even chaotic, and with little carry-over from year to year. There is little coordination between this aspect of the annual plan and such important aspects of it as those pertaining to production, labor, supply, costs, and profits. For that matter, there is little coordination between the various parts of a plan for new technology.[12] Notably, few projects in the plan are supported by any kind of economic justification, and those that are tend to be those for which bonuses to the innovating personnel are to be paid, which renders the economic calculations suspect from the start. And so forth.[13] Perhaps it is just as well that the plans for new technology tend to be only half-fulfilled.

One can consequently appreciate all the more the intense search for institutional improvement, especially in regard to innovation and growth. The following passage, by L. Gatovskii, the recently-appointed director of the prestigious Institute of Economics of the U.S.S.R. Academy of Sciences, underscores the issue:

> Under the conditions of the present-day scientific revolution and of the competition and struggle between the two world systems, the planning of scientific and technical progress must, objectively, be the leading link of the whole system of national economic planning.

[12] In a similar vein, it was alleged by no less an authority than Kostousov (*Pravda*, Aug. 28, 1959) that the planning of machinery output for the Seven-Year Plan (1959-65) proceeded without reference to the investment plan.

[13] Criticisms of the plans for new technology are legion. To cite a few almost at random: [17, *passim*; 18, *passim*; G. I. Samborskii in 15, pp. 268-83; P. Abroskin in *P.Kh.*, 1961:3; I. Kasitskii in Kommunist, 1961:2; A. Pliner in *P.Kh.*, 1962:10; N. Semenov in *Pravda*, March 23, 1962; G. Kozlov in *E.G.*, 1965:46, p. 6; L. Gatovskii in *E.G.*, 1965:48, p. 5]. For analysis once again refer to Knirsch [13, pp. 88 ff.].

Yet, hitherto, this link has perhaps actually been the most lagging link in the whole complex of national economic planning and in the whole system of material incentives for production.[14]

In a society as rich in paradoxes as is the Soviet, the area of technological progress and innovation contains its aliquot share. Everywhere in the U.S.S.R. the technologically most advanced and most backward are to be observed side by side. There is centralization of the highest order and at the same time failure to achieve some of the putative advantages of centralization; for instance, surprisingly modest progress has been made in the field of industrial standards.[15] Or there has been so far remarkably little progress in automation, despite an impressive scientific base and the absence of the institutional features that allegedly hold back automation in decentralized, pluralist economies.

Soviet technical achievements are generally not in dispute; it is the paradoxes by which the outside observer is fascinated and for which he seeks explanations. To be sure, many of the instances of lingering technical backwardness have their economic justification, if not historical explanation, in factor proportions (although on this score the foreign observer may be more generous than the domestic critic).[16] But as often as not Soviet contrasts have little economic rationale: their explanation must be sought in motivational, organizational, and institutional causes. We have already had reference to some of these, particularly those pertaining to the quality and transmission of information, incentives, and the methods of drawing up plans for new technology. We now return to the concept of routinization of innovation.

The routinization of innovation on a national scale presumably means: (1) forecasting of technology for a substantial period, say a decade; (2) an economic methodology for selecting among technological alternatives, supported by projections of economic parameters; and (3) an organizational set-up to realize the desirable innovations. All three are much easier listed than accomplished. Although forecasts of technological progress are notoriously uncertain and risky, this part may be the least difficult in the Soviet case. In the recent past and for the near future the Soviet Union could and still can set its sights largely on the technology already developed and applied in the other industrial countries; thus fortunately avoiding much of the uncertainty

[14] *Loc. cit.*

[15] Cf. [20] [12, pp. 67 ff.]; V. Agranovskii in *E.G.*, 1961:9, pp. 26-27; V. Boitsov in *Izvestiia*, July 9, 1964; editorial in *Pravda*, Feb. 6, 1965; V. Tkachenko in *P.Kh.*, 1965:7. On the economics of industrial standards generally see Brady [5, Chap. IV]. Soviet problems in the area of industrial standards seem to rest on such institutional conditions as "departmental barriers," which block or delay agreement on specifications, and the opposition by producers, who dislike standards because they impose a quality constraint to quantitative plan fulfillment.

[16] Cf. S. Kheiman [1, pp. 136 ff.].

attendant upon the projection of technological progress. Offsetting this advantage is the rigidity of the Soviet economy which resists adjustment occasioned by earlier faulty projections.

For lack of space, we shall not discuss here methods of economic choice among technological alternatives, except to note that for a long time these methods—and the corresponding informational requirements—were neglected in the U.S.S.R., although there is now a strong revival of interest in them among Soviet economists.

Much attention has lately been focused on the organizational side, thanks to Mr. Khrushchev's frequent tinkering between 1957 and 1964, and his successors' economic reform of October, 1965. Here we find many fundamental dilemmas. Should the locus of innovative initiative and activity be primarily centralized or decentralized? What are the informational implications of each alternative? If primarily centralized, what should the formal economy-wide organization be? If decentralized, how are particular and social goals to be reconciled? How to best ensure appropriate motivation and the right incentives?

To take up first the centralized alternative, which is of course the traditional Soviet approach: The "territorial" principle of organization, as we have seen, severs the vertical, "branch-specific" lines of communication and thus impairs the transmission of information and pressure for technical advance and innovation. Khrushchev's attempts to establish a complementary structure of branch organs (State Commissions) to deal with dynamic aspects of planning hopelessly complicated and confused things. We have now (October, 1965) witnessed a reversion to the ministerial (branch) principle of industrial organization, although the authors of this reform must have been fully aware of its sins. But the problem is not thereby solved. How intimately integrated should planning for innovation be with planning for current production? If they are closely combined, the day-to-day pressures of production management will tend to deflect energies and attention from long-range matters, following a kind of Gresham's Law of Planning. If they are separated, the vertical lines of communication will remain at least partly impaired. How formidable will the new "departmental barriers" prove to be? In the past, innovation that cuts across industry lines has had a particularly hard time of it. (The slow progress of automation may be partly explained this way.)

More fundamental is the problem of inserting planning for innovation into the rigid, Soviet-style system of planning and management. This system rests heavily on routine, repetition, minimal disturbance, stability; it resists all new departures. But—as the sorry record of the "plans for new technology" amply shows—the routinization of innovation can be achieved only by at least partly deroutinizing other aspects

of planning, especially that of production and supply. Can it be done in a centralized system? Does Soviet-style planning at all permit of a "delicate moving balance between order and innovation"?[17] Or does it allow only gross, discrete, forced injections of innovation into an otherwise inert order?

The reforms of October, 1965, do not constitute much of a decentralization, in our opinion, despite a few steps in that direction (which is not to say that they may not eventually lead to substantial decentralizations). Unexpectedly, the most significant such step relates to investment (rather than to current production): by 1967, some 20-25 percent of all gross fixed investment in industry will presumably be undertaken in a decentralized way. Uniform technological policy is reasserted, but enterprises are called upon to exercise initiative and to innovate within its framework.[18] In the past, decentralization of investment decisions on an even much more modest scale has been handicapped by the rigidities of the highly centralized environment, especially when it came to obtaining the necessary materials.[19] Since the system of materials allocation and other centralist features are being retained, it is not clear how the much-expanded decentralized investment will now manage to fare better. Nor is it clear by what mechanism uniform technological policy will be enforced without, presumably, squashing decentralized initiative.

The irony is that, in the Soviet case, the imperative for innovation, on the one hand, and the severe obstacles with which innovation has to contend (including many of the inadequacies of information), on the other hand, derive from essentially the same root cause: the extreme historical urgency for the amassing of industrial power combined with the logic of preservation of political control by the authoritarian regime. The Soviet system is not unique, of course, in facing such grand dilemmas or such grandiose ironies. It will be interesting to see how unique will be its solutions.

[17] The phrase is Brady's [5, p. 108].
[18] See Kosygin's speech, *Pravda*, Sept. 28, 1965, and related materials; also, Gatovskii in *E.G.*, 1965:48, p. 5.
[19] We have analyzed this problem at some length in [21].

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LIBERMANISM, COMPUTOPIA, AND VISIBLE HAND: THE QUESTION OF INFORMATIONAL EFFICIENCY*

By EGON NEUBERGER
The RAND Corporation and University of Michigan

Soviet leaders appear to be dissatisfied with the present organization of their command economy—an economy that may be described as having changed from one run by Genghis Khan with a telegraph to one run by Kafka with an abacus [36, p. 23]. The organizational reforms announced by Premier Kosygin in his speech to the Central Committee of the Communist Party on September 27, 1965 [17], may be interpreted as the first stage in the revamping of the Soviet industrial system—the stage of Libermanism. The announcement of the reforms came three years after the now famous article by Professor Evsei Liberman in *Pravda*, September 9, 1962 [20]. I would characterize the reforms as potentially significant, but not radical. It is likely they will not provide a fully satisfactory answer to the Soviet leaders' search for a more effective economic system. It therefore seems useful to examine what alternatives are open to them if they should choose to make other, more radical changes.

What roads are open to Soviet leaders? In addition to the Liberman route of freeing the enterprises from some of the "petty tutelage" in deciding how to produce the goods the plans call for, and making sales and profits the major success indicators, or the retention of the present command system, there are two more radical roads. The Soviet leaders could adopt the prescriptions of Norbert Weiner's Soviet disciples, such as V. Glushkov and M. Fedorovich, and attempt to move toward an efficiently run, centralized command system with the help of mathematical models and electronic computers. Or, alternatively, they could adopt the recommendations of Soviet and East European economists such as A. Birman and O. Sik who consciously or subconsciously are attempting to adapt some features of Adam Smith's "invisible hand" to the problems faced in their socialist economies. This alternative would employ mathematical programming and computers to arrive at better aggregative plans, but disperse detailed decision making to the productive units and attempt to structure incentives to make private benefits correspond to social benefits. This would, in Marshall's words, harness man's strongest, though not necessarily highest, motives. I

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shall call the first alternative "Computopia" and the second the "Visible Hand."[1]

In order to evaluate the alternatives, I shall employ a very simple conceptual framework. Instead of talking in terms of centralization versus decentralization, or in terms of institutions and instruments, I suggest that one might look at any system of economic organization in terms of the following three questions: (1) who makes what decision—the problem of organization; (2) what information does he receive, what signals does he send out, and how does he ascertain the degree of correspondence between his signals and the actions taken by others—the problem of information; and (3) assuming given objective functions for each actor in the system, what are the incentives that will motivate him to reach a given decision, based on the information at his disposal—the problem of motivation. I believe that the answers to these three questions supply most of what we need to know to evaluate the probable static or dynamic efficiency of any economic system, assuming given natural and human resources and technological knowledge. Let me be even bolder and suggest that in seeking the optimal economic system, it may be possible to combine the three aspects into just two criteria. The best system would be one in which decision-making power is distributed to maximize the efficiency with which information is used, and which permits the structuring of incentives to minimize the divergence between the objective functions of the various primary economic units (productive enterprises and suppliers of resources) and the social welfare function (however defined). Although both of these considerations are necessary for a judgment, I shall concentrate on the first—the "informational efficiency" of various reform alternatives in the U.S.S.R. As I will make clear shortly, I am using the term "informational efficiency" to denote much more than a mere minimization of costs.

Information (the collection, transmission, processing, storage and retrieval, and analysis of economic data, the communication of orders or other signals, and the feed-back necessary for the evaluation of decisions taken as a result of the signals) is a necessary input into every aspect of economic decision making. The larger the number of participants in the economic process, the greater the division of labor, the more complex the technological processes, and the wider the assortment of goods and services an economic system produces, the more in-

[1] This name occurred to me as I read in numerous Soviet and Eastern European announcements that they wish to use the principle, "What is good for society should also be made good for every enterprise and every worker," and could not help but think of Adam Smith's invisible hand. I later found, on rereading the pathbreaking book by Professor Berliner [3, p. 50] that he had already used this name, so I grant him the prior rights to the name, without holding him responsible for my use of it.

formation intensive the economic process becomes. The increasing information intensiveness of the Soviet economy partly explains the search for new economic methods in the U.S.S.R.[2]

To evaluate the informational efficiency of an economic system, I shall assume that there are only two subsystems in an economic system: the information subsystem, I_s, and the production subsystem, P_s. The information subsystem can affect the total output of goods and services of the economy in three ways:

1. *The Input Effect.* I_s competes with P_s for factors of production, and thereby, *ceteris paribus*, lowers the output of the economy because only in P_s does any production of goods and services take place. If the amount of labor (L_i) and/or capital (K_i) (each converted to homogeneous units) used in I_s is reduced, without changing the contribution that I_s makes to the efficiency with which P_s uses its resources, the increased amount of labor and/or capital available to P_s (L_p and/or K_p) should cause an increase[3] in output—I shall call this a positive input effect.

2. *The Qualitative Output Effect.* By providing better information[4] on the relative scarcities of the various components of the final bill of goods (the true scarcity prices), without using more L_i or K_i, I_s makes it possible for P_s to attain an output mix closer to the optimal one[5] (defined in terms of the preferences of the planners or the combined preferences of the planners and households).

3. *The Quantitative Output Effect.* By providing better information on the shapes of the individual production functions and the availability of factors of production, without using more L_i or K_i, I_s makes it possible for P_s to increase output by a superior efficiency in the use of the resources.[6] In the long run, it can also affect the supply of the resources themselves and the level of technological knowledge, but a discussion of this is beyond the scope of this paper.

Pure effects are not as likely as some combinations, e.g., a combination

[2] Two recent significant contributions to the Western literature on the information problem in the Soviet Union by R. Judy [15] and L. Smolinski [35] deal with the weaknesses of the present system and with the feasibility of the cybernetic approach.

[3] L_p and/or K_p may be increased because either a larger number of given labor hours or hours of capital services are available, or because relatively high quality labor or capital is released from I_s for use in P_s.

[4] As pointed out by Marschak [23], it is essential to differentiate between the amount of information (measured by the capacity of the channel of information to transmit distinct messages) and the value of the information (measured by its payoff function). In dealing with the information efficiency of the reform alternatives, I shall assume that the value, rather than the amount of information, is to be maximised.

[5] With better information, the preferences themselves may be somewhat altered, so it is not possible to define a social welfare function completely without specifying the information available.

[6] If commodities are valued in prices that diverge greatly from scarcities corresponding to community preferences, a positive quantitative output effect may not represent a true increase in welfare.

of input and output effects, where more resources are used in I_s, but the smaller amounts of L_p and K_p remaining in P_s, are used so much more efficiently with the improved information, that total output is increased.

Changes in informational efficiency may result from (1) a change in the operation of the information system itself, e.g., by more effective use of its labor and capital due to automation—internal effects; and (2) a change in the role played by the information subsystem due to a change in the way the economic system as a whole is organized, e.g., the substitution of some market forces for some commands—external effects. Our discussion will center on the external effects, since Libermanism, Computopia, and the Visible Hand all change the role played by the information system.

Recent Reforms

The reforms in the system of management of Soviet industry, recently announced by Premier Kosygin, will bring about several changes that should affect the informational efficiency of the system:[7] (1) the abolition of sovnarkhozy and their replacement by ministries, (2) the promise of greater autonomy for enterprises in deciding how to produce, in initiating small investments, and in decisions on the particular assortment of outputs, (3) a substitution of sales and profits for the numerous present success indicators, thereby dethroning the quantity of output from its key role, (4) a greater emphasis on direct contracts among enterprises, along the lines of the Bol'shevichka and Maiak experiments [18], (5) an increased role for associations of enterprises (otraslevie ob'edineniia), (6) a reform of the price system, and (7) the introduction of a charge on capital held by enterprises. The second, third, and fourth represent the essense of what has come to be called "Libermanism," although only the second and third were originally suggested by him [20]. However, since none of the other changes conflict, in any important way, with his suggestions, I have labeled the reforms "Libermanism."

The abolition of the sovnarkhozy and their replacement by ministries changes the focus of decision making and the flow of information, but this change by itself cannot be expected to bring about a clear gain or loss in informational efficiency. The 1957 reorganization does not appear to have done this [13], and if the reforms constituted merely a return to the *status quo ante* there would be no reason to expect them to bring about such changes.

More interesting, from our point of view, are: the promise of greater autonomy for enterprises, the establishment of sales and profits as the

[7] Some changes, such as greater emphasis on operational five-year plans, the substitution of credits for grants, the increased responsibility of enterprises for nonfulfillment of contracts, the setting up of various incentive funds in the enterprises, etc., will not be discussed in this brief paper.

key success indicators, and the emphasis on direct contracts among enterprises. In these contracts the specific assortment of commodities, within broad categories set in the plan, and the choice of suppliers and customers, are based on the contracts instead of directives from higher organs. In addition, Kosygin's suggestion that ministries devolve some of their responsibilities to associations of enterprises may prove to be an important change.[8]

One of the surprising parts of Kosygin's report was his omission of any specific mention of informational problems even though these had figured very heavily in the criticisms of the present Soviet system by Soviet economists. I do not believe that this means that Soviet leaders have taken a definite anticybernetics position, but it seems clear that the purpose of the reforms is to deal primarily with the organizational variable (ministry versus sovnarkhoz) and motivational variable (profit and sales as the major indicators) and not with the informational aspect of the problem.

Despite this, I feel it is worth while to speculate about the probable informational efficiency of the proposed system. I shall start with the big assumption that the ministries will really follow the injunctions of Premier Kosygin and avoid returning to the discredited pre-1957 system of "petty tutelage" and that the party will limit its meddling in the economic affairs of the enterprises.

What is the likely input effect of the proposed changes? Lower costs for the informational tasks at the various levels in the planning system might be expected from the elimination of a large proportion of the success indicators with which the planners attempted to guide and hem in the enterprises, and from the possible reduction in the number of commodities planned from above, though the number of materials balances, which must be juggled by the various sections of Gosplan may not be reduced. A relaxation of the detailed planning of the utilization of labor would, by itself, be a considerable step toward reducing the amount of detailed information transmitted from the enterprise and analyzed by higher organs in order to include it in the tekhpromfinplan of the enterprise and to check on the adherence by the enterprise to the plan.[9] The granting of permission to the enterprise to initiate small investment projects, without having to obtain authorization, would have a similar effect.

Assuming, as is likely, that the associations would control a smaller

[8] The reason I stress the role of associations is that they have played a very important role in East Germany (the VVB) [31, pp. 28-30], their role has been expanded under the Czech reforms of 1965 [12], and they had already gained some limited importance in the Soviet Union prior to the reforms [29].

[9] It has been estimated that between 65 percent and 80 percent of the work at computer centers in enterprises in various machine-building branches deals with labor and wages [37, p. 63].

number of enterprises than the main administrations (glavki), the transfer of any given amount of decision-making power and responsibility for the control of the performance of enterprises to the associations shortens the lines of communication and may have a positive input effect. Whether it does or not depends on many factors, such as a possible reduction in the amount of information the enterprise is forced to collect when controlled by the association rather than the glavk, a reduction in the costs of transmission (which, however, is not likely to be a major saving), a possible reduction in the cost of processing and analyzing the information at the association instead of the glavk, and a reduction in the cost of checking up on the performance of the enterprise. Whether the processing and analysis costs would be reduced due to the smaller volume of data handled by the association would depend on the relative economies and diseconomies of scale in processing. These depend, in large part, upon the informational technology used and as yet no light has been shed on this.

An even more difficult, more important, and more interesting question is how the proposed reforms will affect the informational efficiency of the Soviet system in terms of the qualitative and quantitative output effects.

One of the major problems in dealing with this question is the uncertainty about the nature of the price system envisaged in the reforms. All that is known so far is that the recently formed State Committee on Prices of Gosplan was given the task of presenting recommendations by January 1, 1966, on the basic principles of price formation, with the proviso that prices approach as closely as possible to "socially necessary labor costs" and assure every "normally working" enterprise of "reasonable profits" [17, pp. 6-7].

It is not known, for example, whether the newly introduced capital charges are to be considered as costs for purposes of wholesale prices. It does seem clear, however, that with the exception of new products, where the economic effect on the user of obtaining a superior product is to be considered, neither supply-demand equilibrium prices nor linear programming shadow prices a la Kantorovich are to be considered. The Chairman of the State Committee on Prices, V. Sitnin [34], has made it clear that he favors no major change in the principles of price formation; he supports cost of production prices, with average costs of the industry as the basis.

The shift to profits and sales as the major criteria of success, given the constraints set by the wage bill plan, and the probable increase in direct contracts between consumer goods producers and trade networks will give prices a greater importance than they enjoy under the present system. Thus, the nature of the price system will be one of the factors determining the success of the reforms. Let us assume, for the

moment, that prices are reformed sufficiently to perform their allocational function relatively effectively (admittedly a very big assumption). Is it likely that any positive quantitative or qualitative output effects would result from a change in the role played by the information subsystem? I would argue that such effects might be expected, but my expectation is that they would not be very significant. I believe that this is true whether improvement is measured by an increase in output or in the rate of growth of GNP (quantitative output effect) or by a rise in the value of the final bill of goods, when measured by true scarcity prices (qualitative output effect).

There are several reasons for expecting some positive output effects, given the very favorable assumptions I have made. Decisions on how to produce are being partially shifted toward those who have at their disposal the greatest amount of the most reliable information with the least time lag. Assuming that the price signals they receive are satisfactory and that the incentives are set up correctly, as they are likely to be if profits are the success indicators,[10] it is probable that enterprises will be producing the correct assortment, at a lower cost, and with higher quality standards than they presently produce. With the weakening of the need for maintaining a "safety factor" by seeking lower plan targets or worrying about the "ratchet effect" [3, pp. 76-78], it is possible that they will even produce a larger output. Two key problems, however, are how the supply system is going to operate and whether the phenomenon of the sellers' market is going to continue. In addition to stressing the faulty signals given by the existing price system, critics of the Liberman proposals questioned whether it is possible to permit enterprises to determine freely how to produce, if their supplier enterprises are not free to choose what to produce [9] [32]. This same problem occurred in connection with the Bol'shevichka experiment when customers wanted clothing made of a synthetic fabric, but the Bol'shevichka association could not obtain this fabric [18]. The existence of the sellers' market will be one of the major factors determining the level of excess inventories held for safety purposes, the quality of output, and the introduction of new technological innovations.

I do not believe it very likely that prices will be made to correspond closely to true scarcity values, that the centralized supply system will be abandoned[11] or that the ministries, glavki, and associations will refrain from bureaucratic interference. And I consider it at least possible that the sellers' market will continue for most goods. Therefore, I ex-

[10] Merrett [26] has shown that only certain types of profit indicators will yield the correct incentives; Liberman's original ones would not.

[11] The announcement of the creation of a new State Committee of the Council of Ministers for Material-Technical Supply [38] appears to support this contention.

pect the new system to provide only limited gains over the present one, and in some cases to lead to inferior results. This conclusion differs radically from that of Professor Liberman, who, in his latest article [21], argues strongly that there are no contradictions in the new reforms and that they will work very well.

Invisible Hand

At first glance, it might seem that the purely competitive market system, the invisible hand, would have the greatest informational efficiency. It has been claimed that prices are the only information generated in this system and constitute all the information necessary for decision making [11, p. 526]. Since prices result from the free interplay of supply and demand, and there is no need for any control mechanism, virtually no resources would be needed in the information subsystem, and we would get the most favorable input effect possible. If, in addition, the purely competitive system provided the optimal output mix and the maximum efficiency of production, i.e., the static, Pareto-optimal "bliss point," and also the greatest dynamic efficiency, then we would get the paradoxical result of having the most favorable qualitative and quantitative output effects from a virtual zero allocation of resources to the information subsystem. There is no need to spend much time knocking down this straw man. It is, of course, not true that even in a purely competitive system the entrepreneurs are satisfied with the information supplied to them by the current market prices of their inputs and outputs. Even in this extreme case, they would want some information about future prices, since expectations must necessarily enter into their calculations. Second, purely competitive markets are very rare and in imperfectly competitive markets firms do invest considerable resources in information gathering and processing. In large firms, there is an additional need of information for purposes of internal control. There exists a voluminous literature dealing with the question of market failures, explaining why purely competitive markets would not necessarily yield static or dynamic efficiency or the optimal mix of final goods [2].

It might seem that this whole discussion is beside the point, since there is no possibility that Soviet leaders would agree to the institution of a purely competitive market system. The only reason for this discussion is that it examines the informational efficiency of one extreme pure model and throws some light on the probable results from moving in this direction by the institution of a visible hand regime.

Visible Hand

There is no one particular model that could be said to represent the visible hand. Instead, there are many possible alternatives, all of them

having in common one essential feature: both the automatism of the market (the "anarchy" of the market in Soviet terminology) and the rigid bureaucracy of the command system are replaced by a mixed system which harnesses man's material desires for society's benefit. Since I am primarily interested in the problem of information, I shall concentrate on the informational aspects of the visible hand.

The central planners determine the allocation of resources in aggregative terms in order to influence the development of the economy in line with their preferences (we might call this the "development strategy") and then rely, to a large extent, on a decentralized information system[12] to guide the actions of autonomous primary economic units. The essential informational feature of this approach is that the planners do not need to collect and process the billions of bits of information necessary to establish a detailed, comprehensive plan and to control its implementation. The Yugoslav system would appear to come closest to the visible hand of all existing systems. Among the variants of the visible hand that have been proposed are those by O. Lange in Poland [19], O. Sik in Czechoslovakia [33], and A. Birman in the Soviet Union [4]. The recent Liberman-type reforms may be interpreted as a partial move toward the introduction of a visible hand regime in the Soviet Union.

Birman recommends the use of mathematical models and computers at the Gosplan level to work out a central plan as a general framework for the development of the economy, and the use of physical allocations for, at most, a few dozen key commodities. However, the enterprises are to be given autonomy to produce what they want, the way they want to produce it, on the basis of market prices and profitability, within the constraints set by central financial levers like taxes, interest rates, etc.

What can be said about the informational efficiency of this type of system? Its essence is that the central planners, who have the greatest access to information on aggregate economic variables, the existence of

[12] Hurwicz [14, pp. 168-70] has provided a definition of a decentralized information system, based on the concepts of operationality and anonymity. The following description uses this definition as a starting point and takes into account some of Berliner's criticisms [14, pp. 176-78]. A decentralized information system is one which has the following characteristics: (1) a relatively large number of primary economic units, each with a considerable amount of decision-making power (the partitioning problem), (2) a much greater importance of horizontal information flows and a much lesser importance of vertical flows than is true under the present Soviet system, (3) a smaller volume of information flows upward from primary units to central planners and an even greater reduction in the volume of downward flows, (4) upward flows consist mainly of messages which are nonoperational (supplying data on internal conditions, rather than describing a set of visible resource transfers) and anonymous (planners do not care which particular unit sent the message), (5) downward flows are primarily nonoperational and anonymous (they do not constitute commands and are not aimed at particular primary units), (6) horizontal flows among primary units are mainly operational and may, but need not, be anonymous.

non-convexities, and technological developments in general, are freed from their most time-consuming present tasks—those of assuring consistency in the plan and supervising its detailed implementation—and are able to concentrate on their main task—the development strategy. The primary economic units, which have the greatest access to what Hayek has called the knowledge of the particular circumstances of time and place [11, p. 524], are given the opportunity to utilize this type of knowledge most effectively. Arrow has taken a similar position in arguing that one should accept the proposition, long recognized in practice and in management literature, that individuals in close contact with the productive processes cannot, even if they wanted to, transmit all the necessary information in all its detail to someone removed from these processes [1, p. 11].

Soviet managers, as well as any managers whose performance is judged by the data they supply, have a very strong incentive to filter and bias the information [10, pp. 58 and 27, pp. 228-29]. It is easy to see why a system in which planners do not depend on this flow of detailed information and can legitimately deal with aggregates may yield positive output effects, as well as reduce the input burden on the information system. This point has been stressed by McDonough, who has argued that a problem should be assigned to the most qualified individual or group, and "qualified" is defined as the one who is able to make the best decision in the shortest time with the smallest cost in terms of informational inputs [25, pp. 127-31].

A similar conclusion is reached by Richardson. He argues that competitive firms are limited by their inability to have at their disposal sufficient information with respect to investment actions by their competitors or their suppliers. On the other hand, central planners are limited by an inherent inability to get all the necessary information and process it perfectly. He concludes from this that the best results might be obtained, in practice, by arrangements in which both competition and deliberate coordination—public or private—play some part [30, pp. 29-34, 45].

A somewhat different conclusion is reached by Martin, who argues that it is not at all clear that the introduction of planning in a free enterprise economy will yield superior informational efficiency compared with an increase in informational outlays by individual firms [24].

In the visible hand, the informational efficiency at the enterprise level might be expected to be greater than under the present Soviet system. The enterprises would be able to use the resources devoted to the information task to analyze the internal operation of the enterprise and the most appropriate reactions to the informational signals coming from the central planning board or the market, instead of using them

to send voluminous reports and answer innumerable queries from higher organs. Thus, one might expect either a positive input effect and /or positive output effects. Similarly, it is likely that enterprises, under the visible hand would have to devote fewer resources to the information system than United States corporations, which do not have a central plan to provide them with a projection of future developments.

Although these comments would appear to argue that the visible hand approach is the best method of attaining informational efficiency, no such conclusion is warranted without a much deeper analysis of both the advantages and disadvantages of such an approach than is possible in this brief paper. The fundamental problem is that considerable dispersion of decision-making power is required to make this system work efficiently, and this brings forth the danger that the planners may not be able to ensure that the results correspond closely enough to their preferences. A number of economists have, in fact, argued that a mixed system of this type would perform more poorly than either a predominantly market economy or a predominantly command economy [e.g., 5, pp. 190-92] [36, pp. 24-25].

Computopia

The visible hand represents an effort to reduce the burden placed on the information system by reducing the volume of information that must be handled at the center. It achieves this goal by operating primarily on the organizational and motivational variables. The cybernetic route, on the other hand, operates primarily on the informational variable, to a much lesser extent on the organizational variable, and not at all on the motivational variable. Instead of attempting to lighten the burden on the information system, it increases this load greatly, but attempts to organize both the productive system and information system in such a way as to enable the information system to handle the load much more efficiently—the input effect—and to permit the use of resources in the productive sector with greater effectiveness—the output effects.

It is not possible to deal with the variety of proposals to adopt a cybernetic approach to economic reform, such as the Kantorovich shadow pricing or the Kornai and Liptak two-level planning, combining linear programming with a game-theoretic approach [16]. All I can do in this brief paper is to make some comments on an extreme approach, typified by one of the most radical proponents of cybernetization, M. Fedorovich [7] [8], even though this may turn out to be an exercise in demolishing another straw man. Fedorovich recommends a system in which all economic information is transferred to the center and processed with the help of computers and comprehensive mathematical

models of the economy. The results are transmitted as commands to the enterprises, which themselves are to be automated, to eliminate the interference by human elements. This approach would clearly require a highly centralized information system.[18]

This cybernetic system need not necessarily require a tremendous expansion in the amount of data to be collected at the enterprise level, but the composition of the data would have to be altered to eliminate many of the inadequacies of the present data [Mints, 8]. There would arise, however, considerable costs in constructing the mathematical models of the economy, developing the necessary algorithms, writing the programs for the computers, providing sufficient computer time, and transmitting data to the center and orders from the center. In addition, unless an appropriate feedback mechanism could be developed, there would be costs attached to controlling the fulfillment of commands. Thus, it is likely that it would have a negative input effect, as compared with the present system, and *a fortiori,* if compared with the visible hand.

I believe that the real issue with this extreme cybernetic approach is not in its input effect but rather in the output effects. My reason for calling it "computopia" is the belief that, even if computer experts and communications experts were able to solve the difficult technical problems, economists will not be able to solve the problem of constructing economic models that can be trusted to provide a basis for managing the economy effectively and it will not be possible to obtain all the necessary data. Even if economists were able to construct a complete model of the economy, they would be forced to simplify it drastically to make it computationally feasible [28, pp. 97-101]. The imperfect knowledge of all the relevant variables and of the values of all the parameters, the necessity to simplify, and the lag between the time the model construction began and the time the model is ready for use (U.S. experience with the construction of large models in the military sphere indicates the complexity of the task) all point to the fact that there is likely to be a significant divergence between the model and reality. In addition, a computerized system can only give good results in those parameters that can be measured, programmed, and controlled. Thus, some of the basic problems in Soviet industry, such as the quality of output and the rate of application of new technological innovations, would hardly be solved by this method. The problem of incentives is not dealt with by this approach; unless the human ele-

[18] A centralized information system may be characterized as one in which: (1) there is a predominant reliance on vertical information flows in both directions, (2) neither the upward nor the downward flows are anonymous, and (3) the upward flows are non-operational, while downward flows are operational.

ment were completely removed and all of production were completely automated, the problems of data reliability and control over the fulfillment of commands would not be solved.

Unless knowledge of the working of an economy increases much more rapidly than we have any reason to expect, it would seem much more sensible to use modern computer technology and mathematical models, as well as the proposed three-level system of computing centers [28], to deal with problems at the national level in aggregate terms, at the enterprise or regional level in more detailed terms, and to use computers as data reduction devices to transform large volumes of detailed raw data into small amounts of meaningful economic information for use at the center [22, pp. 15-16]. The basic premise is that these tools of modern technology should be used as aids to decision making rather than being substituted for human decision-makers. This general position is supported by most of the leading Soviet cyberneticians, and is expressed well by one of them, Academician N. Fedorenko: "Real socio-economic life is so complicated that, in principle, it does not lend itself to complete description. Therefore a system of direct administration will always be relatively small and insufficiently effective in conditions of tremendous volume of output, its complexity and dynamism" [6, p. 17].

I do not believe that Soviet leaders themselves know what their economic system will look like five or ten years from now, so it would be presumptuous of me to argue that I know more than they do. However, my guess is that neither extreme computopia, nor a visible hand, relying primarily on the market mechanism, are likely in the Soviet Union, and that the choice will be confined to a narrower range. The most likely system, in my opinion, is a relatively centralized version of the visible hand, in which planning by computers and models, assisted by a comprehensive network of information centers, is combined with limited autonomy for primary economic units, guided mainly by incentives. A full description and analysis of this model is better relegated to another paper.

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THE ENVIRONMENT FOR TECHNOLOGICAL CHANGE IN SOVIET AGRICULTURE

By D. GALE JOHNSON
University of Chicago

The environment for economically profitable technological change has been a hostile one during the period of socialized agriculture in the Soviet Union. Certainly this has not been the intended result. One of the reasons, though not the most important one, for several of the important institutional and organizational changes in Soviet agriculture was to overcome the presumed technological backwardness of a peasant agriculture.

The machine tractor stations, in addition to their roles in procurement and political and economic control, were to bring the most advanced machinery to the countryside. Large-scale collective and state farms were to make possible the use of efficient and advanced methods of production that were beyond the capacity of peasant farmers. And the central plans were to provide the guide lines for increased productivity and output. In addition, the support of experiment stations, agricultural colleges and institutes, and vocational training programs were to be expanded.

Farm Size: Giants and Dwarfs

At first glance it would appear that the institutional structure of the agriculture of the Soviet Union would provide for the rapid spread of new knowledge from experiment stations and other sources producing or acquiring such knowledge. For the last thirty years the total number of collective and state farms has been less than 300,000 and at the present time is less than 50,000. Thus compared to an agriculture with several million individual family or peasant farms information need reach only a relatively small number of production units. In fact, the scale of many collective and state farms is now such that experimentation could be conducted on the farms and in some cases by the farms themselves.

An important element in the hostility of the environment for technological change is that the presumptions stated in the previous paragraph represent only a half truth or at best a two-thirds truth. It is still true that about a third of gross agricultural output is produced outside the socialized sector. For idealogical reasons there has been no effort to make available applicable research results to the private sec-

tor nor, so far as I know, has any research been conducted especially for the millions of tiny units that make up the private sector. In addition, as new inputs have been produced these have generally not been made available to the private sector. In fact, the numerous restraints that have been placed upon the private sector have almost guaranteed that this sector remain technologically backward. That despite these restraints crop yields have been higher in the private sector than in the socialized sector must remain as a major anomaly to Soviet agricultural officials.

Supply of New Inputs

One of the most serious restraints upon technological advancement in Soviet agriculture is the failure of industry to provide agriculture with the appropriate inputs. In the United States, Western Europe, and Japan many significant technological advances are made available to agriculture through industries that sell inputs to agriculture. The knowledge that underlies the improved or new input may have come from public research, but the hybrid corn, the antibiotics, the herbicides, the vitamins, and the mineral feed supplements must be produced and made available to the farmer.

Soviet planning has been largely incapable either of anticipating the nature of economically justified machines or inputs such as medicines, feed supplements, insecticides, or herbicides or have failed to give these items a reasonable priority. For example, Soviet farms gain nothing from the fact that herbicides can be used to control weeds effectively unless the decision is made and carried out to produce the herbicides in adequate variety and amount.[1]

In the design and production of farm inputs the Soviet Union has not yet found an adequate substitute for the market test. Apparently recognizing that manufacturing plants could not be trusted to discipline themselves to produce machines of appropriate design and acceptable quality, given the planning rules under which the plants operated, all designs must be approved by machine testing plants. The process of getting a new design approved is apparently a difficult and time-consuming one and, in addition, has not prevented the production of a number of poorly designed machines.

The difficulties of getting a new design approved is undoubtedly a barrier to the development of new and better machines. Furthermore,

[1] Khrushchev commented on the production of herbicides as follows: "Two years ago the Central Committee and the government adopted a decision on (the production of herbicides). Time is passing and there are no herbicides. . . . Some comrades are now saying that it is costly to cultivate sowings with herbicides. But this is a temporary phenomenon. After all, we are obtaining herbicides essentially by laboratory methods." (*Pravda*, Mar. 6, 1962, translation in *The Current Digest of the Soviet Press*, Mar. 28, 1962, p. 10.)

there appears to be little incentive to the farm machinery industry to introduce new machines. Most of the time excess demand for the industry's output has existed and the introduction of a new machine or different kind of tractor may result in a failure to meet plan goals.[2] Thus there has been a strong incentive to standardize output and to produce long runs rather than to increase the variety of machines produced.

There appears to have been a general failure to recognize the many complementarities that exist in the modernization of agriculture. While there is some evidence that progress has been made in recent years, for two decades the emphasis upon combines to the exclusion of complementary inputs may have meant that the heavy investment in combines resulted in relatively little overall labor saving and may have increased the seasonal harvest peak in labor requirements. Without the auxiliary equipment for grain handling and drying and cleaning, the labor requirements for harvesting grain with combines were usually no less and frequently greater than when the grain binder was used. The grain binder allowed natural drying of the grain and made it possible to spread out threshing of the grain over several months if necessary. The combining of grain must be done in a very brief period—generally three weeks or less.

Two other matters affecting the introduction of new inputs may be noted briefly. First, there is no institutionalized way of rewarding individuals or institutes for the actual introduction of new discoveries into use. An invention or discovery is rewarded at the time it is made, rather than when the invention or discovery is used in practice. Second, even when new machines or other inputs are produced there does not exist an adequate organizational structure for the effective distribution to the regions or farms that could obtain the greatest benefit therefrom. Machines are sent to farms that do not order them, while all too often machinery that is ordered never arrives or arrives with a major component lacking such as the cutting bar on a combine. And the problem of providing adequate spare parts is still far from solution.

[2] B. G. Sivak, director of the Minsk tractor plant, discussed the problem of the introduction of new machines in this way: "Tractor plants are now putting more economical and productive machines into production, but existing planning practice does not stimulate the rapid introduction of new machines. In the past six years the Minsk Tractor Plant has produced tractors of the same type and has modernized the design and thus has consistently fulfilled the production plan while maintaining the necessary technical and economic indices. The introduction of a new tractor into production would mean, in addition to difficulties of an organizational and technical nature, increases in our own production expenditures in the form of temporarily increased production costs and larger labor expenditures. Despite the fact that new tractors would obviously be effective in the national economy, the planning agencies have not yet appropriated the money and materials needed to make them. As a result, we are in an inferior position with respect to plants making obsolete tractors. . . ." (*Pravda*, Mar. 9, 1962; translation in *The Current Digest of the Soviet Press*, Apr. 11, 1962, p. 38.)

With older machines it may be possible to cannibalize other machines for spare parts, but with a newly introduced machine this is either not possible, or is an extremely expensive way of obtaining spare parts.

A Missing Essential: Confidence

Another major element in the hostile environment for technological change has undoubtedly been the lack of confidence of farm managers in the worth of many of the changes that have been pressed upon them by Moscow. They have learned from bitter experience that many innovations have been worthless; in fact, many innovations have been more than worthless—they have increased costs and/or reduced output. Many of these disasters can be traced to Lysenko and his near monopoly of agricultural biology for almost a quarter of a century. Lysenko's innovations included vernalization of seeds, pinching back of cotton plants, the summer planting of potatoes, and forest shelter belts. But Lysenko could not have been responsible for so many wrong-headed innovations without support from the highest places. He achieved this support from Stalin, and despite a year of critical discussion following Stalin's death, Lysenko emerged once again, this time with Khrushchev's support. Lysenko achieved this support, in part at least, because his innovations promised important increases in output with little real cost to the nonagricultural sector of the economy.

But Lysenko was not the first of the oracles to receive acclaim. Before Lysenko there was Williams with his grassland rotations. The merit of grassland rotations was that they eliminated the need for chemical fertilizers—an important consideration when there were so many demands upon capital investment for industrialization.

Lysenko's theories of heredity and biology not only resulted in adverse changes but prevented others from occurring. Work on hybrid corn was halted for approximately two decades because hybridization was inconsistent with those theories.

While perhaps not technological changes in the strict sense, the numerous modifications of the concepts of desirable rotations and the pressure from planning agencies to shift from one rotation to another must have created serious doubts about the validity of almost any change recommended from above. Williams' grassland rotation, which was so long supported by Stalin, came under sharp attack by Khrushchev. Millions of hectares of hay, meadows, and pastures were plowed up and planted to cultivated crops after 1960. As a part of the same campaign, clean or black fallow was denigrated and reduced to minimal levels even in dry regions where the merits of fallow are most evident.

The forced introduction of corn into winter wheat regions bears a

large part of the responsibility for the heavy winter killing of recent years and, to some degree, for the low wheat output of 1963. Corn and winter wheat are not satisfactory partners in a rotation unless the rotation is long enough to permit planting a crop such as oats or barley between the corn and the wheat. If winter wheat is sown after corn, the winter wheat will ordinarily not be sufficiently well established to withstand the winter. Since Khrushchev campaigned so vigorously against oats, this crop could not be introduced into a corn and wheat rotation without earning disfavor. Thus in some areas a large fraction of the winter wheat was sown after corn—in recent years in Odessa Province 60 to 63 percent of the winter wheat followed corn.[3] The winter wheat was sown late—a large fraction as much as a month late—and severe winter killing was common.

Apparently one effect of the removal of Khrushchev was that the local agricultural officials took this as a signal to universally condemn corn. There are limited areas of the Soviet Union where corn provides more feed than any other crop. But apparently there was a danger that corn would be nearly eliminated from all production plans. Thus Brezhnev felt compelled to state the following on March 24, 1965:[4]

> A few words about corn. In many regions of the country this crop gives high yields both as grain and as silage, and it would be incorrect not to use its rich possibilities to strengthen the fodder base. We consider it necessary to speak about this at the Central Committee plenary session because an incorrect attitude toward corn has appeared in some places, and dashing from one extreme to the other is being permitted. Wherever it gives a good yield, it must be accorded its due place in the fields.

A Missing Ingredient: Extension Services

Earlier I noted that the small number of farms in the socialized sector should be an asset in the spread of new technology. That this has not been the case is apparently due, in part at least, to the organization of agricultural research, university or college teaching of agriculture, and the absence of an extension service. While there are exceptions, research and teaching are the responsibilities of separate institutions. Thus the training of agronomists and other agricultural specialists does not include either an emphasis upon research or exposure to the most recent research developments. When one adds to these disadvantages the fact that there was no organized system for transmitting research results directly to the farms, it is not too surprising that there were substantial lags in the transmission of research results into practice.

[3] A. Basiyel and L. Vovchenko, "Why Wheat Production Has Been Reduced in the South Ukraine," *Pravda*, Dec. 13, 1964; translation in *The Current Digest of the Soviet Press*, Jan. 6, 1965, p. 11.

[4] *Pravda*, Mar. 27, 1965, pp. 2-4; translation in *The Current Digest of the Soviet Press*, Apr. 14, 1965, p. 7.

Khrushchev's description of an effort to create an effective extension service merits quotation:[5]

> I will reveal a "secret": When the Central Committee effected the reorganization of the management of agriculture, we borrowed something from the practice of America. I have in mind the scientific service, the introduction of scientific achievements. What is valuable in the American practice of organizing scientific service for the farmers? Concreteness, a businesslike attitude.
>
> In the United States of America a farmers' service exists under the universities and colleges. . . . How does this service work? It has inspectors, or agents, as the Americans often call them. The agents meet with the farmers and persuade them to introduce this or that method, help them in the organization or production and furnish them with plans and recommendations. . . .
>
> The farmer pays for the scientific service. This payment is in the form of a tax levied in the state budget. The farmers' funds are used to pay for the specialists who serve farm production.
>
> . . . If the farmer does not receive concrete help from the agent, he no longer turns to him and the agent has to quit the service.

The reorganization that Khrushchev referred to was the establishment of the production administrations in 1962 which were charged, among other things, with bringing new scientific developments to the attention of the farms. Among the other things for which the production administrations were responsible were procurements and plan fulfillment. It is obvious that Khrushchev did not understand all of the secrets of the land grant system and the extension service. He did not understand that one of the unique features of the extension service was its disinterestedness with respect to government policies and programs. It is of no concern to the county agent that the government wishes to restrict corn or wheat production; if he can show the farmer how to increase production of these crops and profit as a result, the county agent will do so. Nor are the agents expected to engage in propaganda activities in support of government programs.

But the inspectors or agents lodged in the production administrations were clearly a part of the government apparatus that controlled agriculture. They must have been considered with the same suspicion as the rayon party chairman or a tax collector.

The United States almost made the same mistake as did Khrushchev. When the Agricultural Adjustment Act came into being more than three decades ago, considerable pressure was put upon the land grant universities, including the extension service, to accept the major administrative role in the production limitation and related aspects of the program. The universities fought this effort and their view prevailed that they must remain independent and in a position to criticize and evaluate what was being done.

The recent reestablishment of the Ministry of Agriculture as the major agricultural planning and control agency does not appear to im-

[5] *Pravda*, Mar. 7, 1964; translation in *The Current Digest of the Soviet Press*, Apr., 1964, p. 6.

prove the situation with respect to the objectivity of the advice provided to farms. Matskevich, the new U.S.S.R. Minister of Agriculture, described the functions of the ministry as follows:[6]

> Until very recently the ministry's only role was to guide agricultural science and to train agricultural and veterinary specialists. Now . . . [it] has been charged with directing the development of agriculture and with responsibility for the state of affairs on the country's collective and state farms. Its major activity will be to satisfy the requirements of production.
>
> The ministry will continue to devote much attention to agricultural science.

Matskevich noted that the V. I. Lenin All-Union Academy of Agriculture was to be strengthened and that the academy would have jurisdiction over the leading branch research institutes. He further noted that the Union-republic ministries of agriculture will have administrations for directing agricultural research. He added: "But the majority of the province experimental stations will be under the jurisdiction of the province agricultural administration and will solve the scientific and practical tasks of the particular province or territory."

Only time will tell whether this structure will gain the confidence of farm managers and technicians. If it does not result in freedom to conduct research and to freely publicize the results of the research, without regard to procurement goals or the views of the local or national administration, it is unlikely that the necessary confidence will be created.

Irrational Planning

Irrational planning has often offset or more than offset any technological gains that may have been achieved. It might be argued—and it has been—that in U.S. agriculture today the gains to be achieved by striving for efficiency in terms of achieving maximum output from given inputs in a specific state of technology are very small compared to the output gains flowing from advances in technology. If this is true of the U.S., it is not true in Soviet agriculture.

A major plan goal in Soviet agriculture has long been the number of livestock. The emphasis upon numbers of livestock rather than upon output only has resulted in inefficient use of feed. In 1964 Khrushchev reported that it took 10 to 12 fodder units to produce a kilogram of pork—twice the official norms and, though not noted directly by Khrushchev, almost double the amount used in the U.S.[7] One of the reasons for the low feed efficiency was that it took a year or more to

[6] *Trud,* Apr. 4, 1965, p. 4; translation in *The Current Digest of the Soviet Press,* May 19, 1965, p. 10.

[7] *Pravda,* Apr. 24, 1964; translation in *The Current Digest of the Soviet Press,* May 20, 1964. Another source indicates an even more gloomy picture (*Ekonomika sel'skogo khoziaistva,* 1965, No. 5, p. 22). To produce a pig weighing 90 kgs. took 380 days in 1958 and 540 days in 1963. These data apparently refer to performance on the sovkhozy.

produce a pig weighing 100 kilograms compared to half that long in the U.S. While part of the reason for the long feeding period has been the limited availability of high protein feeds, much of the reason has been the pressure to meet goals for the number of hogs on farms. Other livestock have been similarly affected. A complaint by a farm manager in July, 1964, merits noting:[8]

> Our collective farm once had 18 to 20 dairy cows per 100 hectares, with an average yield of 3,700 kg. . . . we had about 400 head, which fully provided for the sale of the established milk norm to the state and consumption within the farm itself.
>
> But a few years ago the district set a control figure for us: We must have not less than 800 cows! What could we do? The plan is the plan! We increased the number of cows to the figure set by the district but the milk yield fell to 2,000 kg. per cow. It turned out that while the herd was doubled, there was no longer enough conditioned fodder for the cows. . . .

The new agricultural leadership has stated repeatedly that it is only output and sales that counts. But has the lesson that livestock number goals are inconsistent with efficiency and maximum livestock output been learned? I do not think so. I will have been proved wrong if the next plan omits any mention of goals for livestock numbers. If it does, the underlying inconsistencies will not have been removed.

Brief notice may be taken of two important departures from efficiency in the use of resources. First, a recent article lauds the advantages of moldboardless or disc plows in the virgin land areas.[9] It was only a few years ago that Maltsev's deep plowing—plowing up to a meter deep—was being recommended for the same region. Deep plowing not only did not increase yields on the average—it probably reduced yields in areas with thin soils—but it was extremely costly in terms of fuel and wear and tear on the tractors and in labor used. Second, seeding rates for wheat and other small grains have always been high in the Soviet Union compared to Western Europe or North America. However, in recent years sowing norms have been increased to astronomical levels—in many cases to 300 to 350 kilograms per hectare. In many instances the amount of seed used would equal a fourth to a third of the gross harvest in a good year and half or more in a bad year.[10] It is almost impossible to believe that such inefficiency can be imposed by rational men.

[8] Makar Pzomitny, "Thoughts About the Executive's Role," Literaturnaya gazeta, July 2, 1964, p. 1; translation in *The Current Digest of the Soviet Press*, Sept. 2, 1964, p. 10.

[9] M. Gendelman, "Place Economic Analysis at Basis of Production," *Pravda*, Oct. 25, 1964; translation in *The Current Digest of the Soviet Press*, Nov. 17, 1965, p. 33.

[10] "Time Separates the Wheat from the Chaff," *Komsomolskaya pravda*, Nov. 19, 1964, p. 2; translation in *The Current Digest of the Soviet Press*, Dec. 16, 1964. Can it be that Khrushchev's demise saved Soviet agriculture from a catastrophe equivalent to that of the vernalization of seeds? The answer provided in the article for the high seeding rates was the soaking of seeds in a nutrient solution prior to seeding, sowing in rows, and cultivation to control weeds. The idea, interestingly enough, was apparently opposed by kysenko and the All-Union Academy of Agricultural Sciences.

Concluding Comments

I have argued that the environment for desirable technological change in Soviet agriculture is a hostile one. There are some modest exceptions that I have not noted. These exceptions are to be found in the industrial crops such as cotton, sunflowers, and sugar beets. Substantial successes in research have been achieved and many of the improvements have been adopted by farms. But these crops require only 6 or 7 percent of the total sown area.

And, of course, not every aspect of technological improvement in the rest of agriculture has been so badly handled as I may have implied. But on balance, the Soviet Union's experience in developing or adapting new techniques of production has been responsible for a significant part of the unsatisfactory performance of Soviet agriculture. While the huge size and the organizational structure of state and collective farms have adverse effects upon efficiency and output, the numerous and costly mistakes in the promulgation of unsound techniques, the failure to provide inputs in the variety, quality, and quantity that was possible, and the efforts to dictate methods of production, such as rotations, tillage practices, and herds of livestock, on a national basis have perhaps been as serious deterrents to productions.

DISCUSSION

Alexander Eckstein: I find myself in a rather unenviable position for a discussant; there is very little in these two most interesting papers that I can disagree with or criticize. Therefore, rather than assume the role of an assailant, let me shift gears and examine some of the issues raised in these papers from a somewhat different vantage point, particularly by focusing explicitly on the stage of development dimension of the problem.

Professor Neuberger correctly observes that "the larger the number of participants in the economic process, the greater the division of labor, the more complex the technological processes, and the wider the assortment of goods and services an economic system produces, the more information intensive does the economic process become. The increasing information intensiveness of the Soviet economy goes part of the way toward explaining the search for new economic methods in the U.S.S.R." This increasing information intensiveness is closely associated with industrialization and economic development. Increasing complexity of the economy under the impact of industrialization, however, does not only affect information processes, but organization of decision making or "partitioning" of economic organization, incentives and motivation, as well as attitudes toward innovation.

Moreover, rising complexity borne by industrialism also means growing structural interdependence which greatly complicates the task of attaining consistency and efficiency in a command economy. At the same time, growing complexity in industrial structure tends to be accompanied by increasing complexity in social structure. Industrialization and urbanization leads to much greater occupational differentiation and the rising importance of scientific, managerial, technical, and professional groups, highly responsive to differing reward systems.

Therefore command methods of economic management, which necessarily represent very blunt instruments for resource allocation, will compound the inefficiencies in the system to the point where they may slow down economic growth quite significantly. In effect, at these more advanced stages of economic development, it will be increasingly difficult for the bureaucratic decision-making system—however partitioned—to yield information adequate in quantity and quality to enable planners to make the "right" allocative decisions and to emit signals which would motivate the economic units in the system to yield the outcomes desired or required by the planners.

As pointed up by both Professors Grossman and Neuberger, in essence, this lies at the heart of the dilemmas facing the top policy-makers and planners in the Soviet Union today. The problem, of course, is not new; but at an earlier stage, the inefficiencies, while pervasive, were less serious—in part because the economic structure was simpler and in part because large-scale technological borrowing from abroad combined with high rates of investment outweighed these inefficiencies in current production.

Similar dilemmas, and for similar reasons, face economic policy-makers in

Eastern Europe. Czechoslovakia might be cited as an outstanding example. Not too surprisingly, command methods of economic management have repeatedly broken down in the Czech economy. As the most industrialized and economically advanced member of the Soviet commonwealth, it was naturally exposed to all of the dilemmas outlined above. However, these were compounded and greatly aggravated in an open, foreign-trade-oriented economy. In such an economy, inefficiencies arising from command methods of economic management undermine the country's competitive position in world markets and thus lead not only to marked slowing down in the rate of economic advance but to outright economic stagnation. It is not surprising, therefore, that pressures for economic reform have been most pronounced in Czechoslovakia.

The issue at hand can perhaps be brought into even bolder relief if we contrast the situation in the Soviet Union, Czechoslovakia, and other East European countries with Communist China. Interestingly enough, economic slowdown and stagnation in China has not produced the kind of pressures for economic reform that we are witnessing in other command economies. It would be erroneous indeed to conclude from this that the economy of Communist China is not subject to widespread inefficiency. Rather, given China's economic backwardness, static or short-run efficiency considerations are overwhelmed by deep-seated structural problems revolving largely around population and food supply. The crucial variables in this context are the rate of technological progress in agriculture, the character of agricultural policy, and the amplitude of annual harvest fluctuations.

Thus, in spite of the planners' preferences, the fate of the economy and the course of economic advance is determined by the suitability of the management structure, information systems and incentives in agriculture; and this seems to be much more so than was ever the case in the Soviet Union or Eastern Europe. One could almost be tempted to suggest that in the Soviet case, rapid economic growth and structural transformation occurred regardless of what happened in agriculture, while in China, the position is reversed.

Earlier, I referred to the pressures for economic reform generated by command economies as they approach higher stages of economic development. However, these pressures for reform pose some profound dilemmas for the Communist regimes in these countries as illustrated by the Soviet Union, Czechoslovakia, and in a very different way by China. In a sense, they are engaged in a continuing quest for improved organization in decision making and gains in informational efficiency without sacrificing control over the allocation of resources. Another way of stating the dilemma is to focus on the Communist regimes' quest to enforce the planners' scale of preferences amidst sharp, or possibly even increasing, divergence between these and consumers' preference scales combined with a growing assertion of consumers' preferences. This then immensely complicates the task of designing a structure of incentives and organization which will evoke appropriate motivation (from planners' point of view) and ensure the implementation of the planners' decisions. In a sense, the sharper the divergence and the more assertive the consumer is, the more complicated or even insoluble does the task become.

In the face of this situation, planners can follow two alternative strategies

or a mix of the two; namely, bureaucratic decentralization or economic decentralization. Unfortunately, these two types of decentralization strategies are often not sharply enough distinguished. The first involves different methods and degrees of delegating decisions within a command framework. In effect, it may entail alternative ways of "partitioning" decision making within the administrative subsystem. This must be contrasted with that type of decentralization which involves a transfer of economic decision making from the administrative subsystem to the economic subsystem. The latter does not only entail delegation of decisions from upper level to lower level command organs, but rather delegation by the command or administrative subsystem to the economic subsystem. Concretely, this means delegation to the price and market mechanism or, as Professor Neuberger calls it, to the visible hand.

Thus far, the Soviet, Czech, and other East European planners have mostly experimented with the first type of decentralization, while they have strongly resisted the second. This is not too surprising since movement in the direction of the latter would mean gradual abandonment rather than just restructuring of the command system itself.

JOSEPH S. BERLINER: The papers by Neuberger and Grossman direct our attention to certain aspects of the Soviet economy that are the object of the recent wave of reform proposals. Both authors have responded dutifully to the invitation to consider the economy from the point of view of the information system. But I suspect that in submitting to planners' preferences, they found themselves saddled with an analytic notion which is perhaps not the most fruitful for the problems at hand. Let me first comment on some difficulties with the concept of informational efficiency, and then proceed to those portions of the papers where the authors deal with more central issues.

Grossman deals with the generation of new knowledge, and since the output that may be yielded by new knowledge is potentially limitless, he eschews any formal concept of efficiency and explores instead the less formal notion of "obstacles" to innovation. Neuberger's assignment, however, is to take the stock of knowledge as given, which permits him to employ a formal standard of informational efficiency, to be measured as the ratio of actual output to potential output. I find the argument a bit difficult to follow, however, for while the concept of informational efficiency is discussed at various points, it is nowhere precisely defined. It appears to me that in fact three distinct and independent notions of informational efficiency are used, which may be usefully distinguished. The basic idea is that an economy may select one of several kinds of production-organizing systems, and one of several kinds of information-flow systems. The first concept of informational efficiency is that whatever the information system selected, it should be operated at minimum cost. We may call this "technical" efficiency. The second notion asks whether the economy has selected from all possible information systems that one which, in association with the given production system, best serves the function of providing the necessary information. The information system is then "functionally" efficient if, in association with a given production system, it yields the maximal output. The third notion evaluates not only the choice of an information

system but also the choice of a production system such as visible hand or computopia. This most general notion may be called "economic" efficiency. An information system is then "economically" efficient if, in association with a given production system, output is greater than could be attained by the combination of any other information system with any other production system.

These three meanings of informational efficiency underly the various "input-output" effects and "external-internal" effects with which Neuberger conducts his analysis. But while they are of some use in clarifying our thoughts, I am not sure that they or Neuberger's concepts are very powerful instruments for analyzing the economic reforms, or even identifying all the costs. Consider, for instance, the Liberman reform. Neuberger points out that the reform is likely to have a salutary "input effect" (technical efficiency) because many of the enterprise planning indicators will be eliminated and the number of centrally planned commodities will be reduced. But the reduction in the quantity of information flowing vertically will be offset to an indeterminable extent by increases in the lateral flow of information. For if the central planners no longer receive the information or authority with which to arrange the distribution of a commodity among producers and purchasers, the buyers and sellers will have to generate and transmit the information among themselves. The former system of centralized distribution will be replaced in part by an expanded system of technical catalogues, advertising, and sales organizations—all of which, as Grossman points out, are inadequately developed even under the present system of economic organization. Thus the question of whether the new information system will absorb more resources than the old requires quantitative data for the answer and cannot be answered on a priori grounds alone.

But it is not primarily because of difficulties in applying the standard of informational efficiency that the concept does not appear to be very productive in evaluating the proposed reforms. It is rather that the favorable consequences to be anticipated from the reforms are not due primarily to an improvement in information, but, as Neuberger points out, to the opportunities for more effective utilization of information. And Grossman strikes a similar note when he asserts that it is not chiefly the poor quality of economic information that is inimical to technological progress but rather "the lack of appropriate motivation for innovation at the enterprise level." As I read the two papers, it appears to me that managerial motivation is the key to the thorniest problem in both the use of current technology and in the development of new technology.

First, on the matter of current economic organization, Neuberger's preference for the visible hand system can be supported primarily in terms of incentives. One of the lessons we have learned about incentives in socialist countries is that peasants, workers, and managers all respond vigorously to income opportunities. The evidence is to be found in the vitality of the private plots and in the success of piece-rate wage systems and managerial bonus schemes. The predictability with which managers accommodate to a new bonus scheme may be compared to the fidelity with which a compass searches for the magnetic north. The defect of centralized planning is not that the compass did not

work, but that the navigational theory was wrong. The defect of computopia, on the other hand, is that it fails to employ at all the marvelous property of its human compass. Indeed, to change the metaphor, the manager himself is a wonderfully efficient computer, programmed to maximize the value of any function fed into him that varies positively with income. If centralized planning failed to discover the appropriate function, computopia would make no use at all of the managerial computer, but would substitute an electronic instrument of its own. The virtue of the visible hand as a system of economic organization is that it would take full advantage of the income-searching propensity of the managers.

The case is even stronger when one turns to Grossman's problem of innovation. Innovation involves both great risk and great effort, and both need to be rewarded. The point itself has never been in dispute. What has been the bone of contention is the size of the reward needed to elicit various quanta of innovational activity. I believe the evidence is mounting that the rewards have been far too small in the socialist countries to elicit the desired rate of innovation. Let me cite in passing two pieces of evidence: (1) Grossman notes that research and development have been routinized in capitalist corporations no less than in the Soviet economy. That the innovative results are greater in the former may reflect the greater potential personal rewards from innovation. (2) The Czech economy is reported to have achieved the most egalitarian income distribution in Eastern Europe. It is reported that not only are the rewards for managerial excellence small, but the loss of income from demotion to the rank of engineer is so small that some managers regard demotion as a welcome relief from responsibility.

It has long been established that Soviet pricing practices have undervalued the contribution of capital and land. But perhaps the most seriously underpriced factor has been entrepreneurship. If there is merit in this argument, then the appropriate policy is the creation of a class of Soviet millionaires. There is precedent for this policy, for there are so-called "millionaire-collective farms" (though not farmers), and there may perhaps be millionaire artists and writers. Nor are the ideological barriers any more intractable than those currently giving way to the force of the arguments for interest on capital, rent on land, and profit-based bonus systems. Socialist income-distribution theory has indeed been based on the principle that one's income ought to be related to one's contribution. In implementing the policy, however, neither the full value of the contribution of entrepreneurship, nor the income possibilities needed to elicit it, have been fully appreciated. If my comment has merit, then the reforms in information and organization discussed this morning will fall far short of the mark unless accompanied by an extensive reform in income-distribution policy. The statue of Schumpeter, and perhaps of Emerson, too, will have to stand beside that of Von Mises if better socialist mouse-traps are to catch better socialist mice.

MONEY AND BANKING: INNOVATIONS IN FINANCE

EFFECTS OF AUTOMATION ON THE STRUCTURE AND FUNCTIONING OF BANKING

By GEORGE W. MITCHELL
Board of Governors of the Federal Reserve System

The commercial banking system in the United States owes its existence to its ability to render for all sectors of the economy a unique and pervasive service; namely, the movement of money. This transfer of funds between debtor and creditor, buyer and seller, citizen and government, employer and employee—in fact from anyone with a balance in a bank to any identifiable payee—is ever ready, safe, and convenient.

Banks consummate money settlements through an elaborate clearing system for transferring deposit balances from one account to another in the same bank or in different banks.[1] The principal tool used is the ordinary check. The mechanical and institutional channels by which 60 million checks daily find their way physically, and with appropriate accounting entries, from any one to any other among 70 million accounts is vastly more complicated than might be thought. It achieves a high degree of ultimate accuracy considering its predominant dependence on manual processing. The service is far from instantaneous, however, being no faster than present-day methods of transporting checks in considerable bulk about a city or throughout the country.

In the aggregate, the annual cost of operating the present check and settlement system for the entire country can be estimated at about $3.3 billion. Some of this cost is borne by the Federal Reserve System, some is paid in the form of service charges assessed on the account of the payor, and some is "clipped" from the face of the check instrument itself and charged the payee when nonpar banks are involved. However, the bulk of the total cost is absorbed by the commercial banks themselves to attract balances that can be invested in interest-earning assets.

Given the cost and time pressures inherent in the present check set-

[1] Commercial banks also have an important function to perform in the only alternative procedure for settling the economy's accounts; i.e., by the use of coin and currency. They maintain at significant cost local reservoirs of cash for the use of their businesses and individual customers who can withdraw amounts and denominations when required and return to the pool unfit or excess holdings.

tlement system, it is not surprising that the initial bank efforts to exploit the infant prodigy of electronic data processing have been aimed mainly at lowering the per-item handling cost and speeding up the performance of old-style check collection. This is constructive, so far as it goes, but it falls far short of the potential transformation of banking services rendered possible by automation. Laying aside all the undoubted strains and pains of the transition, it is practical to envision the advancement of the state of the art to a point that will permit—and perhaps almost force—radical change in banking structure and functions. This state will be reached within the discernible future, probably much sooner than most of us expect.

By that time, I expect, check usage as we know it will have largely disappeared, and the intricate process of settlement and deposit accounting will be carried on concurrently at and between 250 or so computer centers located throughout the country.[2] A modified giro system will be used, in which the payor will initiate the settlement process, but will do so by communicating, not with the payee, but with his bank—notifying it directly whom to pay, how much, and when.

Most of this information will be received at the bank in machine language; if not, it will be converted to that form, and the bank's computer will process the bookkeeping entries internally for amounts drawn on it. If one computer handles the accounts for several banks, the operation is still almost entirely an internal one. If payment is to an account in another bank, the information will be automatically routed into that bank's equipment. Bank positions will also be adjusted frequently throughout the day by debits and credits to member bank accounts with the Federal Reserve System. The computers will transmit printed-out confirmations to the payor and advices to the payee at appropriate intervals. The print-outs could be transmitted by mail or telephone wire, at the option of the customer. In the case of larger customers, the bank's computer will communicate directly with customer's equipment.

In this system there is no check sorting and re-sorting, no shipment of checks from bank to bank or bank to customer, no storage requirements for checks, no kited checks, no endorsement, no N.S.F. checks, no float, and a minimum of manual processing. Of course, different problems may later come to light. The machine must work; and the bank must make sure it is being instructed by the owner of the deposit.

[2] The number of computer centers given is conjectural but compromises an optimum operational size and a convenient geographical area. The very rapid growth of computer centers recently seems to presage the automation of all demand deposit accounting in the near future—using checks or any other settlement media. Banks will have a choice of their own equipment, a correspondent's facilities, a cooperative processing organization, or a commercial service bureau.

There is no reason, however, to fear that any such potential difficulties are beyond the technological capacities and, probably, the cost horizons now in view.

Furthermore, it seems logical and practical that at least some of the customer accounting antecedent and subsequent to settlement could be most economically done in a coordinated package with the settlement accounting. Every sales transaction, for example, by specifying a settlement date, might immediately be put into the bank's computer where it could accomplish immediate settlement or subsequent reminder and settlement. Similarly, a bank could handle payrolls and agree to bill and process many types of contractual payments for insurance, rent, and mortgage payments. In short, by virtue of its central position in the payments process, the bank is also able to perform ancillary and antecedent accounting and billing operations more economically than anyone else.

For willing business customers, the bank's service could include a large part of the accounting, analysis, and financing of receivables and even extend to provision of much current cash flow accounting—a basis of analysis that has become of increasing importance in both business and financial planning.

Tied into the possibility of, if not a prerequisite to, expanded service for most depositors is the introduction of a depositor combination cash-credit card. This device could be used for immediate payment, partially replacing the use of coin and currency, or it could be used for the processing of convenience credit or the scheduling and liquidation of installment or revolving credit.[3]

All of these ancillary operations enhance profitable business prospects of a computerized settlement system outlined above. Obviously they and similar extensions of service have an important bearing upon the alacrity and enthusiasm with which banks will convert to or adopt EDP systems.

If the foregoing projections are realistic, they seem to promise, in the aggregate, a substantially more efficient settlement mechanism. And they imply additional profit opportunities for banks that can combine settlement with receivables accounting, payroll accounting, credit card operation, and a consumer credit system for depositors.

[3] The cash card involves some exposure to theft or counterfeiting but various identification devices such as voice "fingerprinting," or other technological developments now under study appear adequate to control losses. Security may be more importantly achieved, however, by the positive identification of all payees inherent in the system. To be a payee one must have an account with a bank and have met whatever identification and responsibilty standards are found to be necessary for the protection of payors. There are no intermediate signatories in the system. Once the payor has directed his bank to make a given payment and the bank is satisfied with its identification of the payor, then there is no opportunity of intercepting and misappropriating the "document" used for this purpose.

Today, no one really knows how much cost reduction, private and social, a fully computerized system might achieve. Some of those with the earliest and most extensive experience in partial EDP applications are taking a hard, if not skeptical, look at "hardware" costs of a full-scale operation. One of the difficulties of bringing the relevant evidence together is the problem of totaling up actual private costs in our present settlement system which could be eliminated or reduced in the "checkless-cashless" society. Another is the allocation of the new system's public and private costs among various public and private beneficiaries. If public costs presently involved in the distribution of currency and processing of checks, for example, are substantially reduced, how can the entrepreneur banker who brings this about collect a *quid pro quo?*

The question, then, of whether commercial banks will regard EDP as an opportunity for profitable service is not easily answered. The banking system is not distinguished for its innovative achievements—despite evidence of improvement in recent years. Its adaptability to change is hampered by regulatory constraints on structure and function. Entry, branching, and merger are closely regulated, as are prices paid for deposits and many of the conditions under which credit can be extended. Thus, there is a tradition of conservatism in management reinforced by competitive sheltering and regulatory constraints that act as inhibitions to innovative steps with any evident structural or functional consequences.

This opens the way for nonbank enterprises to become well established in the EDP record-keeping applications antecedent or related to settlement before banks even enter the field. If they do, their customers are likely to be relatively indifferent to tardily offered adjuncts to a banking settlement system.

The deferred entry of banks into the consumer credit business is a case in point. Today, credit for consumers is relatively independent of the banking system though it is also available there, and generally for less. However, even today, seldom does a bank's consumer credit system exploit the natural advantages of a continuing depositor relationship in the way that vendors, for example, have exploited the continuing patronage of their customers. Moreover, consumer credit is a natural extension of other banking operations. A bank's individual depositors use credit extended by its retail firm depositors with the proceeds of trade-credit bank lines. Vendors have established practical standards of credit worthiness for their customers and profitably priced the credit extended. At the most, very few banks have offered an aggressively competitive alternative to vendor credit by carrying the

credit financing from producer or wholesaler through to the ultimate consumer.

As an industry, banks have moved into consumer credit far more slowly than vendors; they have not been innovative and, by and large, have made almost no use of their key position in settlement accounting to provide services more broadly and economically than anyone else.

Past experience suggests, therefore, that the banking system may well be reluctant or inhibited from exploiting the opportunities inherent in automation, particularly if more aggressively-minded EDP machine sellers or users can carve out large sectors of potential service in which, through lower costs and innovative flexibility, they can establish customer loyalties.

But if skepticism borne of experience leads me to expect opportunities to be missed in this area, logic compels me to insist that such a fate is not inevitable. A clear enough vision of future possibilities exists in some banks today. Hammered home hard enough by both intra-industry communication and private advice, it might serve to erode much of the inertia and inhibition now forestalling a full-fledged revolution in the settlements mechanism.

If this should happen—and assuming public policies are accommodative—not only banking services but also banking structure could be literally transformed. Profound structural changes seem almost a certainty. Automation can and will burst the locational constraints that are implicit in federal conformity to the provisions of fifty state banking laws pertaining to branching. Not only will metropolitan area-wide banking operations become commonplace everywhere at the option of bank managements, but remote control banking state-wide, and even across state lines, will also be feasible, limited by little more than the telephone toll costs of servicing more distant customers.

To be sure, banks have detoured branching restrictions for some time, as large banks in various parts of the country have solicited and made loans all over the United States—or over the world, for that matter—and have accepted deposits by mail or wire from customers wherever located. But this sort of substitute for branching is a reality only for large accounts. The dynamic change that will come into being is that computerizing the demand depositor-bank relationship will make it practicable and in all probability economically profitable for banks spectacularly to extend their present service areas for small and medium-sized accounts. This they can do, by using the U.S. mails or by hooking their computer onto a local telephone in any community they wish to serve.

The same features of automation that will enable banks to achieve

many of the advantages of a far-flung branching system will also introduce a large element of obsolescence into many existing branch facilities. Branches that have been established to achieve proximity to depositors and are essential from the bank's standpoint only because they facilitate the sweeping up of loanable funds or minimize deposit fluctuations by more nearly encompassing the local payments cycle will become superfluous.

Depositors will have no need to visit their banking office any more often than they now visit their telephone or electric utility company office. They will not be making deposits of checks; rather their bank will notify them of credits to their accounts. They will rarely find it necessary to go to their bank to obtain cash, even for transactions that are now typically made with cash. Their credit line will be activated automatically. Their cash-credit card will be the equivalent of cash at a supermarket, the cleaners, or a department store. The coin and currency required for transactions that will continue to be most conveniently handled in that fashion will be supplied from commercial establishments that are regularly serviced by money truck pickup and delivery.

Perhaps there will be a place for "baby branches"—small field offices which might serve as headquarters for account salesmen and loan officers, and for performing custodial, certification, and routine financial advisory services, but it is hard to visualize the typical branch office in existence today as fitting into a computerized banking institution of the future.

Automation in banking will likely have lesser effects on such traditional types of bank credit extension as farm lending, mortgage lending, and loans to large businesses and to financial enterprises. But it should have a major impact upon consumer credit and trade credit between firms, particularly of small and medium size. These types of credit involve substantial investigative and bookkeeping costs relative to interest earned on a typical loan or line of credit. They also involve more surveillance and more losses, though of a readily insurable type, thus adding further to overhead costs.

With automation, banks can offer a credit system which ties settlement accounting into quasi-automatic credit extension; this combination has great operating advantages over other arrangements available to vendors or independent consumer finance companies. A bank depositor credit card is of superlative convenience for the purchaser when he can use it anywhere and in doing so express his preference for cash payment, convenience credit, installment credit or any combination of the three.

The bank, in offering this service, can extend credit to seller or buyer, or both, on the basis of prearranged lines—lines that have been

fixed with access to unparalleled sources of information on the customer's financial activity and responsibility. Moreover, the computer continuously updates this information and can alert the bank's credit department on a timely basis to the emergence of credit abuses by whatever standard the bank may choose to employ. Imagine the convenience of a continuing scrutiny of the customer's cash inflow and outflow in relation to use of bank credit, and all monitored by a sentry who reports instantaneously!

Such a system would not be without losses, to be sure, but they could be controlled by fixing maximum credit lines for various types of accounts. And loan limits could serve another purpose: that of fostering larger demand deposit balances. If a line of credit, for example, were some multiple of average daily balance, it is quite likely that most depositors would gladly pay the "commitment fee" for the convenience and prestige of bank credit. And still another advantage so far as banks are concerned is the conventional preference of many bankers for self-liquidating short-term credit, met in this instance by the rapid turnover of consumer and sales credit of the type envisioned.

Particular beneficiaries of this more flexible and better disciplined credit use should be the many self-employed in the economy, ranging from part-time salesmen to proprietors. While their aggregate credit use is a small factor in bank lending, a significant public interest is served by uncovering any method of economically and conveniently making more bank resources available to them. Under current operating procedures, the overhead costs associated with such credit, when added to a regular interest charge, entail effective interest rates that are prohibitive or appear highly discriminatory. Automation offers a method of minimizing overhead costs and probably reducing risk, thereby making bank credit more accessible to a sector of the economy that has found all sources of credit "high priced."

In their continuous search for loanable resources, banks will find automation a far keener tool than they are accustomed to using. On the one hand, it will enable them to attract demand deposit customers with assurance of a simpler, safer, and more convenient means of payment than has ever been offered. Not only will the computer reduce the risk in paying bills, it will also take over the chores in banking—such as a trip to the bank and the standing in line to make a deposit, the writing of checks and mailing them to creditors, and similar routine tasks. Moreover, it will give the depositor "instant bookkeeping," as he will be able to find out as often as he likes the exact status of his account, and with that knowledge give the bank instruction as to whom to pay, and when. Thus he is enabled to manage his money position as closely as he likes.

From the standpoint of profitable operations, banks that offer a ser-

vice making possible the close management of customers' bank accounts are almost certain to find their demand deposit totals wasting away as a manifestation of the automation program. Moreover, given the capabilities of a computerized economy, more frequent settlement periods are likely, if not certain, to come into widespread use and this development will diminish still further the size of a comfortable operating balance for the typical depositor. Just as weekly wage and salary payments go with a lower operating bank balance than is needed when payments are monthly, the shorter interval made possible by automation will call for even smaller cash balances.

Given declining need for demand deposit balance for these "technological" reasons, banks have the alternative of establishing fees to cover at least a portion of the costs incurred for processing flows through demand deposit accounts or of establishing minimum balances commensurate to the scope and cost of services rendered. If they rely heavily on fees, then in order to maintain their aggregate of loans and investments banks will need to attract time deposits (or to borrow) in one form or another in competition with other banks, other financial intermediaries, and the capital markets. The compensating balance alternative, on the other hand, if enhanced in appeal by linking it to a packaged credit line, would enable banks to minimize losses in demand deposit balances.

Other changes consequent to the automation of money flows—such as operating space and labor requirements of banking institutions—involve formidable housekeeping and management adjustments, but they are of a different order of concern.

Nor has any mention been made of the possibility of the settlement system being nationalized, along the lines of European experience, in the Post Office or the Federal Reserve System. While such a step is technically feasible, if not advantageously suited to a monopolistic operation, our preferences run strongly against extending government operations into service areas that can be satisfactorily performed privately.

If the views and speculations advanced here are at all persuasive, it will probably be with the assenter's proviso that "it won't happen in my time." To this skepticism I can only reply that most of the innovations I have alluded to are now in being, or about to be placed in operation. Individual banks in all sections of the country are adopting, piecemeal, elements of a system such as has been described. Before very long these experimental operations will provide a solid foundation for the new banking system of the future.

RECENT INNOVATIONS IN THE FUNCTIONS OF BANKS*

By A. James Meigs
First National City Bank, New York

The relative decline of commercial banks and the rapid growth of other financial intermediaries provoked debate in the 1950's over whether or not evolution and innovation in nonbank financial institutions would undermine the effectiveness of monetary policy. Since early 1961, however, total assets of commercial banks have grown nearly twice as fast as they did in the 1950's and banks have adopted new practices that might alter their responses to Federal Reserve actions. Consequently, a new debate is under way, focused this time on how evolution and innovation in banks will influence the effectiveness of monetary policy. It has been suggested that recent bank innovations have made the banking system less stable, have weakened Federal Reserve control over the money supply, and have loosened the links connecting money supply with income and prices.

This paper will describe new services, methods of attracting funds, and recent innovations in allocation of bank assets. Some of their implications will be discussed within the framework of the so-called "new view" of banks as intermediaries. Possible effects on Federal Reserve policy and on the growth prospects of commercial banks will be viewed in the light of recent research on the demand for money and other assets. Because the impact of automation has already been covered by Governor Mitchell, I shall concentrate on other innovations, recognizing of course that most of them have been influenced by the new technical possibilities offered by computers.

The Innovations

Banks undoubtedly have done more innovating in the last five years than in the preceding twenty-five. The recent innovations, moreover, are radically different from those imposed by legislation in the 1930's, when the banks were numb from the shock of the Great Contraction. Innovations this time are responses to expanding possibilities in the markets for financial services.

The upsurge of state and local government borrowing has expanded

* This paper has benefited from the help of many colleagues at First National City Bank, and especially Joseph S. Mascia. The judgments and opinions, and any errors, are my personal responsibility.

banks' activities not only as investors but also as underwriters. Bank trust departments have won management responsibility for most of the reserves of private noninsured pension plans, becoming in the process by far the largest institutional group in the stock market. They soon may be offering commingled investment accounts for individuals. As world trade and investment have grown, U.S. banks have greatly expanded their overseas services. The Eurodollar, one of the most remarkable financial innovations in history, is based upon transfer services provided by banks in this country.

Business demand for larger and longer-term financing has been met through new lending techniques and arrangement of participations among groups of banks and other lenders. Since 1963, banks have bought motor trailers, jet airliners, executive aircraft, computers, machine tools, and other equipment for leasing to customers. The number of banks establishing factoring departments or acquiring factoring firms has recently been increasing, possibly leading toward the automated accounting, analysis, and receivables financing described by Governor Mitchell.

Means for speeding the collection and transfer of cash have been offered in ever more ingenious forms. Several banks have recently entered the credit card business, also, foreshadowing the giro system Governor Mitchell has just described.

Probably the most remarkable change, from the viewpoint of bankers, is the sharp rise in the proportion of funds on which banks pay explicit interest, from about one-third of total liabilities in 1960 to about half in 1965. In addition to substantially increasing their share of all savings-type deposits, banks have developed new money market instruments, have expanded the federal funds market, and have engaged in long-term borrowing for the first time since the 1930's.

Negotiable certificates of deposit, or CD's, were introduced in February, 1961. More than $16 billion are now held by corporations, state and local governments, foreign central banks, international agencies, and individuals. Negotiable CD's did not spring fullblown to their present eminence, however. A secondary market had to be developed. Banks and customers had much to learn about the uses and limitations of CD's, and indeed are still learning.

The new bank promissory notes are much like CD's, from the viewpoint of the holders, but they are exempt from reserve requirements and deposit insurance assessments. Consequently, funds secured through promissory notes yielding 4.5 percent to the holders would have an effective interest cost to banks almost one-quarter of a percentage point less than the cost of funds raised through 4.5 percent

CD's. The promissory notes are also free from Regulation Q ceilings on rates. Just before the December, 1965, increase in Regulation Q ceilings, some bank promissory notes were being issued at rates above the maximum 4.5 percent permitted on CD's.

The federal funds market has continued to develop, with an increase in the number of banks participating and with more banks acting as dealers. Money market center banks also have increasingly tended to be net borrowers of federal funds. The federal funds rate has been above the discount rate by one-eighth of a percentage point for days at a time this year and occasionally has been one-quarter point higher. The seemingly irrational practice of paying a premium for reserves borrowed in the federal funds market reflects restriction of access to the discount window under Federal Reserve Regulation A, which prescribes that continuous borrowing and "profiting from rate differentials" are to be avoided. The actual discount rate to the banks, accordingly, is somewhat higher than the nominal rate posted by the Reserve banks.

Banks have raised long-term capital through issue of $1.5 billion in capital notes, convertible debentures, and preferred stock in the last three years. They have borrowed an additional $200 million or more through subsidiaries or affiliates holding title to their premises. The notes and convertible debentures are subordinate to depositors' claims and are subject to the statutory restrictions on banks' total borrowings.

In the last decade, and especially in the last four years, the average maturity of bank assets has increased substantially, as the shares of total assets devoted to mortgages, state and local government securities, and term loans have grown. Over the same period, the proportions of cash and short-term government securities have declined. Whether it is called evolution or innovation, this is a major change in banking practice. The lengthening maturity of bank assets, when contrasted with the generally short maturities of bank liabilities, has aroused concern among bankers and bank regulators, for it conflicts with the hallowed injunction against "borrowing short and lending long."

The rapid changes of the last few years have prompted banks to pay more attention to portfolio management techniques. George Morrison and Richard Selden concluded recently that many of the conventional rules of thumb in bank asset management are inadequate. "For the most part," they say, "the rules . . . do not assure an adequate liquidity position; nor, except through pure chance, could they possibly result in maximum profitability in either the short or long run" [27, p. 39]. Portfolio management, therefore, may well be one of the most fruitful areas for innovation in the future.

Banks as Intermediaries

The new stress on intermediary functions of banks is useful in assessing significance of recent developments. But, as I shall argue in a moment, recognizing that banks are intermediaries does not detract from their function as monetary policy links.

Intermediaries, according to Gurley and Shaw, "lend at one stratum of interest rates and borrow at a lower stratum," and thus are compensated for their services by the margin between yields on primary and indirect securities [16, p. 259]. The intermediary service, as James Tobin says, consists of satisfying portfolio preferences of two groups of people or firms simultaneously [30]. On one side are borrowers who want to acquire more real assets than they can command with their own net worth. On the other side are lenders who want to hold some of their wealth in assets of stable money value and negligible risk of default.

In the 1950's, demand for services of institutions specializing in real estate mortgage lending was increased by the housing boom. In addition, the attractiveness of shares in savings and loan associations was increased by improvement of insurance provisions in 1950 [12, pp. 659-72] [5, pp. 173-75]. In the 1960's, demands for the kinds of loan services commercial banks are especially adept in supplying and are permitted to supply have been relatively great [18]. The banks therefore have had strong incentive to attract funds to meet the demands. The expansion of banks' intermediary function was helped also by relaxation of regulatory constraints by the Federal Reserve and the Comptroller of the Currency. Regulation Q ceilings were raised in several steps, permitting banks to raise offering rates on time deposits and CD's. Removal or reduction of restrictions on some types of lending further aided banks to meet borrowers' demands. Surely, much of the variation in relative growth rates of banks and nonbank intermediaries since World War II can be explained by changes in demands for real assets and in regulatory constraints [8].

Innovations have helped banks to accommodate changing preferences of borrowers and lenders, and thus to produce a larger share of total intermediary services in the economy. I agree with Homer Jones's judgment that the innovations were not accidental; when profitable opportunity developed, innovation appeared [18]. It is probable, furthermore, that some of the innovations reduced the spread between borrowers' and lenders' rates, thus in effect improving the efficiency of the capital market [19, p. 62, n. 9].

These observations would probably be accepted without disagreement if they dealt with any financial intermediary other than banks.

To a generation of economists nurtured on multiple expansion arithmetic, however, perhaps the most intriguing aspect of recent innovations in banking is the implication that the banks themselves have influenced the size of the banking system.

Three of the main questions raised by bank innovations in the last few years are: (1) Have longer maturities of bank assets and the growing dependence upon time deposits made the banking system less stable? (2) Have bank innovations loosened the links between central bank actions and the money supply? (3) Have bank innovations loosened the links connecting money supply with income and prices?

It is, of course, impossible to settle such complex theoretical and empirical questions today, but some of the research published since 1960 provides preliminary answers and suggests where additional evidence might be found.

Innovations and Instability

Some lengthening of maturities of bank assets has been considered justified by growth in savings deposits, which bankers generally believe to be less volatile than demand deposits [27]. Time certificates of deposit, however, have placed bankers in something of a quandary. Although CD's are a convenient means for an individual bank to obtain funds at its own initiative, it is feared that the total volume of CD's in the system might suddenly contract if shifts in yield differentials make other instruments more attractive to investors. Possible consequences of such shifts go far beyond the embarrassment of banks, as George Mitchell points out. "Indeed," he says, "the most immediate and direct constraint on monetary policy posed by the new profile of bank liabilities may lie in the need to weigh carefully the impact of specific actions on such differentials" [24]. Even savings deposits, he suggests, might prove vulnerable to sudden withdrawals to fuel a spurt in consumer spending or to support investment in the stock market.

The immediate result of a shift out of bank time deposits would be an increase in required reserves, as time deposits were converted to demand deposits. If the Federal Reserve did not supply additional reserves, the banks would have to sell assets, which might be difficult to do under some circumstances. Phillip Cagan found, however, that large shifts between demand and time deposits from 1875 to 1955 did not appreciably affect reserves [5, pp. 164-79]. This of course does not necessarily mean that shifts between time and demand deposits will not cause trouble in the future, but it suggests that deposit shifts comparable in magnitude and timing to those of the past should not present insuperable problems for the Federal Reserve or the banks. Nevertheless, there are two complications now that did not exist during

most of the eighty years covered in Cagan's evidence: one is the negotiable CD and the other is the regulation of interest rates on bank deposits.

Ironically, regulation of deposit interest rates, which was intended to reduce instability in banking, now may do just the reverse and is at least a source of uncertainty. If banks are prevented by Regulation Q from adjusting their offering rates when market rates increase on competing instruments, such as commercial paper and Treasury bills, CD holders might shift into these other instruments on a large scale. Thus far, however, the Federal Reserve Board has raised Regulation Q ceilings each time rising market rates threatened to induce liquidation of CD's. The current ceiling of 5.5 percent on time deposits and certificates of deposit of more than thirty days' maturity should be high enough to relieve apprehensions about the volume of CD's but the 4 percent ceiling on savings deposits still is a potential tripwire.

Insurance companies and pension funds depend for liquidity in part by matching long-term assets with long-term liabilities [24]. When their term lending increases, banks might seek liquidity protection in similar fashion by extending maturities on their liabilities, through issue of time certificates with maturities of, say, three to five years or more. In 1965, however, the average maturity of newly issued CD's was being inexorably shortened by the rise of market rates in relation to the Regulation Q ceiling, until the ceiling was raised in December. Removal of deposit rate regulation, therefore, would reduce uncertainty about the volume of CD's and would facilitate adjustment of maturities on bank liabilities, as well.

Notes and debentures, which also might be used to lengthen maturities of bank liabilities, are now restricted in volume by regulatory limitations on borrowing. Effects of these borrowing limitations upon the performance of banks' intermediary function should be carefully studied.

Bank Innovations and Control of the Money Supply

James Tobin had support in recent econometric studies of bank responses to central bank action when he said:

> For the banking system as a whole the Federal Reserve's quantitative controls determine the supply of unborrowed reserves. But the extent to which this supply is left unused, or supplemented by borrowing at the discount window, depends on the economic circumstances confronting the banks—on available lending opportunities and on the whole structure of interest rates from the Fed's discount rate through the rates on mortgages and long-term securities [30, p. 417].

Although Tobin says that the reserve-deposit ratio is not a constant, he does not say the Federal Reserve is unable to influence the money supply through controlling the stock of reserves. He does raise a valid

empirical question: Are there stable relationships among reserves, total deposits or money supply, and other variables that will afford the central bank effective control? The studies of Clark Warburton, Milton Friedman, Anna Schwartz, and Phillip Cagan provide abundant evidence that stable relationships do indeed exist and that the Federal Reserve can control the money supply [5] [12] [31] [32]. Their findings, furthermore, are strongly supported by the theoretical and empirical work of others and by experience in other countries, as well [1] [2] [3] [4] [7] [14, Chap. VII] [20] [21] [22] [25].

The key to the Federal Reserve's power is that banks must hold some reserves in the form of vault cash and balances at the Federal Reserve banks to provide for expected and unexpected clearings drains. Federal Reserve open market operations in turn control the stock of unborrowed reserves. Two built-in sources of slippage in the system as it is now constituted are the ability of banks to vary their excess reserves and borrowings, mentioned by Tobin, and a tendency for aggregate reserve requirements to change because of differences in the reserve requirements applied to various classes of deposits. One approach to these sources of variation in deposits might be to reduce their influence through institutional changes, by eliminating member-bank borrowing or instituting uniform reserve requirements, for example. Another is to learn to cope with them by becoming more familiar with their behavior, so that they can be predicted and offset.

Have the recent innovations in banking so changed the responses of banks that the Federal Reserve's degree of control has been reduced? Tilford Gaines, Lyle Gramley, and Samuel Chase argue that this is the case; they believe growth of interest-sensitive time deposits, CD's, and other bank liabilities has much reduced the Federal Reserve's control of the money supply, if it ever existed [13] [15]. Their criticisms of conventional money supply theory actually do not depend crucially upon recent institutional changes and therefore should be confronted with the evidence from other time periods and other countries mentioned earlier. The empirical evidence of Gramley and Chase indicates that elasticities of substitution between demand and time deposits and between bank deposits and securities changed over the 1948-64 period covered, perhaps partly because of some of the bank innovations discussed here. However, such changes in the substitution relations can be explained within conventional money supply theory.

Banking innovations could influence the demand for excess reserves and borrowings, and hence the ratio of unborrowed reserves to deposits, by affecting banks' expectations regarding their ability to meet clearings drains. Improvements in the federal funds market, increased opportunities for individual banks to borrow at their own ini-

tiative through CD's or notes, and an increase in the proportion of savings deposits should reduce both the quantity of excess reserves demanded under any given set of market conditions and the volume of borrowing from the Federal Reserve. An increase in the proportion of volatile CD's, on the other hand, might have the opposite effect. Although each such innovation may shift banks' demand schedules for reserves and borrowings, there is no a priori reason for expecting a shift to make the schedules less stable.

As long as the demand schedules remain stable, the shifts produced by bank innovations and changes in public preferences merely change the volume of open market operations required to produce a given change in deposits. How much the demand functions have changed in recent years or whether they have become unstable must be determined empirically. Nevertheless, precise estimates of these parameters are not essential for effectiveness in controlling the money supply, because the monetary authorities can cut and try until they produce the desired effects on money stock. The level of money supply was predicted for biweekly periods in 1961 with an average error of less than 0.2 percent by Robert Black, from the data then available for conducting open market operations [2].

Because the new promissory notes are not subject to reserve requirements, they might be expected to escape Federal Reserve control. I would argue, to the contrary, that Federal Reserve control of the stock of bank reserves provides effective control not just over deposits but over total bank liabilities, whether or not reserve requirements apply to all of them. Preferences of the banks and the public influence the relative shares of demand deposits, time deposits, and notes in total bank liabilities, but the total is limited by the stock of reserves. Banks will want to hold some reserves against the promissory notes and thus will in effect observe a voluntary reserve requirement. Incidentally, Deane Carson and others have argued that reserve requirements are not even necessary for control of deposits [6].

Bank Innovations and the Effectiveness of Money Supply Changes

Development of a new liquid asset, such as the CD or some new instrument offered by another intermediary, may shift the public's demand schedule for money, whether money is defined to include or to exclude the new asset. Changes in characteristics of deposits or other assets by modification of regulatory constraints may also shift the demand schedule for money. Consequently, such innovations and other changes impair the predictive value of parameter estimates made under the conditions that existed before the changes occurred. Nevertheless, this does not offset the value of using as a policy variable

some subset of assets whose supply is influenced by the monetary authorities. Or, to put the matter another way, this certainly does not mean changes in the stock of these assets can safely be ignored.

Gurley and Shaw argued that restriction of money supply growth to curb inflation might induce expansion and innovation by nonbank intermediaries, leading to an increase in growth of money substitutes [17, pp. 228-46]. If money is defined narrowly as demand deposits and currency, the same argument might be applied to bank innovations that increase the supply of time deposits or bank notes. A tentative description by Milton Friedman and David Meiselman of the channels through which money supply changes operate implies a quite different hypothesis [11, pp. 217-22]. If the supply of narrowly defined money grows more slowly than the demand for it, the public may try to convert other liquid assets into money in order to maintain cash balances at their desired level. This would have the effect of reducing the volumes of bank time deposits and deposits in nonbank intermediaries (for any given set of deposit interest rates) rather than increasing them. The converse would be true if the supply of money grows faster than the quantity demanded; growth of time deposits and deposits in other intermediaries would increase. Studies of changes in intermediary deposits by David Fand, Warren Smith, Robert Parks, and Edgar Feige appear more consistent with the Friedman-Meiselman view than with the Gurley-Shaw view, but the question is not settled [9] [10] [28] [29]. We badly need to learn more about the public's preferences for demand and time deposits and other assets.

This raises the question of how fast the total liabilities (and assets) of the banking system can be permitted to grow without risking price inflation or deflation. Here again, we need to know more about the substitution relationships. As George Morrison has argued, "shifts from demand to time deposits or from time deposits to savings and loan shares would be inflationary if such shifts cause the public to have excess amounts of liquid assets, whether they be in the form of time deposits, demand deposits, or savings and loan shares" [26]. Feige's results indicate that demand deposits and time deposits are weak substitutes. If this is so, a higher rate of bank growth could be permitted by the Federal Reserve when the ratio of new time deposits to new demand deposits is large than when it is small.

Or, more generally, the rate of bank growth that is consistent with price stability will depend on the degree of "moneyness" of bank liabilities. Recognizing both the intermediary and monetary functions of banks suggests the following hypothesis for resolving the apparent paradox that banks can influence the rate of growth of their total assets while under effective control of the monetary authorities. Faced

with monetary policy constraints on the growth of demand deposits, the banks in recent years have been able to increase growth in their assets by finding buyers for certificate deposits and long-term debentures, and also in effect by inducing the public to substitute bank liabilities for deposits or shares of other intermediaries. The unusually high rates of bank credit expansion of the last four years have resulted in part from substitution of one intermediary channel—commercial banks—for another—the mutual savings institutions—as Homer Jones has suggested [18]. The effects on price levels that might be expected to flow from such a rate of credit expansion may have been moderated by the lower degree of "moneyness" of the new mix of bank liabilities and by retardation in growth of the near-money deposits of the other intermediaries.

In conclusion, it is by no means evident that the recent innovations in bank functions impair the effectiveness of monetary policy, although they probably have changed some control characteristics of the system. The great volume of monetary research now underway should develop much greater understanding of the linkages in the system. As the relationships become clearer, we may find that the effects of innovations on the monetary system are something like those of innovations in aircraft design. When a new airplane is rolled out for its first flight, the pilot knows it will respond a little differently from any other plane he has flown. But it is still an airplane, and he can fly it.

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INNOVATIONS IN INTEREST RATE POLICY*

By FRANCO MODIGLIANI *and* RICHARD SUTCH
Massachusetts Institute of Technology

This paper is an examination of the success, or we should say lack of success, of the policy launched at the beginning of 1961 by the incoming Kennedy Administration, which has become known as "Operation Twist." This was an attempt to twist the maturity structure of interest rates by raising yields on securities with short term to maturity while simultaneously lowering, or at least holding the line on, long-term rates. Higher short-term rates were expected to contribute significantly toward stemming the outflow of capital and thus helping the United States balance-of-payments problem, while low long-term rates were considered desirable to stimulate the economy by increasing the flow of private investment. We are not concerned, however, with the broad issue of whether Operation Twist contributed to improving the balance of payments while sustaining domestic activity. Our focus is, rather, on Operation Twist per se. We direct ourselves to a review of the techniques used by the government and Federal Reserve to affect the term structure and attempt to assess how far they succeeded in achieving the stated goal of twisting the yield curve.

As far as we can see there were two main actions aimed directly at such a twisting:

1. Federal Reserve open market operations and Treasury debt management operations directed toward shortening the average term to maturity of the outstanding government debt held by the public. An increase in the relative supply of short-term securities was expected to exert upward pressure on short-term rates, while the corresponding decrease in the availability of long-term securities should have tended to lower long-term yields, thus twisting the term structure in the desired direction.

2. Beginning in January, 1962, the successive increases in the struc-

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ture of ceiling rates payable on commercial banks' time and saving deposits under Regulation Q. According to the *Economic Report of the President* of January, 1962, this "action was taken to promote competition for saving and to encourage retention of foreign funds by member banks and thus moderate pressures on this country's balance of payments" [3, p. 88, Table 8]. Also under this heading one should include the recent acquiescence by the Federal Reserve Board to the issuance of unsecured notes and debentures by commercial banks.[1]

An examination of the behavior of key short- and long-term rates between early 1961 and the third quarter of 1965, summarized in Table 1, reveals that short-term rates have risen substantially while long-term rates moved relatively little, some moderately up (government bonds, corporate Aaa) others moderately down (municipals, coporate Baa's, mortgage rates). As a result, the spread between rates on long-term government bonds and the bills rate has declined from 150 base points down to 35 base points, while the difference between Aaa corporate bonds and the commercial paper rate shrank from 125 to 12 base points. These figures would seem to provide impressive evidence that Operation Twist was a remarkable success. To make such an interpretation (as has been frequently done) would be much too hasty, for as historical experience has shown, the spread typically tends to close in a period of recovery and rising short-term rates, such as prevailed between 1961 and the present. Indeed, currently prevailing spreads are still appreciably larger than they were at the peak of the previous cycle in 1959 and early 1960, as can be seen from the last row of Table 1, Part B. Thus the closing of the spreads between the turn of 1960 and the present might reflect merely the normal tendency for spreads to close as short-term rates advance. This hunch can be tested by estimating the historical relationship between the spread and short rates with ordinary regression techniques, and then comparing the actual relation between short and long rates with that predicted by the least square regression. Using government securities we estimated the relation between the spread (S) and the Treasury three month bills rate (r) with quarterly data for the period 1952-I to 1961-IV, obtaining

[1] Other tools were brought to bear on the problem but were not designed to twist yield curves; rather, they were policies that were intended to change the reaction of the economy to a given yield curve. These can be broadly summarized under two headings: (1) Measures directly aimed at reducing capital exports for a given structure of long- and short-term rates: these measures include primarily (*a*) the interest equalization tax and (*b*) the Johnson Administration program of voluntary restraint in bank lending to foreigners and to domestic firms for foreign operations, and in direct foreign investments. (2) Fiscal measures aimed at increasing the rate of domestic long-term investments for a given level of long-term rates: these measures include (*a*) the Internal Revenue Department's revised depreciation guide lines and (*b*) the investment credit provisions. An assessment of these policies is beyond the province of this study, which will concentrate only on those techniques designed to change the shape of the yield curve.

$$R_t - r_t \equiv S_t = \underset{(0.17)}{2.16} - \underset{(0.070)}{0.495 r_t} = .495(4.37 - r_t)$$

$$S_e = .39$$

Since in the third quarter of 1965 the bill rate was 3.85 percent, this equation would predict a spread between the long rate and the bill rate of 33 points, almost identical with the spread actually prevailing of 35 base points. Very similar negative conclusions about the effectiveness of Operation Twist can be reached by extrapolating the relation between the commercial paper rate and the spread between this rate and Moody's Aaa bond yields, whether the relation is estimated for the postwar period alone or going back to the beginning of the 1920's.

Are we, then, to conclude that Operation Twist was a total failure, at least with respect to the structure of yields on marketable securities—that the changes which occurred since the inception of that operation are not noticeably different from what might have been expected in its absence? Clearly, to draw such a conclusion from the rudimentary evidence presented above would be no more warranted than to infer from the figures of Table 1 that the policy was a howling success. The point of these simple tests is rather to emphasize that the task of assessing the success of the operation is far from trivial and can only be adequately tackled with the help of a theoretically grounded and empirically tested understanding of the basic forces which tend to shape the yield structure and its variations in time. It is, then, to this challenging task that we must turn first.

I. *Recent Theoretical Developments in the Analysis of the Maturity Structure*

There is by now general agreement that in an ideal world of no transaction costs or taxes, rational behavior and certainty (about future rates), the maturity structure of yields must be controlled by the simple principle that all outstanding instruments, regardless of maturity, must produce identical returns over any given interval of time—where the return is defined as the sum of cash payments plus any increase (or minus any decrease) in the market value of the instrument. This principle in turn implies that at any date t the spread between the yield of an n period bond and the short rate, $S(n, t) = R(n, t) - R(1, t)$, is equal to minus the capital gain from holding the n period instrument. The capital gain in turn is inversely related to the change in yield: $\Delta R(n, t) = R(n-1, t+1) - R(n, t)$. Consequently, $R(n, t)$ can be expressed in terms of the current short rate $R(1, t)$ and the future long rate $R(n-1, t+1)$. Moreover, since $R(n-1, t+1)$ can in turn be expressed in terms of $R(1, t+1)$ and $R(n-2, t+2)$, and so on, recursively, it is readily apparent that $R(n, t)$ can also be expressed in terms of the current and

TABLE 1

BEHAVIOR OF SOME KEY SHORT- AND LONG-TERM RATES FROM 1960–61 TO THE THIRD QUARTER OF 1965

Year and Quarter	Bills Rate	Commercial Paper Rate	Average Rate on Time Deposits	Average Yield on S&L Shares	Long-term Government Bonds	Corporate Bonds (Moody's) Aaa	Corporate Bonds (Moody's) Baa	High Grade Municipals	Conventional Mortgage Yields
A. LEVELS (%)									
	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	(9)
1960–3	2.36	3.37	2.57	3.86*	3.82	4.31	5.10	3.60	6.25
1961–1	2.35	3.01	2.65	3.90*	3.83	4.27	5.06	3.34	6.05
1965–3	3.85	4.38	4.15†	4.19‡	4.20	4.50	4.89	3.27	5.85§
B. CHANGES									
1960–3 to 1965–3	1.49	1.01	1.58†	0.35*,‡	.37	.19	−.21	−.33	−.40§
1961–1 to 1965–3	1.50	1.37	1.50†	0.29*,‡	.36	.23	−.17	−.26	−.20§
1960–1 to 1965–3	−.09	−.31	1.64†	0.35*,‡	−.02	−.05	−.42	−.72	−.45§

* Average for the year.
† Second quarter of 1965.
‡ Average for 1964.
§ Last quarter of 1964.
SOURCES: Columns 1, 2, 5, 6, 7, 8, *Economic Indicators*.
Column 4, Savings and Loan League.
Column 3, Frank de Leeuw and the Federal Reserve Bank of St. Louis.
Column 9, Federal Housing Administration.

future rates for one period loans prevailing in each of the n periods to maturity, $R(1, t)$, $R(1, t+1)$, $\cdots$ $R(1, t+n-1)$, although the precise form of the functional relation will depend on the shape of the stream of cash payments promised by the bond until maturity. Finally, because the return to the lender and the cost to the borrower over any interval will be the same regardless of the maturity of the instrument held or issued, neither would have a special incentive to match the maturity structure of his assets or liabilities to the length of time for which he intends to remain a creditor or debtor.

There is unfortunately much less agreement as to the determinants of the yield structure in the "real" world. The prevailing points of view may be summarized as follows.

1. At one end of the spectrum is the Pure Expectation Hypothesis, which holds that the certainty model provides an adequate approximation to the real world, except that the equality of returns of the certainty world must now be replaced by the equality of "expected" returns, where the expected returns may be thought of as the mean value (or some other analogous measure of central tendency) of the subjective probability distribution of possible returns. In particular, for every n, $R(n, t)$ must equal $R(1, t)$ minus the expected capital gain, determined by the expected change in the n period rate, say $\Delta R^e(n, t)$. For otherwise holders of bonds with lower expected returns would try to sell them, bidding down their price and raising their yield, and to acquire higher yielding instruments, bidding up their price and reducing their yields, until the postulated relation would come to hold.

2. A variant of the expectation hypothesis, of Keynesian inspiration [10] but articulated largely by Hicks [8], which has wide support at the present, may be labeled the Risk Premium Model. It basically accepts the view that yields on various maturities are related to each other by the expectations of future long rates, and hence also short rates, but it calls attention to differences in the degree of uncertainty which attaches to the expected return to be obtained, in the short run, from holding securities of different length. While the return on short-term securities is certain (since the value of the principal is guaranteed by repayment at the end of the period), the return on longer maturities is not guaranteed because of the uncertainty of future rates and hence of the end of period market value of the bond. Furthermore, the uncertainty tends to be greater the longer the maturity, since a given change in the long rate tends to produce a greater variation in terminal value the longer the remaining life to maturity. If, then, investors are prevailingly risk averters, as a good deal of other evidence suggests, one should expect that if the expected return were the same on all maturities, they would tend to prefer the safer short-term instruments. Hence, in order to induce the market to hold the longer-term maturities supplied by long-

term borrowers, the expected return on these maturities must exceed that on shorter-term instruments by an expected risk or liquidity premium. According to this view, the yield curve will tend to rise more than the curve implied by the pure expectation hypothesis because of the increasing risk premium as the term to maturity increases. The size of these risk premiums might be expected to depend on the relative supplies of longer maturities and the strength of investors' risk aversion.

3. Finally, there is the view that might be labeled the Market Segmentation Hypothesis. The proponents of this approach suggest that both lenders and borrowers have definite preferences for instruments of a specific maturity, and for various reasons, partly due to institutional factors and regulations constraining financial intermediaries, will tend to stick to securities of the corresponding maturity, without paying attention to rates of return on other maturities.[2] Hence the rates for different terms to maturity tend to be determined, each in its separate market, by their independent supply and demand schedules. The rates so set might well imply wide differences in the expected return obtainable in the current period, or over some sequence of periods, by investing in different maturities, but such differences, it is argued, would not induce traders to move out of their preferred maturity—or maturity habitat, as we shall call it—except possibly when the discrepancies become extreme and glaring.

In our view, each of the three models has its merits, but also suffers from shortcomings. We propose, therefore, an alternative model which, in essence, blends the previous three, and which we label the Preferred Habitat Theory. This model shares with the Hicksian approach the notion that the yield structure is basically controlled by the principle of the equality of expected returns, but modified by the risk premiums. Yet it differs from it in one fundamental respect. The Hicksian model assumes that all traders are concerned with the short period return and that, therefore, anybody going long is bearing the risk associated with the uncertainty of the short period return from longer-term instruments. But this view would be correct only if we could assume that every lender desires to turn his portfolio back into cash at the end of the short period; i.e., that he has a short habitat (cf. Meiselman [14]). In reality, however, different transactors are likely to have different habitats, as the segmentation theory points out. Suppose that a person has an n period habitat; that is, he has funds which he will not need for n periods and which, therefore, he intends to keep invested in bonds for n periods. If he invests in n period bonds, he will know exactly the outcome of his investments as measured by the terminal value of his wealth (this being only approximately true if he were to invest in a conventional loan and precisely true for a pure n period loan; that is, a loan that was

[2] This view has been stressed by a number of authors, in particular Culbertson [3].

issued on a discount basis). If, however, he stays short, his outcome is uncertain, as it will depend on the future course of the short rates in periods 2, 3, . . . , n-1. Furthermore, he is likely to have to incur greater transaction costs. Thus, if he has risk aversion, he will prefer to stay long unless the average of the expected short rates exceeds the long rate by an amount sufficient to cover extra transaction costs and to compensate him for the extra risk of going short. Similarly, if he should invest in maturities longer than n, he would also be exposing himself to risk, this time to the Hicks-Keynesian uncertainty as to the price he can fetch for his not-yet-matured bonds. Thus, risk aversion should not lead investors to prefer to stay short but, instead, should lead them to hedge by staying in their maturity habitat, unless other maturities (longer or shorter) offer an expected premium sufficient to compensate for the risk and cost of moving out of one's habitat. Similar considerations will clearly apply, *mutatis mutandis*, to the borrower's side of the market.

Under this model the rate for a given maturity, n, could differ from the rate implied by the Pure Expectation Hypothesis by positive or negative "risk premiums," reflecting the extent to which the supply of funds with habitat n differs from the aggregate demand for n period loans forthcoming at that rate. If the n period demand exceeded the funds with n period habitat, there would tend to arise a premium in the n period maturity, and conversely.[3] Such premiums or discounts would tend to bring about shifts in funds between different maturity markets, both through the "speculation" of investors tempted out of their natural habitat by the lure of higher expected returns and through "arbitrage" by intermediaries induced to "take a position" by borrowing in the maturity range where the expected return is low, and lending where the expected return is high.

In summary, then, the Habitat Model implies that the spread $S(n, t)$ between the long rate $R(n, t)$ and the short rate $R(1, t)$ should depend primarily on the expected change in the long rate, $\Delta R^e(n, t)$. But it suggests that the spread could also be influenced by the supply of long- and short-term securities by primary borrowers (i.e., by borrowers other than arbitrageurs) relative to the corresponding demand of primary lenders, to an extent reflecting prevailing risk aversion, transaction costs, and facilities for effective arbitrage operations.

These conclusions can be conveniently summarized in the following equations.

$$\begin{aligned}&\text{Expected current return on an } n \text{ period bond}\\ &\quad\equiv R(n, t) + \text{Expected capital gain}\\ &\quad= R(1, t) + F_t\end{aligned}$$

[3] This is only approximately true, for under risk aversion, funds of habitat n would not be indifferent as to where they would move but would tend to spill, preferably into neighboring maturities where the risk would tend to be smaller.

where F_t stands for the net effect of relative supply factors and could in principle be positive or negative. Solving for $R(n, t)$, and taking the Expected Capital Gain as proportional to the expected fall in the long rate, i.e., to $-\Delta R^e(n, t)$, we can also write

$$(1) \qquad R(n, t) = R(1, t) - \text{Expected capital gain} + F_t$$
$$= R(1, t) + \beta \Delta R^e(n, t) + F_t$$[4]

II. *An Operational Formulation of the Habitat Model*

Before we can test our hypothesis we must recast equation (1) into an operational form suitable for empirical estimation. This entails specifying both a theory of how expectations are formed and a functional form for the summary term "F." For a model of expectations we draw on the highly imaginative approach of Frank de Leeuw [4] who synthesized two currently held views as to the determinants of the expected change in long-term rates.[5]

One widely held hypothesis associated with Keynes [11] holds that the market expects the interest rate to regress toward a "normal" level based on past experience. Modifying slightly De Leeuw's formulation, we approximate this normal level, denoted by $\overline{R}_t$, by some average of the long rates for the past m periods and a constant which could be thought of as a very long-run normal level. Thus:

$$\overline{R}_t = v \sum_{i=1}^{m} \mu_i R_{t-i} + (1 - v)c \qquad 0 < v < 1$$

where R_t is used hereafter as a symbol for the long-term rate and the μ_i's are weights adding up to one. Since the recent experience should be more salient we should expect the μ_i's to decline toward zero as i rises from one to m. This regressive hypothesis can thus be formalized as

$$(2) \qquad \Delta R_t^e = \alpha_1(\overline{R}_t - R_t) = \alpha_1 \left[v \sum_{i=1}^{m} \mu_i R_{t-i} + (1 - v)c - R_t \right]$$[6]

where α_1 is a measure of the speed with which R_t is expected to return to $\overline{R}$.

[4] The substitution of $-\beta \Delta R^e(n, t)$ for the expected capital gain should be recognized as an approximation, if β is taken as constant. Strictly speaking, β can be shown to be a function both of the length to maturity n and of $R(n, t)$ (as well as possibly future short rates). However, the dependence on n need not be neglected if we deal with a fixed maturity n; and the effect of $R(n, t)$ can be shown to be sufficiently small to be neglected to a first approximation within the range of variation of $R(n, t)$ prevailing in the period with which we are concerned.

[5] Meiselman [14] and Kessel [9] have also made important contributions in this area, but while their work provides impressive support for the expectations model, their approach is not directly applicable to our problem.

[6] This hypothesis could also be derived by replacing the notion of a normal level with the notion of a normal range (cf., Malkiel [13]).

A quite different hypothesis, advanced by James Duesenberry, suggests that expectations might be extrapolative: "a rise in rates [leading] to an expectation of a further rise and vice versa" ([5], p. 318). De Leeuw suggests that the recent trend in rates might be approximated by the difference between the current rate and some weighted average of recent past rates and accordingly expresses the extrapolative hypothesis as

$$\Delta R_t^* = \alpha_2 \left(R_t - \sum_{i=1}^{n} \delta_i R_{t-i} \right); \qquad \alpha_2 > 0 \tag{3}$$

where n should be appreciably smaller than m and the weights, δ_i, would probably decline rather rapidly.

Now, as De Leeuw rightly points out, it is quite credible that both hypotheses contain an important element of truth—that expectations contain both extrapolative and regressive elements. If so, we can combine the right-hand side of (2) and (3) to obtain

$$\Delta R_t^* = -aR_t + \sum_{i=1}^{m} b_i R_{t-i} + dc \tag{4}$$

where $a = (\alpha_1 - \alpha_2)$, $b_i = \alpha_1 v \mu_i - \alpha_2 \delta_i$, with δ_i defined to be zero for $i > n$, and $d = \alpha_1(1\text{-}v)$. Since the term in the summation now represents the difference of two lag structures, we can no longer expect it to be of a simple geometric form. Indeed, if the extrapolative element is at all significant (i.e., α_2 is not zero or small compared with $\alpha_1 v$) we should find that initially, since δ_i falls faster than μ_i, b_i rises (possibly even from negative values), reaching a peak in the neighborhood of n and then declines back toward zero.

We are ready now to substitute equation (4) into the basic hypothesis (1) which yields

$$R_t = r_t - \beta a R_t + \sum_{i=1}^{m} \beta b_i R_{t-i} + \beta dc + F_t$$

where r_t is used hereafter to denote the short rate $R(1, t)$. As it now stands, this equation involves the current long rate on both sides; but this can be readily handled by solving the equation for R_t, obtaining finally

$$R_t = Ar_t + \sum_{i=1}^{m} B_i R_{t-i} + C + F_t' + \epsilon_t \tag{5}$$

where

$$A = \frac{1}{1+\beta a}, \quad B_i = \frac{\beta}{1+\beta a} b_i, \quad C = \frac{\beta dc}{1+\beta a}$$ and ϵ_t is the error term.

We note that, since β and a are supposed to be positive, the coefficient A should be positive but distinctly below unity, and that, since the lag coefficients, B_i, are proportional to the b_i of (4), our earlier inferences about the b_i's—which define the lag structure—applies equally to the B_i's.

III. *Estimation of the Model*

If we disregard for the moment the nondescript supply term F', equation (5) contains only observables and is in principle ready for estimation and testing. In so doing, however, we must face two rather difficult problems. Since the distributed lag on the previous long rates should be quite long and not of the familiar exponentially declining type, it poses estimation problems. These De Leeuw solved with an ingenious technique that involved estimation of only a small number of coefficients rather than a separate coefficient for each lagged value of R (an alternative which would undoubtedly lead to severe multicolinearity problems). However, since his writing, an alternative, more powerful and far more flexible technique for estimating lag structures has been developed by Shirley Almon [1] and pursued by Charles Bischoff in work currently in progress at the Massachusetts Institute of Technology. This procedure imposes very little a priori restriction on the lag structure, requiring merely that it can be approximated by a polynomial. Since our formulation suggests that the lag distribution should rise to a single peak and then fall, we concluded that a fourth degree polynomial would be sufficiently flexible to closely reproduce the true structure.

The Almon Interpolation Distribution involves the calculation of Lagrangian interpolation polynomials, which are used to weight a specified number of past values of the variable whose lag is to be estimated. These weighted averages, or Almon variables, are then entered in the ordinary least squares regression equation. For a fourth degree polynomial, five Lagrangian polynomials would be needed to define the structure. However, since we have a priori reasons to believe that the lag structure will taper off to negligible values at some finite distance in the past, we further impose the restriction that the polynomial to be estimated should assume a zero value at a finite lag. This allows us to use only four Lagrangian polynomials and hence also four Almon variables.[7] The four coefficients estimated for these variables in the regression plus

[7] This is a modification of the procedure which Mrs. Almon followed in her paper on lags between capital appropriations and expenditures [1]. She specified that the lag distribution began as well as ended with a zero value, and thus only three Almon variables were necessary to estimate the fourth degree polynomial. For our purposes the requirement that the polynomial pass through zero at t plus one seemed to place an unwarranted restriction on the shape of the distributed lag. Experiments with several alternative restrictions indicated that a free estimation of the head of the distribution yielded more sensible lag structures and closer fits.

the a priori specification of the intercept yield the five points necessary to define a fourth degree polynomial.

However, before we attempt to apply this technique, we must face another difficulty. Equation (5) purports to explain the dependent variable R_t in terms of lagged values of itself. It is well known that in the presence of serial correlation of the error term, ϵ_t, such a procedure will lead to biased estimates of the coefficients [7].

The problem is particularly serious for our present purposes, as it can be shown that if, in fact, Operation Twist was successful, then an equation of the form (5) estimated by ordinary least squares would very likely tend to conceal and understate the true effectiveness.

One way to handle this difficulty would be to estimate (5) using recently developed techniques for consistent estimation of equations which include a lagged dependent variable.[8] However, we propose an alternative approach. As is well known, an equation in the form of (5) implies that R_t can also be expressed as a function only of r_t and a weighted sum of all previous short rates, r_{t-i}. This result can derived by using equation (5) to express R_{t-1} in terms of r_{t-1}, and R_{t-2} to R_{t-m-1}, and so on, recursively. The final result involves only r_{t-j}, with j extending indefinitely into the past, but with the coefficients of the far removed r_{t-j} approaching zero. Hence, to a first approximation R_t can be expressed as an average of a finite and reasonably small number of lagged values of r:

$$R_t = \alpha + \beta_0 r_t + \sum_{i=1}^{m} \beta_i r_{t-i} + \eta_t \quad (6)$$

[9]

This equation is very similar to (5), from which it differs only because the distributed lag on the long rate is replaced by a distributed lag on the short rate. This substitution is in essence equivalent to hypothesizing that the expected long rate R_t^e can be approximated as a weighted average of past short rates rather than past long rates. This is certainly as sensible a hypothesis as De Leeuw's original. Indeed, it is basically an implication of that hypothesis, and conversely. Whether it is more convenient and efficient to approximate the basic model by a long lag on the long rate or on the short rate is, in the last analysis, a purely pragmatic and empirical issue.[10] But even if (6) should fit the data less well than

[8] Such a technique would be similar to that suggested by Liviatan [12].

[9] Ideally, one might wish to estimate an infinite lag on r_t. Estimation techniques for the Pascal-Solow lag distribution [15] now being developed by Robert Hall look to be suitable for such a model, but this will have to wait for a later date. It is very unlikely, however, that refined estimation techniques could substantially alter our conclusions. Incidentally, equation (6) can be recognized as simply the first stage of the Liviatan technique as applied to this model (see footnote 8 above).

[10] The only statistically significant difference between (5) and (6) lies in the stochastic properties of the error term hypothesized for the two models. If (5) holds with nonserially correlated error, then (6) will have an error vector which is autoregressive, and conversely.

(5), it has two significant advantages: (1) because it does not involve the lagged dependent variable, an unbiased estimate of its coefficients can be obtained by ordinary regression techniques; and (2) it provides a more reliable tool for testing Operation Twist, free of the bias noted above.

The basic hypothesis (6) was estimated using the Almon technique described above for the forty quarters spanning the pre-Operation Twist period, 1952-I to 1961-IV, with R_t defined as the yield on long-term government bonds (i.e., due or callable in ten years or later) and r_t defined as the three-months Treasury bills rate. Since we are particularly interested in the behavior of the spread, we have found it convenient to subtract r_t from both sides of the equation. This transformation converts the dependent variable into the spread $S_t = R_t - r_t$ without affecting the right-hand side of the equation or its statistical properties except for changing the coefficient of r_t on the right-hand side from β_o to $-(1\text{-}\beta_o)$.

Lags of between two and seven years were tested with the most satisfactory results obtained for lags of around four years. The 16-quarter lag produced lower standard errors, smaller serial correlation, and the most sensible lag structure, although the multiple correlation and DW statistics[11] were not very sensitive to the length of lag, at least beyond four years. The result can be summarized as follows, omitting for the moment variables besides the short rate:

$$S_t = \underset{(0.028)}{1.239} - \underset{(0.030)}{0.684}\, r_t + \sum_{i=1}^{16} \beta_i r_{t-i} \tag{7}$$

$$R^2 = .975 \qquad S_e = .093 \qquad DW = 1.42$$

The expression

$$\sum_{i=1}^{16} \beta_i R_{t-i}$$

represents the finite lag. The 16 coefficients of r_{t-i} (the β_i's) are plotted within a band of plus and minus one standard error, in Figure 1.[12]

These results are rather striking. The coefficient of r_t has the predicted sign and order of magnitude, the lag structure has the predicted shape, and its initial rising segment provides impressive support for the hypothesis that expectations involve significant extrapolative as well as the

[11] The symbol DW denotes the Durbin-Watson statistic, a measure of the estimated first order serial correlation of the residual error [6].

[12] The actual least squares regression entered four Almon variables, each of which received highly significant coefficients. These coefficients were unscrambled to obtain the lag structure and its standard error plotted in Figure 1. Because there is no unique way of selecting the interpolation polynomials to be used in the estimation of the lag distribution, the presentation of these four coefficients was thought not to be as helpful to the reader as the summary statistics presented.
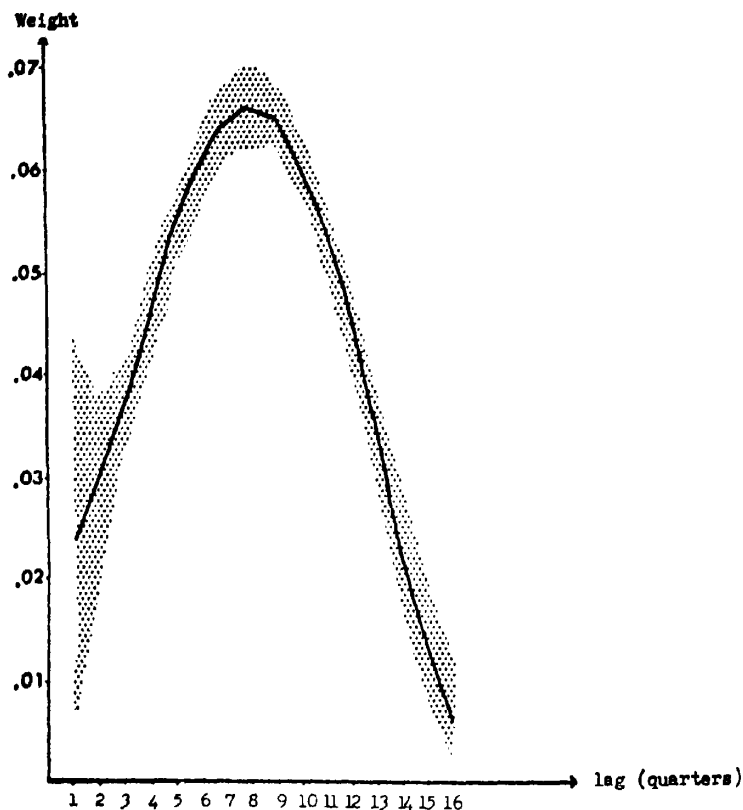
FIGURE 1

LAG STRUCTURE ON THE SHORT RATE PLUS AND MINUS ONE STANDARD ERROR (EQUATION 7)*

* The coefficients and their standard errors plotted are, from left to right: .0229(.0215), .0293(.0091), .0373(.0054), .0458(.0060), .0536(.0058), .0599(.0048), .0641(.0044), .0656(.0049), .0644(.0055), .0603(.0056), .0537(.0053), .0449(.0051), .0347(.0058), .0239(.0070), .0136(.0074), .0514(.0056).

widely recognized regressive elements. The multiple correlation is quite high and the standard error remarkably low, less than 10 base points. (This, incidentally, is a vast improvement over De Leeuw's original model, which for the same period, even with additional significant variables, has a standard error of 34 base points and a *DW* of .79.)

There remains to examine whether the small residual error might reflect in some measure supply effects, subsumed under F'_t in equation (5); or more precisely variation in supply conditions for, to some extent, the rather large constant term of (7) may already reflect a risk premium

resulting from supply effects. Unfortunately, the measurement of supply-demand effects poses formidable problems, even if we are prepared to limit ourselves to variations in supply, on the assumption that the demand side is not subject to significant variations. The problem arises not only from shortage of data but even more from the statistical and conceptual difficulties of separating the total supply from the relevant primary supply. For this reason, most authors have ended up by measuring supply effects by the composition of the outstanding supply of marketable federal debt outside the Federal Reserve and Government Trust Accounts. De Leeuw in particular tried to test the effect of both the composition and the change in the composition of the debt outstanding in each of four maturity classes. He could find no evidence that the proportion outstanding in the various classes had any effect, but found some evidence that an increase in the proportion of both short (less than one year) and intermediate debt (one to five years) tended to reduce the spread.

We have repeated the tests of De Leeuw and others, and we find that none of the many debt variables we have tested obtains a significant coefficient with the predicted sign.[13] This is somewhat surprising in light of our a priori expectations, but does confirm the findings of most other authors. We must conclude that neither the maturity structure of the government debt nor changes in the maturity structure exert any significant, lasting or transient, influence on the relation between the two rates. This conclusion is supported by lack of positive evidence that these variables affect the spread in the generally supposed direction, and is reinforced by the consideration that the behavior of the spread can be accounted for quite closely without any reference to such variables, implying that their effect, if at all present, could be only of a secondary order of magnitude. This is not to say, of course, that there are no supply-demand effects, but merely that we could find no evidence that operations on the government component of the supply have a noticeable effect on the term structure as defined.

Ironically enough, this finding should be a source of relief to the authorities concerned with debt management because it turns out that,

[13] To illustrate, when we add to (7) the two variables that De Leeuw found to have significant effects in the predicted direction, namely, the change in the proportion of short-term debt, $\Delta[D_S/D_T]$, and the change in the proportion of intermediate debt, $\Delta[D_I/D_T]$, we find

$$S_t = \underset{(0.061)}{1.233} - \underset{(0.040)}{0.709}r_t + \sum_{i=1}^{16} \beta_i r_{t-i} + \underset{(1.20)}{0.93}\,\Delta\left[\frac{D_S}{D_T}\right] - \underset{(1.49)}{0.97}\,\Delta\left[\frac{D_I}{D_T}\right]$$

The variables D_S and D_I were calculated with an elaborate averaging technique by the flow-of-funds section of the Federal Reserve Board, who generously made them available to us. We have also tried using the proportion of short and long debt, the changes in these proportions, the average length to maturity of publicly-held debt, and other such variables, and in no case were significant supply effects in evidence. This leads us to suspect that the significant coefficients that De Leeuw found in his original studies must be a spurious result.

with a very few and fleeting exceptions, the combined result of Federal Reserve-Treasury debt management since the last quarter of 1961 was to lengthen steadily the maturity of the debt held by the public, reversing a previous trend. While in the first quarter of 1960 the average maturity stood at 4.3 years, the lowest figure on record until that time, by the second quarter of 1965 it stood at 5.7 years. Thus, if lengthening the maturity had the usually supposed effect of increasing the spread, then debt management would have pretty consistently worked toward defeating the goal of Operation Twist.

IV. *Testing the Effects of Operation Twist*

Since equation (7) has sound theoretical underpinnings, as well as strong empirical support, it should provide a sensible basis for a test of the effectiveness of Operation Twist. To this end we extrapolated this equation from the first quarter of 1962 to 1965-II, and the result is graphed as a dashed curve in the top panel of Figure 2. It is apparent that, through the middle of 1964, there is very little evidence that these policies produced an appreciable effect on the term structure. With but a couple of exceptions, the error is within ten base points, or less than the standard error; beginning with the second quarter of 1962, however, the spread is consistently smaller than the computed value and, beginning with 1964-IV, the difference becomes impressive, four to six times the standard error. Thus the best that could be said on the basis of (7) is that the twist policy was slightly to moderately successful.

Since our results indicate that this success is not attributable to debt management by the Treasury or the Federal Reserve, we must consider whether the only other major tool applied might be responsible for what twisting took place. That was, as we noted, the successive increases in the ceiling rates on time deposits under Regulation Q. Particularly noteworthy is the fact that the major increases in the ceiling rate came precisely at the beginning of 1962 and again in the last quarter of 1964. But while the coincidence of dates is suggestive, it is at best circumstantial evidence. To put our case on a solid footing, we must specify the mechanism by which an increase in the ceilings on interest payable to savings deposits could be expected to affect the spread, and then look for direct evidence that this mechanism was actually at work in the period under consideration.

To see what light the Habitat Theory can shed on the nature of the mechanism, we note once more that the rather large constant term in (7) suggests that during the postwar period the expected return from long-term bonds tended to exceed the short rate by a positive premium. According to the Habitat model the prevalence of such a positive premium would indicate a systematic tendency for the primary supply of

Equation (7) Extrapolated

Equation (8) Actual and Fitted Values
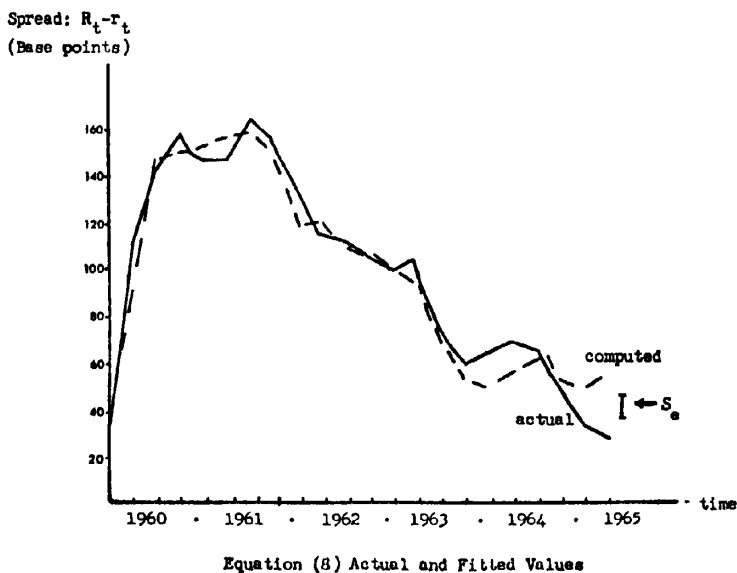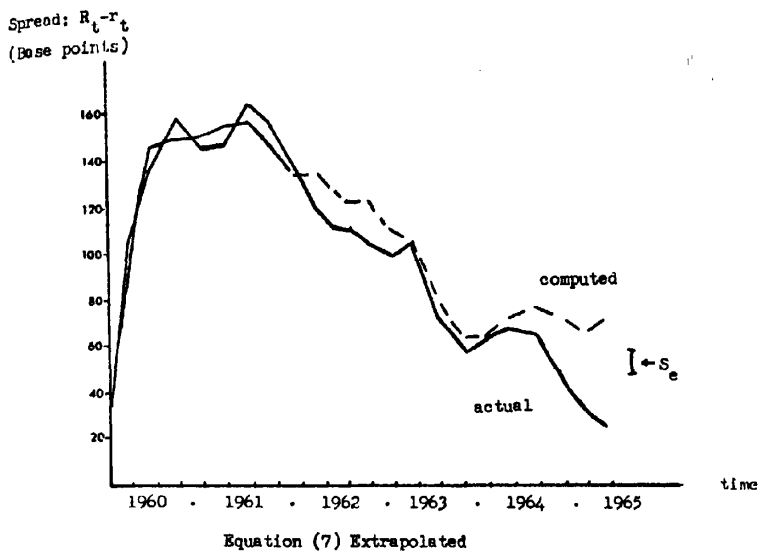
FIGURE 2

funds to exceed the primary demand in the short market and to fall short of the primary demand in the long market. We have further seen that under these conditions the size of the premium on longs would depend, among other things, on the "facilities for effective arbitrage operations." In particular, we should expect that any significant impediment to arbitrage, such as a curtailment of the ability of a certain class of would-be arbitrageurs to attract short-term funds with a rate as high as they would otherwise be prepared to pay, would tend to raise the premium. Among such potential arbitrageurs one presumably would include commercial banks, hence the Regulation Q ceiling on time deposit rates (if sufficiently low to be effective) would be a force creating an artificially large premium. Thus, we would presume that increases in the ceiling rate would tend to reduce the spread by allowing banks to arbitrage away part of this premium.

This theoretical formulation suggests that to measure the effect of Regulation Q we need to introduce a variable which (1) should treat the successive lifting of the ceilings, not as positive forces contributing to twist, but rather as the removal of an interference with normal arbitrage operations; and (2) should play the largest role when other short-term rates are very close to or above ceiling; while it should cease to have effect once the ceiling is sufficiently above these rates. Beyond that level, changes in the ceiling should no longer affect the spread. Thus we define a variable, Q, as follows:

$$Q_t = r_t - (q_t - a) \qquad \text{if positive, zero otherwise}$$

where q_t is the ceiling rate under Regulation Q and $(r_t + a)$ is a threshold level such that any higher ceiling would be irrelevant at time t. Just how high the gap, a, should be is hard to guess a priori, and depends in a large measure upon what rate is used for r_t. Since we are dealing with the market for government securities it seems sensible to use the treasury bills rate itself for r_t. For a we assumed somewhat arbitrarily a value of one hundred base points.[14]

When we fit our regression model, including the variable Q, for the entire period from 1952 to mid-1965, we find that the coefficient of the variable Q has the expected positive sign, although it is on the borderline of statistical significance.[15] It is also rather small, as it implies that when r_t equals the ceiling, the premium is only ten base points higher than it would be in the absence of an effective ceiling. The marginal

[14] We use the ceiling rate q_t rather than the average rate actually offered by banks because q_t is the policy variable whose effect we wish to estimate. See also footnote 15.

[15] Nearly identical results are obtained if Q is entered with a one-quarter lag, raising the possibility that a short distributed lag on Q might improve the results, although we have not investigated this approach. It was also found that if the threshold level, a, was chosen to be 50 base points rather than 100, the same qualitative results were obtained.

statistical significance and the small magnitude of the coefficient estimated for Q raises the possibility that other events in the period after 1961 are causing a spurious effect. One such major development, and one that could have affected the ability of commercial banks to attract short-term funds, was the introduction in 1961 of negotiable Time Certificates of Deposit (CD's). To be sure, the spectacular growth of this instrument after 1962 could not have occurred had not the ceiling been raised that year, so that banks could offer CD's at rates competitive with other short-term instruments. Nonetheless, the CD must be regarded as a true financial innovation which could have enhanced the capacity of banks to arbitrage even if Regulation Q had never existed.

To test the effect, if any, of this innovation, one could rely on the dummy variable technique, adding to (7) a variable taking the value one after 1962 and zero everywhere else. This would allow the constant term of the equation (which is a measure of the risk premium) to assume two values: one value for pre-1962 and a second, lower value (the sum of the constant term and the coefficient of the dummy variable) after 1962.

When we entered into the regression such a dummy—denoted by Z in the equation below—its coefficient has the expected negative sign and is significant in relation to its standard error:

$$S_t = \underset{(0.064)}{1.278} - \underset{(0.031)}{0.695}r_t + \sum_{i=1}^{16} \alpha_i r_{t-i} - \underset{(0.043)}{0.124}Z \qquad (8)$$

[16]

$$R^2 = 0.964 \qquad S_e = 0.103 \qquad DW = 1.02$$

Actual and computed values for this equation are shown in panel 2 of Figure 2.

When Q is added along with Z in a similar regression, the coefficient of Q not only loses its statistical significance but actually becomes negative. This result is confirmed by rerunning (7) with Q but not Z for the period before 1962, thereby excluding the CD years altogether. The coefficient of Q is again negative. This outcome suggests that the successive increases in the ceiling contributed to twist solely by permitting

[16] The use of a dummy variable in (8) may appear less than satisfactory, failing to bring out the fact that the contribution of the newly introduced CD's depends on whether the ceiling rate is sufficiently high to enable banks to offer rates competitive with other short-term instruments. With these considerations in mind, we had actually defined the variable Z to take (1) the value 1 only when the ceiling q_t exceeds r_t by at least 50 base points, a gap which we assumed to be sufficiently large to give banks the needed elbow room; (2) the value zero when r_t equals the ceiling, by which time banks would likely lose nearly all power of attracting CD funds; and (3) to decrease linearly between these limits as $(q_t - r_t)$ shrinks from 50 base points to zero. As it turns out, however, from 1962 to 1965-II the ceiling was consistently kept at least 50 base points above r_t. (Note in this connection that for the last quarter of 1964 the ceiling was taken as 4.25, a simple average of the 4 percent rate ruling up to November 24 and the 4.5 rate ruling thereafter.) Therefore, the variable defined above always has the value one, and is undistinguishable from an ordinary dummy. But in extrapolating (8) beyond the period of observation, care should be taken if r_t gets too close to the ceiling, as it has done very recently.

the invention of CD's—for which Operation Twist cannot properly claim credit—to exercise its maximum effect, some twelve base points according to the estimate of equation (8).[17] It should be acknowledged, however, that our results do not effectively enable us to ascertain whether the coefficient of the dummy variable Z measures just the effect of CD's, as intended, or whether it also picks up other, yet unspecified effects of Operation Twist, including possibly psychological effects modifying expectations.

In concluding, we wish to emphasize that the results we have reported represent but the preliminary findings of a continuing study of the determinants of the maturity structure of interest rates. We can indicate, however, that these results are broadly supported by a similar study of the behavior of the spread in the corporate market between the yield on Aaa rated bonds and the commercial paper rate, both for the postwar period and for the longer span beginning with the inception of the Federal Reserve System. At this stage we feel that the following conclusions can be advanced with considerable confidence.

1. The expectation model can account remarkably well for the relation between short- and long-term rates in the United States. Furthermore, the prevailing expectations of long-term rates involve a blending of extrapolation of very recent changes and regression toward a long-term normal level.

2. There is no evidence that the maturity structure of the federal debt, or changes in this structure, exert a significant, lasting or transient, influence on the relation between the two rates.

3. The spread between long and short rates in the government market since the inception of Operation Twist was on the average some twelve base points below what one might infer from the pre-Operation Twist relation. This discrepancy seems to be largely attributable to the successive increase in the ceiling rate under Regulation Q which enabled the newly invented CD's to exercise their maximum influence.

4. Any effects, direct or indirect, of Operation Twist in narrowing the spread which further study might establish, are most unlikely to exceed some ten to twenty base points—a reduction that can be considered moderate at best.

[17] Note that if Z is interpreted along the lines of footnote 16, then for the initial period 1962-I to 1963-II when r_t remained below the old ceiling rate of 3 percent, the introduction of CD's would have contributed some to the closing of the spread even if the ceiling had not been raised. Thus for this initial span the contribution of the increase in ceiling as such must be estimated at somewhat less than 12 points, although how much less it is not really possible to say with confidence.

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DISCUSSION

WARREN L. SMITH: Many of the innovations in banking so ably discussed by Meigs—including the underwriting of state and local government security issues, the expanded activities in connection with the management of private pension funds, and the development of new techniques of business financing—have as their main result an increase in competition in some financial markets and in all probability an increase in the efficiency of capital allocation. No doubt they will have some effects on the functioning of monetary policy, but these effects are probably limited, and, in any case, I find them difficult to evaluate.

The innovations that do appear to have important and discernible implications for monetary policy are the new techniques that commercial banks have developed as a means of attracting funds: the negotiable time certificate of deposit and, more recently, the new promissory note. Prior to the development of these instruments, I was inclined to doubt the importance of nonbank financial intermediaries—in which category I include the time-deposit-related activities of commercial banks—as a factor influencing the effectiveness of countercyclical monetary policy. Systematic two-way interest-induced substitution between demand deposits and time deposits seemed to me to be probably of little importance when the vast bulk of time deposits was held by households, and the empirical evidence appeared to support my position.

However, the rise of the CD and the associated attraction of more sophisticated investors—especially corporations—into the market for time deposits has caused me to revise my views somewhat concerning the impact of substitutions involving time deposits on the effectiveness of monetary policy. The important substitution in this new setting, however, seems to be not between time deposits and demand deposits but between time deposits and securities. When market interest rates on short-term debt instruments rise in relation to interest rates on time deposits—which may be rigid or relatively sticky due to the existence of Regulation Q ceilings or for other reasons—the new sophisticated holders will tend to switch from time deposits into securities. If X dollars are switched in this way, the immediate effects are (1) to increase the demand for securities by X dollars and (2) to increase the stock of demand deposits by X dollars (as investors convert time deposits into demand deposits which they use to buy securities). If the reserve requirement for demand deposits is r_d and that for time deposits is r_t, this operation will reduce the excess reserves of the banking system by $(r_d - r_t)X$ and (disregarding cash drain) force the banking system to contract its loans and invesments and the supply of demand deposits by $(r_d - r_t)X/r_d$. Thus, the net increase in the demand for securities (ΔS) and in the supply of demand deposits (ΔM) will be $X - (r_d - r_t)X/r_d$, which reduces to

$$\Delta S = \Delta M = \frac{r_t}{r_d} X.$$

At present levels of reserve requirements for member banks of 4 percent for time deposits and about 15 percent for demand deposits, a shift of $1 million from time deposits to securities would increase the net demand for securities and the supply of demand deposits by about $267,000. The effect is to reduce the amount by which a given volume of open market sales of securities by the Federal Reserve will (1) reduce the money supply and (2) raise the level of interest rates.[1] If security rates move substantially relative to rates on time deposits and if investors are sensitive to these rate movements, the effects can be substantial.

In several important respects, the implications of interest-induced substitution between time deposits and securities are quite different from those of the direct substitution between time deposits and demand deposits that was emphasized in the earlier discussion of financial intermediaries. For example, the impact of substitution between time deposits and demand deposits on the effectiveness of monetary policy would be minimized if interest rates on time deposits were not permitted to fluctuate and if the reserve requirements on the two types of desposits were equalized so that substitutions between them would not affect the banking system's demand for loans and securities. Thus, under these conditions the existence of effective ceiling rates on time deposits under Regulation Q would be likely to strengthen anti-inflationary monetary policy. However, if the dominant substitution is between time deposits and securities, the strength of monetary policy will be increased by the removal of all impediments to fluctuations of time-deposit interest rates and by the complete elimination of reserve requirements on time deposits.

If the rise of the CD and related developments have made substitution between time deposits and securities a more important phenomenon than substitution between demand deposits and time deposits, as I believe to be the case, some strengthening of monetary policy might be achieved by the adoption of two reforms: (1) removal of the ceiling rates applicable to time deposits under Regulation Q, and (2) elimination of reserve requirements for time deposits.[2]

The phenomenon of interest-induced substitution between time deposits and securities has the effect of making the money supply function interest elastic. It is now generally recognized that banks adjust their borrowings from the Federal Reserve and their holdings of excess reserves to changes in short-term

[1] The substitution effect that is discussed in isolation here is developed in the context of a model of the relevant financial markets in my paper entitled, "Time Deposits, Free Reserves, and Monetary Policy," which is scheduled for publication in the near future. The conclusions presented above are found to hold true within this more complete framework.

[2] It should be noted, however, that if substitution between demand deposits and time deposits is more important than substitution between time deposits and securities, the optimal arrangement from the standpoint of strengthening monetary policy would be to retain the Regulation Q ceilings and make reserve requirements for time deposits the same as for demand deposits. The general principle is that if time deposits are regarded by investors as closer substitutes for securities than for demand deposits, they should be made as similar to securities as possible; on the other hand if they are regarded as closer substitutes for demand deposits than for securities, they should be made as similar to demand deposits as possible.

market interest rates relative to the Federal Reserve discount rate. These adjustments add another element of interest-elasticity to the money supply function. A third reform, under which the discount rate would be set substantially —perhaps 1 percentage point—above the Treasury bill rate and adjusted each week to maintain this differential, should eliminate most of these interest-induced responses of member-bank borrowing and excess reserves. The combined result of the three proposed reforms should be to reduce substantially the interest-elasticity of the money supply function. The effect on the potency of monetary policy would be precisely the same as would be produced by a reduction of the interest-elasticity of the demand for money.

The paper by Modigliani and Sutch is an important contribution to the research that is now in progress on the determinants of the term structure of interest rates. I have only one comment to make concerning it. While I have always believed that expectations are the major factor shaping the term structure, I find it difficult to believe that debt management is without significant influence. One reason why the effects of debt management may be difficult to detect is that the Treasury itself is influenced by the interest rate structure in the conduct of its debt-management operations. To illustrate the problem, suppose the treasury were to conduct an orthodox countercyclical debt-management policy. This would mean that the issuance of short-term debt would be emphasized in recessions when interest rates were relatively low and the differential between short- and long-term rates was large. The result would be an apparent association between shortening of debt maturities and widening of rate differentials even though the response of the market to the Treasury's action was such as to make the differential smaller than it would have been had the action not been taken. While no such simple hypothesis as this could adequately characterize the Treasury's approach to debt management, it seems clear that there is a serious identification problem here, which may make it impossible to determine the effect of debt management on the rate structure without incorporating into the model a description of the behavior of the Treasury itself.

ALBERT R. KOCH: In his paper, Governor Mitchell has sketched in some of the likely economic effects of the technological revolution that is gathering momentum in the banking system, particularly with reference to the payments mechanism and banking structure. If anything, I suspect his predicton of a sharp drop in demand balances is under- rather than overestimated. This means that for banks to continue to grow and to provide facilities in the same magnitude as now, they must become even more important financial intermediaries for savings. This means, in turn, that the pace of bank credit expansion per se will become a less and less significant indicator of the contribution of monetary policy to either inflation or deflation.

The increased importance of commercial banks as financial intermediaries has, of course, been developing now for some years, not only for technological reasons. It has been reflected in innovation in the development of new savings

instruments and has opened up new opportunities for longer-term bank lending and investing.

One final point that Governor Mitchell properly recognizes is the validation problem in a payments system such as he envisages. He suggests the possibility of the use of voice "fingerprinting." But such verification, say from San Francisco to New York, would not likely be cheap even allowing for the fact that further technological development will occur in the communications industry as well as in banking. And what if the chap involved had a cold?

This final point also raises the all-important question of the likely costs of automation in banking. There will be plenty of jobs for economists comparing costs and benefits of new ways of performing services as the brave new world of finance unfolds.

Let me turn now to James Meigs's paper on recent innovations in the functioning of banks. I shall focus my remarks on the last half of Meigs's paper where he addresses himself to three important questions: (1) Have longer maturities of bank assets and the growing dependence upon time deposits made the banking system less stable? (2) Have bank innovations loosened the links between central bank actions and the money supply? (3) Have bank innovations loosened the links connecting money supply with income and prices?

With Meigs's answer to the first question I have little to add. I have been struck, as he has, by the increasing tendency of banks to pay more and more for shorter and shorter-term funds and to invest such funds in longer and longer-term assets.

The bank regulatory agencies have played an important part in this movement. To slow up the flow of short-term capital abroad and at the same time to continue to stimulate domestic investment, they have not only given banks the opportunity to borrow short and lend long, but have urged them to do so.

The bank supervisory agencies should concern themselves much more with the liquidity surveillance of banks. In particular, they should know much more than they currently do about the liquidity and quality of the various kinds of bank assets and about the likely liquidity requirements of the different types of bank deposits and other liabilities.

I should like to comment on Meigs's last two questions jointly. His summary answers to them are: (1) that the relationship between central bank actions and the money stock is a fairly stable function if one takes into account a sufficiently large number of variables; and (2) that the links between the money stock and income and prices are also relatively stable at some definition of money.

I do not disagree with Meigs's first conclusion. The Federal Reserve can influence the stock of money, and the relationship between the stock of money and the monetary policy instrument variables is reasonably determinate. However, I feel it is much more appropriate to consider the money stock as an endogenous variable of the system, for movements in it can be a highly misleading indicator of the existing degree of monetary restraint or ease.

As for Meigs's second conclusion, this poses the familiar Gurley-Shaw ques-

tion, raised again by Messrs. Gramley and Chase in an article in the October *Federal Reserve Bulletin;* namely, has the development of new types of bank liabilities affected both the guides to Federal Reserve policy and the likely effectiveness of such policy.

As for policy guides, unfortunately it is not enough to agree in principle on a matter such as the relationship between the quantity of money and income. The problem is to ferret out the rationale for the relationship. This can be done only by spelling out in detail the response of spending and investing decisions to changes in monetary policy that alter, among other things, the money stock.

As Meigs notes, the control characteristics of the system have no doubt changed as a result of recent banking innovations. That is precisely the problem that puts us in our present quandary.

We need much more information about the behavioral aspects of the ownership and use of money if we are to find money stock-income relationships that are helpful for policy purposes. In this connection, we at the Federal Reserve have been culpable for not having pushed ahead more aggressively to collect more comprehensive and meaningful information on deposit ownership and use, but I am happy to report we plan to do so again, hopefully in the not too distant future.

As for the effectiveness of Federal Reserve policy under this new set of bank liabilities, I suspect that the Federal Reserve can get more "bang from its open market buck" now than it could earlier. I need only mention the bind that banks were getting into this fall before the Federal Reserve raised the maximum ceiling rates payable on certain time deposits.

Finally, let me turn briefly to the Modigliani-Sutch paper. I am quite sympathetic with the authors' conclusion that much of the decline in the spread between short- and long-term interest rates that has occurred over the course of the current long economic expansion can be explained by normal cyclical factors rather than by Operation Twist, defined to include Treasury management of the maturity structure of the government debt and the raising of maximum rates payable by commercial banks on their time and savings deposits.

In the case of Treasury debt management, however, I wonder whether the minimum effects of recent debt lengthening on the interest rate structure were not, in part at least, a reflection of the innovation of the advanced refunding technique. Were the Treasury to have achieved as much debt lengthening through the cash sale of long-term securities, the effects on the term structure of rates would no doubt have been much more pronounced.

As for the effects of the increases in Regulation Q ceiling rates on the term structure of interest rates, they have had important temporary if not permanent effects. To cite one example, in the first half of 1962, the course of long-term rates, particularly those on municipal securities, did seem to be significantly affected by the raising of Q ceilings at the beginning of the year and the subsequent sharp rise in the time and savings deposits of banks.

My final comment concerns the authors' conclusions that the spread be-

tween short- and long-term rates depends essentially on expectations regarding future rates, which in turn depend mainly on weighted averages of past interest rates or of past changes in such rates. In deciding whether interest rates will rise, fall, or remain unchanged in the months ahead, can banks, security dealers, and other investors do no better than look back at the past and make some simple extrapolation of it? Are not the future courses of business activity, credit demands, and monetary policy likely to be important factors in the formation of interest rate expectations? Yet the Modigliani-Sutch approach pays no explicit attention to such factors. One wonders whether the past interest rate variables are "standing in" for some of these other influences. If so, one wonders whether the Modigliani-Sutch equations will hold up as well in the future as they have in the past.

An implication of the sole reliance on expectational variables in the making of investment decisions is that at some structure of rates—a structure which just compensates for expected capital gains or losses and liquidity needs—securities of all maturities are very close substitutes for all investors. The facts that regardless of rate spreads insurance companies hold almost entirely long-term securities, business corporations concentrate on the short end, and banks specialize in the short- and intermediate-term range seem difficult to reconcile with this implication. In any case, the Modigliani-Sutch position would be much more convincing if an examination of portfolios disaggregated by type of institution confirmed the hypothesis that even within some narrow portion of the security spectrum these institutions were sensitive to the rate structure in adjusting their portfolios.

Burton G. Malkiel: Dr. Meigs raises the interesting question of whether or not the recent innovations in banking may, in some sense, have made the banking system less stable. He emphasizes, in particular, the potentially mercurial qualities of the new certificates of deposit. If market rates were to rise above the ceiling rates imposed under Regulation Q, a massive shift out of CD's into other money market instruments could occur. Meigs further notes that the changes in the relationship between market interest rates and ceiling rates during recent periods have tended to shorten the maturity distribution of the CD's issued. For the present, at least, Regulation Q poses no difficulty on either count. The current 5½ percent interest rate ceiling affects neither the volume nor the maturity composition of the CD's issued.

Nevertheless, I believe that the development of the CD does raise additional troublesome questions. Another effect of the CD has been its contribution to an increase in the maturity of bank assets. Before the development of the certificate of deposit, banks relied on a secondary reserve of short-term liquid assets (typically Treasury bills) to meet any sudden increase in loan demand or any large deposit withdrawal. Now commercial bankers feel that there is less need for a secondary (or so-called "liquidity") reserve of short-term liquid assets. Instead, these bankers rely more heavily on the creation of new liabilities, namely, the sale of certificates of deposit, than on the sale of short-term governments as a source of funds. Thus, the development of the CD has

supported a lengthening of the maturity of bank assets. Especially noteworthy has been the substantial increase in the granting of term loans and the increased holdings of intermediate-term tax exempts.

Even if there were no potential liquidity problem—that is, even if CD's could always be sold to meet any need for funds—these developments in bank portfolio management could have disturbing consequences. To the extent that a bank simultaneously increases both its short-term interest-bearing liabilities and the maturity of its assets. it will tend to increase the variance of its earnings. For example, if the level of interest rates rises, the increased costs of the short-term interest-bearing liabilities will be reflected quickly in the income statement, particularly since short rates are more volatile than long rates. On the other hand, because the assets tend to be long term, earnings will not increase correspondingly, and net income will decline. Indeed, the longer the maturity of the assets, the larger will tend to be the capital losses that are suffered. Needless to say, increases in the variability of its earnings could ultimately make it more difficult for the bank to market its CD's. This underlines the necessity for distinguishing carefully between owned reserves and borrowed reserves, a distinction that has become indispensable in discussions concerning the international monetary system.

If, in fact, bankers increased the maturity of their assets and decreased the maturity of their interest-bearing liabilities specifically to take advantage of an expected fall in interest rates, one would be less inclined to question the banks' prudence. Indeed, this is precisely the kind of arbitraging operation that economists have relied upon to impose the equilibrium conditions of the expectations theory of the term structure of interest rates. In the present case, however, as Dr. Meigs has pointed out, the maturity of the CD's has been kept very short by the interest rate ceilings. Moreover, in many cases the lengthening of assets was not the result of a conscious attempt to take advantage of higher holding period yields, but resulted instead from a perceived need to invest in a security having a current earnings rate exceeding the rate on the CD.

Compounding these potentially disturbing developments in bank portfolio management are the recent changes in the capital structures of commercial banks. In recent years, banks have tended to shift from a pure equity capitalization to a more levered capital structure by issuing new debentures. This more levered capital structure also tends to increase the variance of earnings and, indeed, to increase the stockholders' risk of ultimate ruin. The relative decline of price earnings multiples for bank stocks is dramatic evidence that these factors have not gone unnoticed in the securities markets. The added risk to the stockholders may be compensated for by an increased expected rate of return. From the point of view of the monetary authorities, however, the increased risk to large depositors is the more important consideration. While it is too early to come to a final verdict, I would suggest that these innovations are far from an unmixed blessing for the banking system. It seems to me they put a greatly increased burden on bank examiners to protect the public inter-

est. At the very least, these developments call for more intense theoretical and empirical study than they have thus far received.

The Modigliani and Sutch (M & S) paper concentrates on the important question of the effectiveness of Operation Twist in altering the term structure of interest rates. Their paper is an important one which, I believe, makes a significant contribution to the empirical literature on the subject. M & S first formulate an eclectic theoretical model of the term structure that is both simple and appealing. Then, following De Leeuw's sensible approach, they proceed to show (1) that changes in the relative supplies of debt instruments exert a negligible influence on the rate structure and (2) that expectational variables alone account quite closely for term structure relations both before and after the period when Operation Twist commenced. Despite my sympathy with their basic approach, however, I am not convinced that their useful work finally settles the question and that we must conclude that the monetary authorities are powerless to alter the rate structure. There are two issues I would like to raise.

In the first place, I am not at all surprised that the supply variables themselves did not prove to be significant. One reason for our inability to detect the effect of changes in the supply of debt instruments on the rate structure may be that there have been only very moderate changes in relative supplies. Moreover, the analysis cannot be confined to the federal debt because there exist several similar types of securities that are highly substitutable for governments. Indeed, several additional variables on both the supply and demand sides of the market are relevant. Finally, there is the problem of the appropriate classification of the maturity distribution of the debt. While M & S recognize the difficulties involved, they state that since 1961 the federal debt has been lengthened and they suggest that debt-management operations were actually perverse. They neglect to point out that the relative supply of very short-term securities did increase during the early part of the period. I have argued elsewhere that changes in relative supplies of the very short-term issues can be expected to have a far greater effect on the rate structure than a refunding of ten-year bonds into thirty-year issues. In any event, supporters of Twist would argue that the very presence in the market of a buyer with essentially unlimited resources is likely to exert a psychological impact on the formation of expectations which can be far more important than the actual number of dollars committed. Thus, even if the actual changes in the maturity composition of the debt effected under Operation Twist were negligible (or partly perverse), it is not fair to conclude that the policy was ineffective.

The second issue I would like to raise is a more fundamental one. Several alternative and equally legitimate specifications of the expectations hypothesis can be found which fit the data reasonably well during the pre-Twist period. With their specification, M & S can also explain the post-Twist observations and thus they conclude that the effect of Twist was insignificant. With other specifications, however, it can be shown that the underlying structure generating the observations has changed in the post-Twist period and that relative

interest rates have been altered significantly. Since both types of specifications can explain the pre-Twist data, and since there seem to be no compelling reasons to choose among the models on a priori grounds, it is not possible to obtain a decisive answer to the policy question at hand.

Let me illustrate this point with two examples: One is taken from the work of Neil Wallace[1] and the other utilizes my own results. Using quarterly postwar data on government yields from 1946 through 1962, Wallace first developed parameter estimates for Meiselman's error-learning equation supplemented by an additional term measuring the effect of changes in the maturity composition of the federal debt. He then used these parameter estimates (together with 1963 data on one-year spot rates and on the maturity composition of the debt) to predict the rates on four-year government bonds during 1963. The predictions reveal that the monetary authorities have been far more successful in twisting the rate structure than would have been suggested by the regression results. When the rising short-term rates and perverse supply changes of 1963 were used to generate predictions of the four-year rate, Wallace found that the four-year rate was consistently overestimated. For example, the difference between the four-year and one-year rate in September, 1963, was only 27 basis points. The regression results predicted a difference of about 59 basis points. Wallace's results suggest that the policy pronouncements of the monetary authorities may have altered considerably the market's expectations.[2]

My own empirical work also supports the possibility of deliberate alteration of the rate structure. I used a model similar to that of M & S except for two differences in specification. First, I used the long rate rather than the short rate on the right-hand side of the estimating equation. Second, I employed a much longer period of market history and a different weighting procedure to form the average of past rates used in the equation. Applying the Chow test to the subperiods, January, 1957, to January, 1961 (pre-Twist), and April, 1961, to April, 1965 (post-Twist), I was able to reject the hypothesis that the post-Twist observations were generated by the same structure as the first set.

Lest my differences with M & S appear much wider than they are, let me conclude on a note of agreement. I strongly support M & S's suggestion that the development of the CD has probably been far more influential in altering the term structure of interest rates than was Operation Twist itself. During the past few years over $16 billion in short-term securities, which are closely substitutable for Treasury bills, were added to the supply of short-term instruments. The proceeds from these sales were typically invested in longer-term securities, as I previously mentioned in my discussion of Dr. Meigs's paper. Since the new securities bought were often municipals, these operations perhaps had a more noticeable effect in reducing the spread between federal

[1] Neil Wallace, "The Term Structure of Interest Rates and the Maturity Composition of the Federal Debt" (unpublished doctoral dissertation submitted to the Univ. of Chicago, Dec., 1964), pp. 45-47.

[2] Support for this viewpoint may be found in Harry G. Johnson, "Issues in Monetary and Fiscal Policies," *Fed. Res. Bul*, Nov., 1964, p. 1410.

government and municipal securities. This offers some explanation for the evidence reported in row 3 of M & S's Table 1-B.

I would like to append a few remarks about the effects on bank concentration of these financial innovations and the technological advances mentioned by Dr. Mitchell. The certificate of deposit has tended to favor large banks over smaller banks by enabling the large bank to attract deposits far beyond its normal service area. Because of their size and financial reputation, the bigger banks were better able to tap the large national pool of short-term funds. Indeed, when CD rates were pressing on the Regulation Q ceiling, the nonprime institutions were effectively frozen out of the market because they could not sell their notes at the same interest rate as the large banks. In addition, because a $1-million CD was the standard unit of trading in the secondary market, the market favored the CD's of large banks who were willing to issue the higher denominations.

In Dr. Mitchell's fascinating vision of the future, we see that computerizing the relationship between the demand depositor and his bank will make it practicable and profitable for large banks to extend their present service areas even further. There will be no need to have a physical location contiguous to the customer-depositor. The major competitive weapon will be an efficiently running computer system for customer accounting, billing, and payment for personnel and other services. It is hard to imagine that this world of nationwide banking service could be established without great reductions in both the actual and the ideal number of banking units.

PUBLIC FINANCE: PROMOTION OF KNOWLEDGE PRODUCTION AND INNOVATION

TAX TREATMENT OF INDIVIDUAL EXPENDITURES FOR EDUCATION AND RESEARCH

By RICHARD B. GOODE
International Monetary Fund

For economists the tax treatment of education and research is important as a determinant of individual tax liability, as a question of public policy, and as an application of the concept of human capital.

In this paper I shall use the word "education" in the restricted sense of "schooling" and concern myself with individual expenditures connected with courses at trade and vocational schools, colleges, and universities. By "research" I shall mean individual efforts, other than those of students or candidates for university degrees, to contribute to knowledge or to discover new or improved products or processes. Thus I leave out of account activities such as private study, travel, and attendance at professional meetings, which perform some of the same functions as schooling and research. The tax treatment to be considered is exclusively that accorded individuals under the federal income tax in the United States.

Education

Broadly speaking, the intent of U.S. Treasury regulations appears to be to allow deductions against taxable income of educational expenditures that are costs of maintaining earning capacity but not to allow deductions for the cost of creating earning capacity. No provision is made for the amortization of educational outlays that create human capital.

The regulations allow deductions for educational expenditures "undertaken primarily for the purpose of: (1) Maintaining or improving skills required by the taxpayer in his employment or other trade or business, or (2) Meeting the express requirements of a taxpayer's employer, or the requirements of applicable law or regulations, imposed as a condition to the retention by the taxpayer of his salary, status, or employment." Deductions are not allowed for expenditures for education "to meet the minimum requirements for qualification or establishment in . . . [a] trade or business or speciality therein. . . ."[1] While a

[1] U.S. Treasury Regulations 1.162-5.

later promotion or salary increase attributable to the education is not an insuperable obstacle to a deduction, advancement must not be the primary purpose.

An illustration given in the official regulations draws an instructive contrast between two teachers. A, who is a public school teacher, is required by her employer or by law "either to read a list of books or to take certain courses" in order to hold her job. She chooses the courses and is allowed to deduct her expenses, though she incidentally acquires a master's degree and in consequence a salary increase. B, a graduate student and teaching assistant at a university, aspires to become a regular faculty member and, as he and the Commissioner of Internal Revenue both recognize, must obtain a Ph.D. to qualify. B cannot deduct expenses incurred in studying for the Ph.D., because he has not yet met the minimum requirement for his chosen career.

Although the present regulations may seem restrictive, they represent a great liberalization of previous policies. The regulations were adopted in 1958 after a series of court decisions overruling the Treasury's denial of deductions for virtually all educational expenses.

The educational expenditures that are not deductible are characterized by the Treasury Department as "personal expenditures." Most of the illustrations given in the regulations, however, as well as published rulings and court decisions, refer to expenditures that are intended to create earning capacity and that could be regarded as capital outlays. A sounder conceptual basis for the regulations would have been a threefold classification of educational expenditures as (1) maintenance costs, (2) capital outlays, and (3) consumption. The Treasury Department might have argued that current deductions are inappropriate for capital outlays and that, under the statutes and court decisions, the outlays cannot be amortized because the length of life of the intangible capital asset and hence the correct amortization period are highly uncertain.[2] This would have had the great merit of clarifying the issues and focusing attention on the question whether the Internal Revenue Code should be amended. In defense of the Treasury Department it may be said that, though the idea of human capital formation through education was not unknown in 1958, it had received far less attention up to that time than in recent years.

In order to define net income more accurately for tax purposes, expenditures for education undertaken to maintain or increase earning power should be offset against the income that they generate. This would require, in addition to the present deductions for "maintenance"

[2] Bernard Wolfman, "Professors and the 'Ordinary and Necessary' Business Expense," *Univ. of Pennsylvania Law Rev.*, June, 1964, pp. 1089-1115.

costs, the recognition for tax purposes of the concept of educational capital and the introduction of proper amortization or depreciation allowances for it. The innovation would appear to be consistent with income tax principles and equity and to be good public policy in view of the great contribution that education makes to economic progress.

Conceptual and practical difficulties would be involved in revising the treatment of educational expenditures. Perhaps the most serious difficulty would be to distinguish between costs of earning income, including both maintenance and capital costs, and the consumption element of education. Generally, in determining business and professional income, an outlay is regarded as a cost if the purpose is to produce income, even though no direct connection can be traced between the outlay and an income receipt. This principle suggests that it would not be necessary to require a showing that income actually resulted from a particular educational expenditure in order to support its deduction or capitalization but only that the outlay might reasonably have been expected to produce income. To minimize subjectivity in the classification of educational expenditures, criteria might be established in the statutes, regulations, and rulings partly on the basis of statistical studies of the relation between education and earnings.

There is little doubt that attendance at professional schools and at trade and vocational schools is motivated primarily by the desire to increase earning capacity. The possibility that students may derive direct satisfactions or cultural values from their studies does not change the fundamental nature of expenditures for such education any more than incidental enjoyment of business or professional activities bars the deduction of ordinary and necessary business costs. Graduate studies in arts and sciences also seem to be career-oriented in the great majority of cases.

The classification of liberal arts studies at the undergraduate level is more debatable. There is a strong tradition that regards liberal education as preparation for living rather than for earning and that would imply that expenditures for this education should be classified as consumption. Casual observation and opinion surveys, however, suggest that most college students and their parents take a more mercenary attitude, and there is good evidence that on the whole college education substantially increases earning power. The available studies indicate that male students earn a good return even if their college expenditures are viewed entirely as investment and any consumption element ignored.[8] For women, the picture is less clear because it is not feasible to take account of the nonmonetary earnings of housewives.

[8] Gary S. Becker, *Human Capital: A Theoretical and Empirical Analysis with Special Reference to Education* (N.B.E.R., 1964), pp. 69-113.

Elsewhere I have suggested that all individual costs of college education as well as costs of vocational and professional education might justifiably be considered for tax purposes as costs of earning income.[4] This rule would admittedly err on the side of liberality, since part of the expenditures are really consumption. A more conservative approach would take in only vocational and professional education. A more liberal plan would extend to secondary education.

The items to be recognized as current or capital costs would include expenditures for tuition and fees, books and supplies, and travel. Ordinary living expenses should not be included since they would have to be incurred in any case; any special living expenses should in principle be deducted or amortized, but in practice it might be better to exclude living expenses because of the difficulty of identifying special expenses. Forgone earnings, which are the largest element in private educational costs, would not need to be deducted or amortized since they are already excluded from taxable income. Educational costs financed by taxes and philanthropy would not be covered.

By analogy with the present treatment of business expenses and depreciable assets, the deduction or amortization allowance should be taken against the income of the student even when the expenses are met by parents. The expenditures by parents could be treated as gifts to their children just as is the cost of physical capital that is gratuitously transferred to children. This is a fundamental point because in our society expenditures by a parent for the education of his children cannot realistically be viewed as a means of increasing the parent's income; hence the rationalization of the deduction as a cost of earning income is inapplicable to parents.

Basically, the distinction between capital outlays and current expenses for education turns on the question whether new earning capacity is created or existing capacity is maintained. Though this test cannot be rigorously applied, it offers some guidance for income taxation. Under a revised tax treatment, the kind of educational expenditures that are deductible under present regulations might continue to be currently deducted as maintenance expenses while other eligible expenditures were capitalized. As an alternative or supplementary standard, expenditures might be currently deducted or capitalized depending on whether they are small or large relative to income.

Educational capital in the form of acquired earning capacity is an asset that normally lasts throughout a person's working life. Accounting principles would suggest that outlays for this capital be

[4] "Educational Expenditures and the Income Tax," in *Economics of Higher Education*, Selma J. Mushkin, ed. (U.S. Department of Health, Education, and Welfare, Office of Education, Bul. 1962, No. 5), pp. 281-304, and *The Individual Income Tax* (Brookings Institution, 1964), pp. 82-93.

amortized (or the asset depreciated) over the whole expected working life; the estimation of this period is less difficult than the ascertainment of the probable useful life of many assets now subject to amortization or depreciation. Provision would have to be made for later adjustment of the original estimate if earnings were prematurely ended by death, disability, or obsolescence. A straight-line, declining-balance, or other reasonable method might be used. It may be objected that the usual formulas would be inappropriate since the economic value of education tends to increase rather than decrease until middle age or later, as indicated by rising earnings curves for highly educated people.[5] I find this argument unconvincing, for two reasons. First, a large part of the increase in earnings with age seems to be due to experience or on-the-job training rather than to formal education. Second, in my opinion, income tax depreciation or amortization schedules should be regarded as a conventional means of spreading capital costs over time rather than a measure of the changing market value of an asset.

The timing of allowances, to be sure, is significant. In fact, it could be argued that amortization of educational expenditures is unnecessary to secure equal treatment of human and physical capital because forgone earnings, which are the major component of educational investment, are in effect immediately offset against taxable income. On plausible assumptions about the discount rate and the marginal tax rate, the present value of immediate deductions equal to the amount of forgone earnings exceeds the present value of a series of deferred deductions covering total educational costs. But in my judgment this does not justify the failure to amortize explicit educational costs. The omission of forgone earnings from taxable income is not peculiar to education. Individuals and business managers frequently choose to give up immediate income in order to obtain more income later; yet no one contends that this creates a tax advantage or calls in question the deductibility of ordinary costs of earning the later income.

Administrative expediency and convenience in compliance strongly suggest that the usual amortization period for educational capital might be arbitrarily set at ten to twenty years rather than the student's entire working life. This would also be in accord with the recent tendency to accelerate depreciation allowances for physical capital and the liberal treatment of research and development costs.

While a number of other problems would have to be solved in order to devise a workable plan, I think that the effort would be well

[5] On earnings, age, and education see Becker, *op. cit.*, pp. 136-52; and T. Paul Schultz, *The Distribution of Personal Income* (U.S. Congress Joint Economic Committee, 88th Cong., 2d sess., 1965), pp. 15-23, 43.

justified. Provision for the current deduction or amortization of a broad range of expenditures for income-increasing education would make the income tax more equitable and would eliminate a discrimination against a strategically important kind of capital formation.

In my judgment, however, it would be wrong to expect that this tax revision would greatly stimulate education or would have an important influence on occupational choice. According to my estimates, amortizable costs of college and university education would amount to only about one-sixth of total private costs; the remaining five-sixths consists of forgone earnings. With marginal tax rates of 20 to 25 percent, the tax saving due to amortization would amount to only about 3 to 4 percent of total private costs, and the saving would be spread over many years. The significance of amortization would diminish if scholarships became more prevalent or tax rates fell. It would increase if loans and higher tuition became more usual or tax rates rose. The amortization scheme, in any case, would have the advantage of calling attention to the investment aspects of education which have often been overlooked or misunderstood.

The amortization plan differs sharply from the proposals for a deduction or tax credit for parents of college students which have received much public and congressional attention. The deduction or credit for parents, as explained above, could not be justified as a refinement of the income definition but would have to be defended as a subsidy or encouragement of a socially desirable expenditure. As such, the allowance for parents should be expected to meet more rigorous standards of equity and effectiveness than would the deduction or amortization of costs of earning income. In particular, the allowance for parents should be compared with larger government expenditures for scholarships and student loans. This is true if one accepts a proposition that I hold to be basic to a sound income tax, that there is a presumption in favor of the deduction of all legitimate costs of producing income but a presumption against other deductions and credits.

Research

It is now possible for individuals to offset against taxable income certain expenditures for scholarly inquiry as well as many outlays for applied research. In 1963, the Commissioner of Internal Revenue issued a ruling that greatly liberalized the treatment of academic research.[6] In connection with this ruling the Commissioner and the Department of Justice withdrew their opposition, in a case pending in the Court of Appeals, to a deduction claimed by Professor Harold H.

[6] Revenue Ruling 63-275, *Internal Revenue Bul., Cumulative Bul.*, 1963-2, 85; see also Wolfman, *op. cit.*

Davis, of Pomona College, for travel expenses incurred to visit England in order to carry out research for a monograph entitled, *A Critical Census of Translators and Translations into English from 1475 to 1640*. Professor Davis was not required by the college to do this particular research, and since he held a tenure appointment he could not be discharged for failure to do any research at all. Furthermore, he expected no profit from the sale of his book. He claimed the deduction as an "ordinary and necessary" expense of carrying on his "business," which included the advancement of knowledge of the literature of the English Renaissance.

The facts of the Davis case indicate significant points in the present official attitude toward individual research expenditures. The Internal Revenue Service recognizes that research is a normal part of the activity of professors, regardless of whether they have any specific contractual obligation to do research. There is no distinction in this respect between professors with tenure appointments and other faculty members. Deductions may be obtained for expenditures for research in the field of a person's special competence regardless of whether there is a direct relation to earning capacity or a profit-seeking intention. Most expenditures for eligible research may be currently deducted, but outlays for equipment, books, and other items that last more than one year may have to be capitalized and written off through depreciation allowances.

Presumably, other professional persons could claim deductions similar to those allowed faculty members if they could prove that it is customary for them to carry on individual research as part of their work. But this would be possible only for persons who are established in a profession or position. Research expenditures made to help one enter a profession or get an appointment appear to be subject to the same rule as educational expenditures and hence to be neither currently deductible nor amortizable. If the law were revised to allow amortization of the cost of basic professional and vocational education, the same treatment should in strict theory be extended to research expenditures that help one qualify for a profession or a particular job. This might not be practical, however, because of the difficulty of verifying the purpose of individual research.

If I may inject a hortatory note, I should like to caution economists against the temptation to abuse the discretion that they now enjoy regarding research expenditures. The danger is not so much that the deductions claimed will be lavish in amount as that doubtful items will be classified as research expenditures. This is especially true of travel. One should be prepared to prove that the travel was indeed necessary for the research. If too many deductions are claimed for field work on

the Riviera or the consultation of general libraries in Western Europe, stricter controls may be applied by the Treasury Department or Congress. However, a liberal ruling issued in 1964 allows professors on sabbatical leave to deduct expenditures for travel directly related to their teaching duties, even though no research is done.[7]

Surprising as it may seem, research associated with the preparation of a textbook or other writing that promises to yield royalties may be less favorably treated than scholarly research offering no reasonable expectation of gain. Expenditures related to the income-yielding asset may have to be capitalized rather than currently deducted. Application of this requirement would seem to be a discrimination against a particular kind of research. Although the Internal Revenue Code gives a taxpayer the option of currently deducting "research or experimental expenditures . . . incurred . . . in connection with his trade or business" or amortizing them over sixty months (sec. 174), the official regulations hold that this provision is not applicable to "literary, historical, or similar projects."[8]

Research and experimental expenditures made in an effort to discover new products or processes may be currently deducted only if the activities are directly connected with the individual's trade or business. Although a person can have more than one trade or business, not every activity engaged in for profit constitutes a trade or business, and a part-time inventor is likely to have difficulty in qualifying for the current deduction of research and experimental expenditures.[9] He can capitalize his outlays and, if successful, can amortize them over the useful life of his product or patent. If unsuccessful, he may obtain a loss deduction when he abandons the project, but can do so only if he can prove that the project was a serious venture for gain rather than an avocation, which may be hard to do. While these restrictions discriminate against would-be inventors, they seem justifiable as a safeguard against the deduction of personal expenses associated with puttering and hobbies rather than the production of income.

Conclusion

Generally, individual expenditures for education and research that are primarily for the maintenance of earning capacity, or the performance of the customary duties of one's position, now receive favorable treatment under the U.S. income tax. Subject to limitations that on the whole seem reasonable, current deductions are permitted for these expenditures, including some items that in strict theory are capital out-

[7] Revenue Ruling 64-176, *Internal Revenue Bul., Cumulative Bul.*, 1964-1, 87.
[8] U.S. Treasury Regulations, 1.174-2(a)(1).
[9] *John F. Koons*, 35 T.C. 1092 (1961).

lays. There is some discrimination against those who seek income by doing individual research as a sideline, but it would be hard to eliminate this without opening the way for tax avoidance.

Educational and research expenditures that are intended to create earning capacity, by qualifying for an occupation or position, usually cannot be offset against taxable income because the concept of human capital has not been recognized in the income tax. The possibility of introducing this concept into the Internal Revenue Code by allowing the amortization of certain educational expenditures merits further attention.

THE TAX TREATMENT OF RESEARCH AND INNOVATIVE INVESTMENT

By RICHARD E. SLITOR*
United States Treasury Department

Introduction

The sources of technical progress are threefold: "learning by doing," autonomous technological change, and research and innovative activity. The last is the most important in the modern economy. The production of new industrial technology and its introduction into the market place are, by and large, economic activities. Innovative output, like any other, depends on the inputs that go into making it and the relevant production functions. The tax system impinges in various ways on the costs and rewards which influence this important phase of business activity and decision making.

There is widespread belief that civilian technology can be greatly improved by increased federal support of research and development for civilian needs. In the words of a congressional observer:

> Though many innovations which are needed to improve the quality of our lives have been made by industrial research, in many areas of need privately-financed research has not and cannot be expected to be forthcoming. . . . The development of new technology may require too much capital investment or involve too great a risk of failure to stimulate private investment; the existing industry may be fragmented in small production units unable to generate the capital needed for a research and development program; or the new technology, while socially useful, may not be economically saleable at present.[1]

Government has many means to promote civilian technology, through loans and grants to research institutes, encouragement of scientific education, modification of its own practices as a purchaser of innovative output, and other programs. The use of tax stimulus devices is often advanced as an alternative or supplementary avenue. The world being as tax, innovation, and productivity conscious as it is, tax policy-makers will be called upon increasingly in coming years to make important decisions relating to the extent of structural use of the tax system to stimulate, modify, and redirect particular forms of economic activity. Industrial research is only one of a number of claimants for

* The author wishes to express thanks to the many individuals with whom he has had occasion to discuss the subject of this paper, including Mr. Gerard M. Brannon, Miss Anita Wells, Mr. Martin Cohen, Mr. William L. Hooper, and Professor Edwin S. Mills.

[1] See release of November 30, 1965 by the Research and Technical Programs Subcommittee of the House Committee on Government Operations, containing a statement by Representative Henry S. Reuss, Subcommittee Chairman.

such assistance. Job training, regional economic development, urban renewal, air and water pollution control, conservation, and education are examples of others. The competing demands on our economy and the reduced margin for error under conditions of high employment call for increasing emphasis on the application of modern program analysis to the use of tax monies, directly or indirectly, to achieve desired objectives.

Range of Tax Provisions Affecting Research and Innovation

Provisions affecting research, invention, and innovative investment in some pertinent sense cover a wide range of sections of the Internal Revenue Code. These include capital gain treatment on patent sales by individual inventors and their "angels"; ordinary loss deductions on small business stock; business loss carryovers; "conduit" treatment for investment companies furnishing capital to development corporations; special rules for small business investment companies and their shareholders; Subchapter S partnership treatment for closely held companies; income averaging; deduction of expenses and losses of "amateur" inventors; the tax exemption for scientific organizations; the charitable contribution deduction for donations to such institutions; and many others.[2]

Research and Experimental Expenditures

In view of time and space constraints, this paper concentrates on the tax status of research and experimental expenditures. It does not attempt to delve into the basic economics of innovation and technical change, as exemplified by the "embodiment" controversy;[3] questions of optimality in the rate of technological progress; or the relationship between business size, market power, diversification on the one hand and effective research and innovational activity on the other.[4]

While these are topics which bear on the subject of this paper, of more immediate relevance are the much discussed questions of the sources of invention and innovation and the role of the small-scale independent. In this area, the viewpoint underlying this discussion may

[2] All investment is in some sense innovative. The speed of the process of replacement and indeed the rate of growth of capital affect the average age of capital facilities, the lag in the shaping of facilities to the current technology, and the speed with which technical advance is embodied in capital stock. For this reason, the investment credit provisions, the depreciation guideline reforms, and the tax rate structure itself all have a bearing on the speed of technical advance.

[3] In simplest terms, this involves the differing estimates of the effect on economic growth and productivity of alternative investment rates due to divergent appraisals of the impact of the embodiment of new techniques in capital goods. See E. F. Denison, "The Unimportance of the Embodied Question," *A.E.R.*, Mar., 1964, pp. 90-94.

[4] See *Economic Concentration*, Hearings before the Subcommittee on Anti-trust and Monopoly of the Committee on the Judiciary, U.S. Senate, 89th Cong., 1st sess., Part 3.

be stated very briefly. The independent is an important originator and propagator of new ideas; numerous recent episodes in the history of technological innovation attest to this.[5] As such, he should be encouraged, although his motivation is not primarily pecuniary. However, the entire process of technical change, comprising development and commercial introduction into the market place, for which the tax treatment of R and D is most significant, is now largely a matter of corporate-scientific, institutionalized teamwork.

Prior to 1954, the tax law made no specific provision for the research and experimental expenditures of business. As a practical matter, large businesses with continuing research and experimental budgets were able to deduct most of these expenses currently. However, in the case of many small businesses, unable to afford a regular budget for research, doubt existed concerning the deductibility of such expenditures. Moreover, when they were capitalized, there was no assurance that they could be amortized over a definite period or that an abandonment loss could be readily established.[6]

The provisions of section 174 of the Internal Revenue Code adopted in 1954 give all business taxpayers the option to deduct such expenses currently or to capitalize them and write them off over a period of not less than five years.

The option to expense does not apply, however, to expenditures for land or depreciable property used for research. The cost of research equipment is recovered through depreciation allowances. Depreciation is liberal under the new guideline system,[7] and research equipment qualifies for the 7 percent investment credit.

"Research and experimental expenditures" for this purpose means, by regulation, expenditures incurred in connection with the taxpayer's trade or business which represent research in the experimental or laboratory sense.[8] The term includes generally all such costs incident to the development of an experimental or pilot model, a plant process, a product, a formula, an invention, or similar property, and the improve-

[5] See, for example, testimony of Daniel V. De Simone, Director, Office of Invention and Innovation, National Bureau of Standards, U.S. Dept. of Com., *ibid.*, pp. 1093-1118.

[6] See *Annual Report of the Secretary of the Treasury*, 1954, Taxation Developments, p. 50.

[7] The guideline revision of 1962, liberalized with respect to the transition and reserve ratio test rules earlier this year, shortened guideline lives for machinery and equipment on the average about 40 percent below the old *Bulletin "F"* and roughly 20 percent below prior practice.

[8] Federal Income Tax Regulations 1.174-2. Costs of obtaining a patent, such as attorneys' fees, are included in the definition of research and experimental expenditures. On the other hand, the costs of purchased technology, represented by another's patent, model, or process are not included. The latter are treated as capital outlays to be amortized over the useful life of the asset acquired, if that is ascertainable. Specifically excluded are expenditures for literary, historical, or similar projects, which, if deductible, are handled under other provisions of the law.

ment of an already existing property of the type mentioned. It excludes expenditures such as those for the ordinary testing or inspection of materials or products for quality control or for efficiency surveys, management studies, consumer surveys, advertising or promotion.

The section 174 provisions apply not only to research expenditures incurred directly by the taxpayer but also those paid or incurred by him for research done on his behalf by another person, research organization, or institute. For the research contractor, however, research costs incurred in performance of a research contract are merely ordinary business expenses.

The existing federal income tax treatment of research and experimental expenditures of industry is thus highly favorable in several respects: (1) Those expenditures which are in effect successful investments create capital assets. As a result of the current expensing, the cost of these investments is shared by the government through the tax deduction system in the same proportion as the income from these assets—the fruits of successful research—are taxable. This, of course, is the standard formula for virtual neutralization of the income tax on investments. (2) The option to expense currently or defer is advantageous to the new or struggling firm. (3) Unlike many ordinary costs, R and D expenditures are excluded from the valuation of inventory, so that their deduction is not deferred through capitalization in goods in process.

Foreign Experience

Canadian Approach. Canadian tax law since 1961 has permitted a 100 percent deduction for capital and current expenditures for scientific research. This was amended in 1962 to permit corporate taxpayers to deduct an additional 50 percent with respect to an increase in expenditures over a specified base period. Under a 50 percent corporate tax rate this was equivalent to a 25 percent tax credit on the incremental outlays.

The Canadian plan was designed to reduce the revenue cost of tax stimuli to research by concentrating the bonus allowance on increases, cutting down on the waste of extra deductions for research that would have been carried out anyway. Early experience seemed to indicate the plan was only partially effective in stimulating additional R and D.[9]

[9] In an address before the Conference on Canadian Industrial Research, Carleton Univ., Ottawa, Sept. 2, 1964, the Minister of Industry, C. M. Drury, stated that although statistical data were not yet available, preliminary indications were that the 1962 provision had induced a 10 percent rise in industrial R and D activity. At the same time, he specifically noted the inherent limitations on the tax incentive device in solving deficiencies in the R and D effort because (1) tax remission does not benefit the young or rapidly expanding firm not yet in a profit-making position, (2) it does not help the small specialty firms (where many of the brightest ideas originate) due to the relatively

In the April, 1965, Budget Message of the Finance Minister, it is indicated that—optionally effective in 1966 and fully effective in 1967—the fixed base is to be converted into a moving average of three years, but the incremental feature of the plan is limited to "current" expenditures. All capital research expenditures qualify for the tax bonus. In lieu of an additional 50 percent deduction, the bonus takes the form of an optional 25 percent tax credit or cash grant, making it equally available to the nontaxable firm with losses. The administration of the plan (with the apparent exception of automatic application to R and D expenditures under $50,000 a year) is switched from the Department of National Revenue to the Department of Industry, in recognition of the special administrative requirements of such selective subsidies and the direct conflict between principles of uniform tax administration and selective financial incentives.[10]

The shift from a fixed base to a moving average reflected dissatisfaction with the previous penalties against firms which had performed well and rewards for those which had a low research performance in the base period.

Even more strictly than before, the new tax incentives are limited to research activity performed in Canada. Canada is confronted with special national problems in the R and D area because of its heavy reliance on "imported technology" and the drain on its potential research talent through migration to the United States. Its policy is tailored to special national objectives of developing local research capability and reducing dependence on the United States.

United Kingdom Provisions. British law permits full current write-off of research expenditures related to the taxpayer's trade, including ordinary expenses, payments to approved scientific research associations, universities, and similar research organizations related to the taxpayer's trade, as well as capital expenditures for laboratories, pilot plants, or other equipment, made since November 5, 1962.

In addition an investment allowance of 30 percent (over and above 100 percent of cost) is allowed for capital expenditures since November 5, 1962, on buildings, works, and new plant and machinery.

The Inland Revenue consults with various government research councils in the administration of these provisions.

Since 30 percent investment allowances plus 10 percent initial allow-

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large volume of business required to support even a minimal R and D effort, and (3) to achieve progress in the more speculative areas of technology where risk exceeds normal business practice, a "rifle" approach in the form of direct financial assistance is likely to be more effective than the "shotgun" method of tax incentives.

[10] For a review of the new Canadian treatment, see William R. Latimer, "Scientific Research Expenditures," *Corporate Management Tax Conference,* 1965 (Canadian Tax Found.), pp. 3-9.

ances are available generally on industrial plant and equipment, the differential stimulus to research per se under the British rules is confined to the excess of the current write-off over the normal depreciation allowable on the 90 percent of cost remaining after the generally applicable 10 percent initial allowance.

The level of R and D in Britain approaches that of the United States in terms of the research ratio (ratio of R and D to GNP). In spite of favorable tax allowances, however, the British government finances over 60 percent of the national R and D effort—nearly as high a proportion as in the United States.[11] Whatever the results of their research effort, as such, the British are not satisfied with the rate of infusion of results into the market place or the pace of innovative investment, modernization, and automation in their basic manufacturing industries. What is needed seems to be something—perhaps outside the tax field—to encourage the big, bold, new step in industrial automation and modernization. (Since this paper was presented, the British government has announced a general changeover of the investment allowance position of their incentive scheme to a system of direct investment grants applicable to scientific assets and plant and machinery investments generally.)

The United States R and D Performance. At current levels total R and D expenditures in the United States are estimated at roughly $20 billion or 3 percent of the GNP. Of this total, about $7 billion is for research ($2 billion basic, $5 billion applied) and $13 billion for development.[12]

Business-financed expenditures for R and D, now about $6 billion a year, have doubled during the past decade.

Government provides about $13 billion or 65 percent of the 1965 total of R and D funds, primarily for defense, atomic energy, and space programs. Government-financed R and D have nearly quadrupled over the past ten years.

Industry pays for about $500 million or a quarter of the total basic research, and the faster rate of growth for basic research compared with the R and D total, particularly in the past five years or so, seems to be due to support by the federal government, the universities, and other nonprofit institutions.[13]

[11] See international comparisons in *Basic Research and National Goals,* A Report to the Committee on Science and Astronautics, U.S. House of Representatives, by the National Academy of Science, Appendix B, pp. 325-334.

[12] My own round-number estimates based on data from *Reviews of Data on Science Resources,* National Science Foundation, Washington, D.C., Vol. I, No. 4 (May, 1965).

[13] See *Reviews of Data on Science Resources, op. cit.,* pp. 4 and 8.

Spin-off Effects

While the government effort dominates the R and D total, there is some spillover of government research benefits into "adjacent" civilian industries and a very high social pay-off from government-sponsored research in the field of health. This "spin-off" or "fallout" effect has been deprecated by some because of its exiguous impact, variability and selectivity among industries. Specific gains for the civilian economy, such as submarine diesels for railroads or solar cells for portable radios, are said to be tiny compared with the sums spent.

On the other hand, the indirect spin-off effect of government-sponsored research in speeding up the whole pace of technology is sometimes given high rating. Planning techniques developed by military research and systems analysis are only beginning to be utilized in industrial management.

Striking advances in research-active or research-benefited industries help prompt the idea that rich pay dirt awaits direct encouragement to greater R and D effort by other nonresearch-oriented industries.

Is the Record Satisfactory?

Focusing on the industrial R and D—the $6 billion supported by industry's own funds for its own commercial purposes—some competent authorities maintain that industrial R and D in the United States today, on balance and in most fields, has a worldwide advantage and can maintain a front rank position with reasonable wisdom on the part of industry and government.[14]

Others feel that the R and D contribution to technological advance should be speeded up or diffused more rapidly in one direction or another.

Criticism of existing mechanisms focuses variously on: (1) the ability to sustain basic research by industry, (2) the need for more effective applied research and development and more dynamic business attitudes toward exploiting known principles, and (3) the concentration of research on military objectives and the "starvation" of sectors of civilian industry which do not enjoy substantial spillover benefits.

Rationale for Tax Subsidization Plans

The basic case for tax credits for R and D follows familiar lines: (1) technical advance has accounted for a large fraction of growth and

[14] "Research, Development, and World Competition," a speech by Robert L. Hershey, Vice President, E. I. du Pont de Nemours and Company before the National Security Industrial Association Symposium, Nov. 4, 1965.

productivity gains in recent decades; (2) the private rate of return on R and D is apparently high[15] and the social rate of return is still higher, indicating underallocation of resources to this activity; (3) the disparity between social and private product is due in large part to imperfect appropriability of results, "externalities,"[16] lack of exclusive rights, and inability of the creator to patent, maintain secrecy, or corral the spillover of benefits to other firms and industries; yet strengthening of property rights in innovations would not be an optimal solution since the social costs of additional utilization of existing technology are negligible; (4) the market mechanism in the R and D area is further weakened by risk and uncertainty which discourage even sizable firms from staking large amounts on promising projects; (5) economies of scale and the need for pooling risk may prohibit effective research for many small- or medium-sized firms below a critical size and blunt the competitive spur among the relatively few large firms above the critical size classification; a laissez faire approach entails the danger of generating R and D monopolies; (6) so-called "grey areas" exist which fall between the public and private sectors of the economy and offer limited appeal to the profit motive; these include air and water pollution control, urban transportation and renewal, highway safety, educational technology, and public health; while direct government assistance is a prime solution, each area needs to be approached pragmatically in terms of a variety of possible techniques, including tax devices.

Some Specific Proposals

1. *Curtis Bill, H. R. 3791 (89th Congress).* The Curtis bill would allow a 75 percent tax credit (up to 3 percent of tax) in lieu of a deduction (which amounts to a 48 percent tax credit under the present corporate tax rate) for expenditures on basic research, if certified by a board of scientists appointed by the President on recommendation of the National Science Foundation. It would also allow an optional tax credit of 90 percent (up to 4 percent of tax) in lieu of the existing deduction for charitable contributions to a scientific or educational institution to be spent exclusively for basic research in science. A specific publicity requirement is attached to the results of the in-house expenditures.

[15] See Edwin Mansfield, "Rates of Return from Industrial Research and Development," *A.E.R.* May, 1965, pp. 310-32.

[16] For an enlightening review and clarification of "externality" concepts, see James M. Buchanan and William Craig Stubblebine, "Externality," *Economica*, Nov., 1962, pp. 371-84. For an early statement of the economics and policy implications of the "external-economy" aspects of basic research, see Richard R. Nelson, "The Simple Economics of Basic Scientific Research," *J.P.E.*, June, 1959, pp. 297-306.

Recent justifications of the Curtis proposal have come up with a new, somewhat puzzling rationale, not clearly related to its provisions. They stress somewhat divergent objectives—presumably with the 90 percent credit for research contributions in mind: (*a*) to help little science (the individual investigator who often makes great breakthroughs); (*b*) encourage the interdisciplinary team technique characteristic of operations research; and (*c*) create financial support for venturesome research projects which could not meet the standards necessary to justify federal government grants.[17]

2. *Expensing of Research Equipment Outlays.* Current expensing for tax purposes of business capital expenditures on machinery and equipment used in research and development was recommended by President Kennedy in his 1963 Tax Message to Congress.

The 1963 proposal aroused little enthusiasm in the business community; it was not included in the bill reported to the House, nor was it considered by the Senate.[18]

3. *Tax Credit to Stimulate Industrial Support of Academic Institutions.* A special tax incentive proposal urged by some experts would provide a tax credit of, say, 75 percent applicable to funds spent by businesses under research agreements with universities. This approach is designed both to improve the development of research manpower and capabilities of the universities in the civilian industry sector and to shift the balance between civilian industry and space or defense mission-oriented research, thus producing important innovational benefits for industry as a whole. A related objective is to help small firms with limited in-house technical resources to support industrial research of interest to them.

To qualify, the research activity would be under the control of the university and would conform to criteria already established by universities for acceptance and administration of government research contracts and grants. The credit would not apply to arrangements assigning proprietary rights to the sponsoring business. The substance of the proposal in a sense is therefore to grant a special tax reward for industrial research on condition that the findings be in the public domain.

[17] Strangely enough, referring to a government which mounts an expedition to the moon and explores outer space as well as the oceans and subsurface of the earth, one advocate of the Curtis bill has alleged: "The Federal Government will support a Lewis and Clark expedition but not a Columbus. It is increasingly frustrating to be told, 'Bring back a tobacco leaf and an Indian and then, and only then, will we finance the Niña, Pinta, and Santa Maria.'" *Cong. Record,* House, May 11, 1965, p. 9837.

[18] Liberalized guideline depreciation, liberalization of the investment credit through removal of the basis adjustment rule in 1964, and tax rate reduction in the interim have tended to reduce the practical significance of the 1963 expensing proposal.

4. *General Tax Credit for All Industrial R and D.* Another approach argues that, since the distinction between basic and applied research is essentially arbitrary and basic research is largely outside the market arena anyway, tax subsidization via a tax credit should apply equally to all industrial R and D.

Are Tax Stimulants for R and D Desirable?

Perhaps, having stated the problem and its institutional setting, my discussion should end at this point. Without undertaking a systematic review of the pros and cons of tax stimulants for R and D, I would nevertheless like to highlight some of the relevant considerations. First, a few specifics.

1. *Basic Research and the Market Mechanism.* The greatest social pay-off to an effective stimulus would probably be in the area towards the basic end of the research spectrum where market mechanisms are least satisfactory. However, it is doubtful that a large number of businesses would respond to even a substantial tax stimulus by diverting their best people to an area where results are likely to be non-proprietary.

2. *Definitional Problems.* Definitional problems are especially difficult with respect to basic research but they permeate the research field. The full scope of the practical and conceptual problems which would arise under a special credit is not disclosed by the experience under the section 174 option which offers little or no inducement to reclassify an ordinary expense as research. Tax stimulants would run the risk of qualifying all sorts of outlays not remotely resembling what the amiable tax dirigist intended, including some already financed directly or indirectly by government funds.

Careful definition and real pinpointing of benefits requires administrators who are able to distinguish research when they see it. This may call for talents other than those of a revenue agent—a consideration which highlights the difficulty in calling upon the tax system to carry out a sophisticated program to attain research supply and allocation objectives.

3. *Revenue Costs versus Results.* Suppose we shirked the problems of defining basic research, and allowed a general tax credit on industrial R and D. With company-financed R and D about $6 billion annually, a 10 percent credit would lose $600 million revenue and a 25 percent credit $1.5 billion before even an additional dollar of research emerged. A 10 percent credit would have to increase the R and D effort by $667 million or about 11 percent in order to match each dollar of revenue cost with an additional dollar of R and D. A 25 percent

credit would require a $2 billion or 33⅓ percent increase in R and D to produce a dollar-for-dollar effect.

The chances that the marginal inducement of the credit plus the cash flow effects would produce additional R and D of an amount equal to the revenue concession seem slim. In the case of a general investment credit, the cash flow effects of such a concession are in themselves helpful towards achieving higher investment levels. In the case of a special form of investment such as R and D, the cash flow effect is likely to be scattered and dissipated through leakages or uses unrelated to the special goal.

Ignorance and uncertainty about the investment function generally are impressive, but with respect to the R and D function they are even more imposing. Until we know more about the elasticities involved, with respect to either reallocation of existing resources or response of the basic factor supply, we can only furnish answers by intuition, hunch, and prejudice. Economic skills and econometric techniques need to be sharpened and focused to give operational answers in these areas.

4. *Concentration of Benefits.* The redistribution of the tax burden under such a credit would follow a predictable pattern. Tax reductions would be in proportion to the R and D effort. Major reductions would go to those industries and companies (typically large multiproduct businesses, already research-oriented) in which R and D is normally a major factor in operations.

The creative little fellow—the dedicated scrounger—the independent, low-cost, garage-based innovator of proverbial fame—obstinate and undiscouraged though he may be—would receive little or none of this largesse except as he benefited indirectly through a high-bracket "angel."

5. *Unevenness of Gap Between Private and Social Marginal Product of R and D.* The disparity between private and social net gain from R and D due to inappropriability and externalities will vary from situation to situation and over time. A fixed tax credit, even of generous proportion, will fail to close the gap if only a small fraction of the value of research can be captured by the individual entrepreneur. Where external economies dominate, only something approaching a 100 percent credit would be effective. In other cases, even a 10 percent credit may be a windfall.

Some General Observations

Tax subsidization plans for R and D pose four basic questions:

1. Does the existing tax environment impose artificial barriers to research, invention, and novel forms of investment?

2. In light of the generally successful record of the free enterprise system, how serious are the defects in the market mechanism for controlling the quantity and allocation of R and D resources and how amenable are they to corrective or compensating adjustments in the tax system?

3. Are tax devices more efficient than alternative measures such as direct expenditures and other forms of assistance?

4. How grave is the clash between R and D tax incentives and the integrity and workability of the tax structure; and does any case which can be developed for the tax incentive override the basic requirements of equity and administrative economy in the revenue system?

The case for tax concessions for specific activities is generally argued in terms of the merit and urgency of the purpose, which may or may not correctly identify the true basic objective. The question of the use of the tax device is, however, essentially one of means or approaches.

Assuming a basic need for government intervention, the general case for the tax approach as against the direct assistance approach is that it avoids red tape, the waste motion of collecting taxes and disbursing them back to beneficiaries, the bureaucracy of subsidy administrators or worse; that it provides a speedy, semiautomatic access to public funds on a matching basis which depends on the credit or deduction technique used; that it relies primarily on the decentralized decision making of the businessman within the framework of the tax system.

What is often overlooked is that the tax device does not offer a free and easy escape from the practical burdens of the subsidy process: "it substitutes tax administrators, tax planners and a tradition of extended administrative controversy and litigation."[19] Where certification by a special nontax authority is a condition for special tax treatment, the administrative saving via the tax route seems negligible. Anything the tax device gains is at the cost of introducing conspicuous tax differentials which, however consistent with an economic subsidy objective, are difficult to reconcile with the principle of uniformity in the sharing of tax burdens. Tax concessions tend to outlive the specific conditions which called them into being, to become permanent, and to expand.

The contribution of research is beyond question. Yet the merits of further stimulation by tax or other means is far from clear. At one extreme, there is considerable disenchantment over the commercial payoff of the enormous organized R and D effort, even after allowing for

[19] See Bernard Wolfman, "Federal Tax Policy and the Support of Science," *Univ. of Pennsylvania Law Rev.*, Dec., 1965.

the time lag with which it yields its benefits. The mystique of the research center and the computer has its skeptics, who regard much of this outlay as a prestige or corporate-image item or at best as an insurance premium. At the other extreme, one hears of a pending speed-up in the pace of innovation, organizational change, and automation in many industries which it would seem unnecessary if not unwise to accelerate artificially.

Nontechnical Innovation

Innovation into the market place is, after all, the ultimate objective. Preoccupation with technical research as an object of public policy may neglect various other essential aspects of the innovative process, including:

1. Costs of commercialization of new products. These include such important items as design engineering, tool manufacturing engineering, manufacturing start-up expenses, and marketing start-up cost which in many cases apparently account for the bulk of the innovation costs.
2. Marketing and organizational innovations. New forms of merchandising have affected many service lines ranging from hamburgers to insurance. These new ideas have originated outside technical laboratories.
3. Aesthetic innovations. Matisse and others of the modern French school have revolutionized commercial design; yet their contributions are nontechnical and virtually autonomous from the economic standpoint.
4. R and D managerial skills. The difference between success and failure is often not the merit of the research project itself or the availability of R and D funds but the shortage of creative R and D management.

Cost Effectiveness Studies

Tax solutions are sometimes adopted under the illusion that because tax rebates, unlike expenditures, do not show up to be counted in the budgetary process, they impose no perceptible burden on society or the taxpayers as a whole. This illusion has probably been encouraged by long habits of thinking in terms of a less than full employment situation, in which additional use of R and D resources may be a kind of free good from the social standpoint. In the context of a full employment situation, however, a tax stimulus like any other involves diversion of resources from other uses and the possibility of inflationary spinning of wheels with no net increase in the social aggregate of R and D effort.

Intelligent appraisal of proposed tax concessions as of expenditures requires a systematic comparative evaluation of need, cost, benefit, and alternatives. The babel of expert opinion on our R and D posture and how to improve it underscores the need for more exacting analysis.

The Budget Bureau, at the direction of President Johnson, is now in the process of applying to nondefense expenditures and programs the cost-effectiveness techniques which have been successfully used in the Defense Department.

As Assistant Secretary Surrey of the Treasury has recently indicated: "Taxes foregone because of a desire to benefit a particular activity or to induce certain activities are in a real sense, monies spent."[20] Consistent with the new emphasis on modern methods of systematic program analysis in federal budgeting,[21] Mr. Surrey has suggested that cost-effectiveness studies be applied equally to the analysis of the efficacy of the tax approach as compared with the direct expenditures approach to achieve desired objectives.

The cost-effectiveness or systems analysis approach does not imply that decisions on tax policy can be routinely computerized or that value judgments can be excluded. It does indicate that the tax policymaker will increasingly require the guidance of systematic comparative analysis of alternatives, in terms of both cost and achievement; the explicit formulation of premises and the quantification and documentation of conclusions.

Consistent with the credo of modern science generally, this means being hardnosed about the world of tax changes. It means being insistent on the need to maximize the use of figures and to minimize reliance on "feel" and intuition. It means being prepared to use all modern techniques of calculation and simulation to obtain needed insight into the operation of the tax laws.

Conclusion

This paper avoids taking doctrinaire positions either on the need for additional government action to strengthen the national R and D effort or on the comparative merit of alternative modes of action. It does urge that policy formation in this area needs to rely on the most modern and sophisticated techniques, that it must resist both routine negativism and the uncritical advocacy of tax incentives which often rationalizes drives to minimize taxes by indirection.

[20] Remarks by the Honorable Stanley S. Surrey, Assistant Secretary of the Treasury, at the Financial Analysts Federation Conference, Washington, D.C., Oct. 5, 1965.

[21] See, for example, *Program Budgeting, Program Analysis and the Federal Budget*, David Novick, ed. (RAND Corp.-sponsored Research Study, 1965); and Charles L. Schultze, "Needed: New Approach to Expenditure Evaluation," *Tax Rev.*, Aug., 1965, pp. 31-34.

Particularly in a full employment economy with heavy defense costs, the burden of proof is on those who would insert new well-intentioned gimmicks into the revenue system. It may well be that military and space research is a costly and inefficient way of pushing technological advance for the civilian economy;[23] but rather than making ready resort to tax gadgets which may be a still less efficient way of giving direct impetus to lagging parts of the civilian R and D sector, our main tasks should be: (1) to search out the areas in our economy where adequate research is lacking and where government grants, pilot research projects, and foundation activity may help effectively and (2) to encourage business schools, business organizations, and other interested groups to discuss and explore continually the research area so as to acquaint business with the essential problems and opportunities in this field, give it additional insight into the nature of the public policy issues involved, and assist it in making an intelligent allocation of its innovative dollars.

Looming on the horizon are basic developments in the physical and life sciences, in the fields of health, high-speed transportation, both surface and air, environment control, and many others which will almost certainly result in heavy pressure for direct government support Under the circumstances, society may in all prudence wish to keep its financial powder dry to meet these demands as they arise.

[23] This is reported to be a key conclusion of an unpublished study by experts of the Organization for Economic Cooperation and Development. See feature article entitled "OECD Sees Space Study Value Limit," by Bernard D. Nossiter, *Washington Post*, Dec. 2, 1965, p. A24.

THE EFFICIENT ACHIEVEMENT OF RAPID TECHNOLOGICAL PROGRESS: A MAJOR NEW PROBLEM IN PUBLIC FINANCE

By Richard R. Nelson
The RAND Corporation

Introduction

In contrast with the pre-World War II years when the federal government spent very little on research and development, in 1965 the federal government will spend approximately $16 billion on R and D —over 20 percent of total federal spending on goods and services.[1] While a small proportion (such as the NSF budget) will be spent on research with no clear-cut technological objective, the lion's share is aimed, directly or indirectly, at advancing the technology of a particular product field or industry: military aircraft, rocket systems, nuclear devices, medicine, agriculture. Furthermore, particularly in defense and space R and D, but also in a few other areas, the objective is a pace of technological progress seldom achieved in purely private sector product fields. While we may search in vain through the economic textbooks on public finance for an appreciation of the fact, the efficient achievement of rapid technological advance now is a major problem of public finance.[2]

Success in achieving this objective depends on how well the characteristics of the underlying process—the outputs, inputs, and their relationships—are understood. Understanding of these characteristics determines the decision-maker's grasp of the range of and limits on the meaningful strategies and actions open to him, and his ability to perceive the implications of choosing one or another. While in broad principle all constrained maximization problems have the same abstract form, the nature of the efficient strategies and relevant subdecisions, and the criterion which should be used to choose among alternative courses of action can differ sharply from case to case.

When dealing with allocation and decision problems regarding a process with which they are unfamiliar, economists rather naturally tend to carry over a view of the decision problems from the area with which they are most familiar—production. Economic research on the

[1] National Science Foundation, Federal Funds for Science and Other Scientific Articles, Vol. XIII (Government Printing Office, Feb., 1965).

[2] For an analysis of the objectives of the range of government R and D programs see R. R. Nelson, M. J. Peck, and E. M. Kalachek, *Technological Advance, Economic Growth, and Public Policy* (forthcoming).

special characteristics of the R and D process is still in its infancy, and there are still vast areas of ignorance surrounding the process. However, research to date suggests that while R and D and production do have a number of elements in common, there are certain rather special characteristics of the R and D process that lead to consideration of classes of strategies and employment of decision criteria quite different from those of the more conventional breed of production theory.[3]

In this paper I first shall present a way of looking at the R and D process which focuses on certain characteristics which seem to be important when major advances are being sought. The discussion will necessarily be more of a stylized view of certain loosely defined concepts and relationships than a formal model with well-defined variables and equations; formal models of these facets of the R and D process are only beginning to appear. Then I shall present some ideas as to certain characteristics of an efficient R and D policy which appear to be consistent with, if far from strictly derivable from, the qualitative description of the R and D process.[4] The prescription, as the "model," is crying out for more rigorous formulation and testing, but these things are very hard to do.

Characteristics of the R and D Process

A Way to Look at an R and D Program. It is useful to think of an R and D program as comprising a set of projects, the overall program aimed at enhancing the range of products man knows how to produce and the ways he knows to produce them in a particular product field, and the individual projects chosen to complement each other in adding up to the achievement of that objective. Here we will be focusing on R and D programs aimed at creating the capability to produce new products with significant performance advantages over existing ones.

One can think of the output of the R and D program as being in the form of a flow of instructions, each new set of instructions specifying inputs and operations to be performed in them, which, when followed, will result in a new product of specified attributes. The instructions may be thought of as like a cooking recipe (including the steps) or a computer program. They are the operational form of new production functions. The payoff to the program is the net value (discounted over time) of being able to employ these new production functions.

[3] See, for example, R. R. Nelson, "Uncertainty, Prediction, and the Economics of Parallel R and D," *Rev. of Econ. and Statis.*, Nov., 1961, and the papers by B. H. Klein, T. A. Marschak, A. W. Marshall, and W. H. Meekling, and R. R. Nelson in *The Rate and Direction of Inventive Activity* (University's N.B.E.R., Princeton, 1962), Nelson, Peck, and Kalachek, *ibid.*, T. A. Marschak and J. A. Yaher, "The Sequential Selection of Approaches to a Task," *Management Science* (forthcoming), T. K. Glennan, "Issues in the Choice of Development Policies" (RAND Corp., Oct., 1965), p. 3153.

[4] See the references above.

The principal inputs to an R and D program are specialized and trained direct labor (in particular scientists and engineers), technical and managerial support staff, material and equipment used for experimentation and the creation of test articles, and various other capital items (for example, capital, such as libraries, to facilitate information retrieval). The cost of the program is the alternative cost of these resources.

It is convenient to think of these resources as being allocated among three different kinds of projects. The final output of the program is created through "systems" development projects, where the term systems refers to the complexes of interdependent components that make up the final products in the field under consideration. In addition, some resources may be allocated to "component" development projects, aiming at improving a particular component, but stopping short of building that new or improved component into a new or improved system. Finally, some resources may be allocated to research projects aimed at creating new knowledge that will facilitate systems or component development, but stopping short of utilization of that new knowledge. Component development and research produce intermediate products. Their payoff is in terms of reducing the expected cost and time, and the uncertainties, of systems development projects.

It should be noted that the division of resources into the three classes of projects is in large part a question of organization, not the specific content of what is done. A systems development project can, and generally will, involve some work to improve or create new components and some research; a component development project can and generally will involve research. However, as I shall argue later, how the R and D program is divided into classes of projects is a central variable of R and D policy.

The way the three kinds of projects work together in an overall R and D program can be seen by examining the factors that influence expected cost and time, and the initial uncertainty, of a systems development project—the kind of project that creates the final output of the R and D program. Here it would appear that there are two key factors: first, the magnitude of the advance sought over existing products; second, the stock of components that designers can take as operational (or nearly so in that much of the needed R and D already has been done) and the stock of knowledge they can bring to bear on the design problems.[5]

When only a modest advance is sought over existing product systems, this advance generally can be achieved by modifying only a few components and will not involve design problems so new as to be out-

[5] The size and complexity of the system also is an important variable. See Nelson, Peck, and Kalachek. But here we are looking at a particular product field with a particular set of systems.

side the range where strong light is shed by existing experience and formal engineering and scientific knowledge. However, if major advances in performance over existing products are sought, it becomes less likely that changing only a component or two will do the trick and less likely that there will be suitable new components (created for other purposes) which simply can be purchased from catalogues. Thus, in the absence of prior work to create the needed components, there simply will be more components to design. It also is less likely that, in the absence of prior work to create the relevant knowledge, existing knowledge will shed strong light on all the design and development problems. This will increase input requirements in part because more research will be needed integral to the program to create the knowledge necessary to solve certain problems, and in part because, despite the research, almost inevitably certain things will not work out as expected or hoped and a portion of the job will need to be redone, or perhaps done many times, before a satisfactory solution is achieved. Thus costs tend to rise with the magnitude of the advance sought largely because the objective then requires going beyond the capabilities of existing components and knowledge.

It is for this reason that the richness of the available stock of components and the strength and scope of existing knowledge are important variables determining the inputs required to achieve a particular advance. These are the variables that determine the ease or difficulty of moving significantly from the design of existing products. When the catalogue is rich with components that have promising characteristics and the scope of knowledge far transcends the details of existing products, inventor-developers can work with design concepts considerably different from existing products without being forced to create new components and with considerable ability to predict how the designs will work. If the stock of components is sparce and knowledge limited, the costs and uncertainties of any major design change are likely to be great. It is the expansion and enrichment of this stock of knowledge and components that is the contribution of research and component development projects.

Special Characteristics of the R and D Process. In the main line of production theory uncertainty plays only a minor role and much of the limited treatment that exists aims at reducing a situation with uncertainty to an equivalent uncertainty-free situation. Almost no role at all is played by the phenomenon of learning—either through research or through experience. This is reflected in the fact that analysis of the time sequencing of inputs tends to concentrate on the role of the interest rate and not on the role of uncertainty and uncertainty resolution.[6]

[6] There are exceptions. In the management science literature there are several treatments of the role of uncertainty resolution on scheduling decisions. See, for example, C.

Further, in much of production theory it is assumed that one activity affects the output of another only through the application of its principal output as an input. Yet in R and D uncertainty is a key element of the decision-making environment, learning is the essence of the R and D process, and external economies are of paramount importance.

The R and D process involves determining how to do something that could not be done before, or at least not so well. In a far-reaching system or component development project there is bound to be considerable uncertainty at the start. There certainly will be uncertainty regarding the time and cost needed to achieve a given broadly defined final design, and the performance characteristics of that final design also are likely to be subject to considerable uncertainty. In part the cost, time, and performance outcome of a development project will be sensitive to the choice of components and the development approach, and it may be far from clear in advance (or even after the fact) which components and approaches are best. In addition to uncertainties regarding cost and performance outcome, and about the best way to go about development, there may be very great uncertainties regarding the value of the final product, even if performance and cost objectives are met; in part this uncertainty is akin to any demand uncertainty but it is highly accentuated in R and D because a radical new product often has a large part of its payoff in doing things that were not or could not be done before.

The R and D process essentially is the process through which enough of these uncertainties are resolved so that an operational capability is achieved. Much of the work involves research and experimentation to resolve the key technological uncertainties. Naturally, to exploit what is learned in the course of research and experimentation requires that a considerable amount of flexibility be maintained regarding the next round of decisions. Efficient decision making in R and D is inherently sequential, and involves both the acquisition of information and the maintenance of decision flexibility to give the information value.

While much of what needs to be known for a successful development project must be learned in the course of that project, earlier discussion of input-output relationships points out that today's research and development builds on the results of yesterday's research and development. It is not just that today's R and D adds improvements to yesterday's improvements or builds on yesterday's components. In addition,

Holt, F. Modigliani, J. Muth, and H. Simon, *Planning Production, Inventories, and Work Force* (Prentice-Hall, 1960). But there is little in the main-line economic literature, A. C. Hart's work (for example, "Anticipations, Uncertainty, and Dynamic Planning," *J. of Bus.*, Oct., 1940), and some hints by T. C. Koopmans, in his *Three Essays on the State of Economic Science*, being prominent exceptions.

the knowledge created and the design solutions in yesterday's problems often are the key to today's design and development decision problems. A major contribution of today's R and D is the improvement in the ability to make good decisions tomorrow.

Conjectures About Efficient R and D Policy

There are many dimensions to the R and D efficiency problem, many instruments of policies that can be identified, and many ways that the question of alternative "strategies" can be posed. To my knowledge no one yet has posed the R and D efficiency problem in terms of a formal programming model, except superficially. However, the following view of the problem has evolved over the years in the literature cited above.

In the theory of optimal production decision making the two key classes of decision are choosing the quantities of different kinds of output to try to procure and selecting the mix of inputs used to produce given outputs. In the conventional formulation these decisions are made optimally through the maximization of a payoff function (involving both benefits and costs) subject to constraints imposed by resource availabilities and the set of relevant production functions. While the conventional formulation admits a time sequencing of inputs and outputs, for all practical purposes the full set of decisions is made on the basis of the information existing in the initial period. In R and D there are similar classes of decision problems—what R and D projects to undertake and how best to go about undertaking them—and a similar payoff function and set of constraints can be defined. However, the very great uncertainties regarding both payoffs and constraints, the fact that the R and D process inherently involves learning, and the fact that today's R and D contributes to the ability to make better R and D decisions tomorrow, suggest that the essence of good policy is not making detailed decisions regarding future allocations on the basis of present information. This implies that the expected benefits from any possible R and D allocation should be viewed as involving the creation of knowledge facilitating future choice, as well as direct payoff in terms of systems performance. Further, the alternative costs of undertaking an R and D project at any time should be viewed as including consideration that if the same project were initiated later both the uncertainties regarding costs and benefits, and the expected cost and time of achieving a given performance, are likely to be smaller.

The Role of Conventional Benefit-Cost Calculations. Much has been written on the use of benefit-cost calculations—really just simple economic analysis—in guiding government decision making. Clearly

this kind of analysis can, and has, played a major role in guiding R and D decision making.[7] Alternative R and D projects can be compared with each other and with the alternative of staying with existing systems. Where benefits and costs are not highly uncertain (and diffuse), cost-benefit analysis, along the lines of conventional economy theory, can go a long way in guiding choice among alternative projects.

However, conventional cost-benefit analyses have to be modified significantly to shed light on the overall size and composition of the R and D program when the objective is a rapid pace of technological progress in the long run. Here, the contribution of an R and D project to the ability to conduct R and D projects in the future, the fact that R and D projects can be started tomorrow rather than today, and the consequent necessary structuring of the problem is one of sequential decision making, must be placed at center stage in the analyses.

A Central Question: Probing the Technological Frontiers in Systems Development, or in Research and Independent Component Developments. Looking at the R and D program as a whole and over the long run, the central question of R and D policy may be posed: what allocation of R and D resources today will maximize the net value of the expected flow of improved systems over the future (there may be a discount rate involved), recognizing that today's R and D decisions will influence the quality of the knowledge base for the same question when posed tomorrow and that projects that are not initiated today will be open for consideration tomorrow. In part, at least, the operational significance of this formulation of the choice problem involves two related questions. First, the amount of resources that should be allocated to research and component development. Second, the extent to which initiation of systems developments should wait upon the establishment of a strong base of knowledge and components.

A considerable portion of the literature cited earlier is aimed at proposing that it is efficient to allocate a significant share of the R and D budget to research and far-reaching component development projects to provide greater knowledge and a richer stock of components and, except in special circumstances, to limit the reach of systems developments.[8] This belief is based on appraisal of the difficulties that almost always have arisen in systems development efforts that reached far beyond the capabilities of existing components and knowledge and which required major component developments and research programs integral to the effort.[9] Very often unforeseen difficulties resulted in very major redesigns involving not only the component or subdesign

[7] For a discussion of benefit-cost analyses and its role in defense, the classic statement of course is C. J. Hitch and R. N. McKean, *The Economics of Defense in a Nuclear Age* (Harvard Univ. Press, 1960).

[8] See Klein (*ibid.*) and Glennan (*ibid.*).

[9] See Marschak (in the *Rate and Direction*) and Klein (*ibid.*), and Marshall and Meckling (*ibid.*).

problem directly involved, but also other components and aspects of the design which were sensitive to the design of the problem component or part. Sometimes the entire effort had to be abandoned or resulted in a product of dubious value. Undertaking a systems development effort when there still are major uncertainties inherently greatly raises the costs if anything goes wrong. Thus when the sought-for advance is great and there are major uncertainties regarding the details of a good system or even what system should be developed, there clearly are major advantages in delaying systems development decisions until more is known and components are better explored.[9] On the other hand, to delay research and component development until a system has been decided upon is to lose a principal benefit of this work: to guide choices as to what systems to develop and how best to develop them. The cases where the successful development of a new component or a research finding resulted in sharp appraisal regarding what systems were worth developing are legion.

Together these arguments suggest that a strong research and component development effort not tied to particular systems developments may be the key to good systems development choices and to systems development efforts which, to reach a given performance objective, do not need to stretch so far. While under this strategy many component development efforts and many research endeavors will fail, generally large systems will not be tied to their fate.

The Objectives of Research and Independent Component Developments: Facilitating Better Systems Choices and Expanding the Range of Nonrisky Alternatives. That a large share of the total R and D program will be in projects not tied to a particular systems development should not obscure the fact that the payoff from the overall program comes from better systems. To a considerable degree, selection of research and component development projects should be guided by an assessment of what systems are likely to be desired in the future, and the major technological uncertainties which make choices among alternative future systems and configurations presently risky to make. Here, one step removed from the final product, conventional cost-benefit analysis can play a major role by attempting to calculate the contribution to future systems development of particular new components or knowledge. However, experience suggests that present perception of future desired systems is likely to be near-sighted, constrained both by limited vision of demands for and payoffs from different systems, and by overly conservative judgments as to what is likely to be feasible. While in part the portfolio of projects should be selected to facilitate future choices and future developments of presently perceived alternatives, part of the portfolio should aim for expanding the vistas of choice tomorrow.

In either case, the objective of this work can be viewed as that of expanding the set of systems development options which are not subject to extremely great risks. Implicit here is a two-stage notion of the decision-making mechanism. In the first stage, a rough cut is made eliminating a large number of alternatives (say in consideration of dominance by a particular other possible action). In the second stage, a finer, more detailed comparative evaluation is made of the remaining alternatives. It is clear that in systems development, risk, due to technological ignorance, should and does result in a major screening down of the alternatives in the first stage. It does this in part because generally the decision-maker tends to be a risk-averter (although one might argue that the U.S. government or the Department of Defense should optimize over a large enough portfolio of projects to provide insurance). More important, the major reason why technological uncertainty should and does eliminate many projects from consideration is that the expected cost of the research undertaken and errors made in resolving the uncertainty is very great; given these expected learning costs, the project simply is not an attractive gamble at the particular time in question. The conjectures about efficient R and D strategy made in the preceding section can be put this way: in light of the high expected learning costs involved in developing systems which will require significant research and component development work before an operational design is achieved, these projects should generally not be entertained; instead research and component development should be used to try to assure that at any time there are attractive systems development possibilities that can be based largely on existing or relatively certain components and knowledge.

One important corrolary of this view is that the decision to undertake a particular research or component development project need be viewed neither as a once-and-for-all decision, nor based on assessment of chances of success and the expected net return, if successful. Rather, in considerable measure the decision to initiate a project can be viewed as the decision to explore a possibility, aimed at finding out more about the chances of success and the costs and benefits of the project, if it were to be completed. With a large portfolio of projects and avoidance of having future systems developments hinge on the success of any particular one, it may be desirable to run many risky projects—projects which, accounting full development cost, may have a quite low expected return because the chance of success is small but with some chance of very high return—if it is possible for a limited expenditure to improve assessment as to whether the project will prove a success.

The Running of Systems Developments: The Importance of Hedg-

ing. With independent research and component development projects carrying a large share of the burden of the risky part of advancing a technology, systems development can be relatively conservative. With a more rapid rate of advance in knowledge and components, a given rate of advance in systems performance can be accomplished with less resources engaged in systems developments per se, less uncertainty in these projects, and a significantly compressed time span between decision to initiate development and the achievement of an operational system.

However, while a strong program of independent research and component development can reduce greatly the risk and reach in the typical systems development effort, from time to time pressing systems needs will arise that compel a quite ambitious systems development effort, and in almost all cases the objective of achieving major advances will mean that there is a significant residual element of risk surrounding some aspect of the system. Therefore, in systems developments there should be strong concern paid to the possibilities of hedging against best early guesses proving wrong. Hedging can take a wide variety of forms. On the overall systems design side, an attempt can be made to design the system as a whole so that there is not too much sensitivity to variations in particular components. For components or design problems about which there is considerable uncertainty, parallel approaches can be undertaken, or a "back-up" project run at a low level of effort. The overall project can be scheduled so that the elements that both are highly uncertain and on which other design elements are sensitive are undertaken first, and decisions delayed on the elements that are sensitive to the outcome.

The foregoing is a presentation of a broad policy philosophy which appears roughly consistent with the implications of a very qualitative abstract description of the R and D process. It is far from a set of specific operationally defined policy implications derived from a formal and well-tested model. Although certain parts of the analysis can be made quite rigorous and formal,[10] data can be collected and used to test certain hypotheses,[11] and some policy conclusions can be systematically derived for certain special cases, this type of formulation and testing of the overall model and policy philosophy is just beginning.[12] With efficient R and D policy becoming an increasingly important economic objective, this paper is offered in the hopes that it will stimulate members of the profession to join in this much needed work.

[10] See R. R. Nelson, "The Economics of Parallel R and D Projects" (*ibid.*).
[11] Marshall and Meckling (*ibid.*).
[12] T. A. Marschak, B. H. Klein, and T. K. Glennan, *Inside R and D* (forthcoming), will greatly expand the published literature of models and descriptions of the R and D mechanism.

DISCUSSION

DOUGLAS H. ELDRIDGE: In discussing federal income tax treatment of R and D, the underlying question is whether modifications would be desirable as a means of fostering technical progress and stimulating economic growth.

Slitor's knowledge and experience with differential federal tax treatment to serve economic, social, or political purposes is second to none. He has provided a succinct description of current law as it relates to research, development, and innovative activity, and he has hit upon most of the issues. My criticism is chiefly that he has not hit hard enough.

If we take it as the business of economists in the area of government policy to help society understand the choices, benefits, costs, and risk confronting it, I believe we must concede that we have made only meager contributions with respect to tax incentive devices.

More significant advances seem to have been on the expenditure side of the budget where cost-effectiveness analysis has been credited with improving resource use for defense. Here there has been explicit formulation of quantitative objectives and systematic search for least cost solutions. The expenditure side may also afford hope for technical advance in situations where knowledge is mostly qualitative. Nelson has suggested a probing or sequential experimental process by which a limited program can be launched in an area where uncertainties are great, some results and relevant data can be produced, and the experiment then "evaluated by a group who had no connection with the running of the program and no interest in it."[1]

These approaches are much less feasible for tax proposals. With expenditures at least the cost to the government is known, objectives are fairly specific, programs are subject to periodic review and are perhaps reversible. Tax incentive proposals, in contrast, tend to be vague. Indeed the current rationale for tax subsidization as Slitor has summarized it comes to this: technical advance accounts for a large fraction of growth and there are some reasons to believe that more resources should be allocated to R and D.

The motivation for further special treatment in this tax area is worthy. Proponents want to push out the production possibility curve, and more rapid technical advance would help. However, knowledge of how and at what cost we can speed technical advance is rather primitive. Many variables are involved on the input side: the general education and flexibility of the labor force, the number and quality of scientists, engineers and managers, the efforts devoted to basic and applied research, and innovative investment (which as Slitor notes may be taken to be virtually all investment). At what point should there be additional tax incentives for greater inputs with the expecta-

[1] Richard R. Nelson, "Aggregate Production Functions and Economic Growth Policy," Conference on Research in Income and Wealth, N.B.E.R., Oct., 1965 (publication forthcoming).

tion of gaining, at lowest revenue and opportunity costs, the greatest outputs of technical advance? Those who would manipulate the economy through tax devices have differing views. If we add the proposed "Human Investment Act" now in the Senate and recently adopted devices to stimulate physical investment to Slitor's examples of proposals to strengthen academic institutions, basic research and all industrial R and D, we virtually run the gamut. Tax subsidies for everybody could become tax subsidies for nobody. If economic stimulus is desired, rate cuts across the board that would increase after-tax return for all productive activity might well be preferable.

Moreover, the effectiveness of differential tax changes in inducing taxpayers to use more resources for particular inputs depends on demand functions, which are not clearly defined, especially for R and D or for philanthropic contributions to academic and research institutions. The revenue cost of obtaining significant additions of particular inputs depends on supply functions, and the less elastic they are the more of the tax break will be reflected in economic rents. One's confidence about the efficacy of special tax treatment should be conditioned by his knowledge of these functions. Informative economic research is being conducted in these areas, but much more is needed.

Proponents of tax incentives seldom give enough weight to institutional constraints, and Slitor has barely mentioned them. Significant changes in the tax law affect millions of people and often in ways that are not anticipated by academic or government economists. Careful review in the legislative process avoids mistakes as well as giving hearing to competing pressure groups. The congressional tax committees do consider alternatives and attempt to assay costs and benefits, many of which are not easily quantified. While they seek the advice of economists, they can have only a limited degree of confidence in the analyses and estimates that are offered. This is especially true when advice from different economists is contradictory. No planner's scheme is likely to be enacted without significant change. In practice tax devices are broad and therefore blunt, and they tend to get blunter with time. They are not likely to be least cost means of achieving particular ends.

Recent experience has shown a marked difference between academic suggestions for a tax credit to be effective only for marginal investment at limited revenue cost and the broadly based—or a-share-for-all—type of credit that is acceptable to the Congress.[2] Once enacted, further changes should be expected. The investment credit of 1962 was broadened and the revenue loss increased in 1964. Measures currently in the Congress would further increase the availability of the credit in areas restricted in the initial legislation.

Incentives offered by administrative ruling are subject to similar pressures. In 1962 taxpayers were offered shorter depreciable lives for calculating tax liability, provided that within the next few years they brought their actual replacement practices in line with the more rapid depreciation claimed. A reserve-ratio test was designed to help measure performance against claims.

[2] Cf. James Tobin, "Growth Through Taxation," *The New Republic*, July 25, 1960, p. 17; the President's 1961 Tax Message, House Doc. 140, 87th Cong., 1st sess., Apr. 20, 1961; and Revenue Act of 1962, secs. 169 ff.

Business taxpayers evidently approve the more liberal depreciation and disapprove of the test. The Treasury has been forced to give ground in attempting to apply it, and currently it is questionable whether the test can survive. If it does not, taxpayers will have been accorded more liberal allowances while much of the desired inducement to rapid replacement and modernization will have been lost.

One of our longest experiences with a tax incentive has been with depletion allowances in excess of taxpayers' investment costs. This tax inducement was developed during World War I when there was concern about adequacy of domestic mineral supplies. It was designed to encourage the prospectors and wildcatters who actually discovered mineral deposits. Since then the form of the incentive allowance has been modified to percentage depletion and extended in every direction. The special treatment is now available to any taxpayer with an interest in mineral production—domestic or foreign—including royalty recipients, whose tax savings are essentially economic rents. It is available not only to producers of oil and metal ores but also to extractors of stone, sand, and gravel. Where, as is often the case, the allowance frees from income tax half of net income from production, the tax inducement is equally strong for producers of oil, coal, copper, or gravel. For many minerals the calculation of the allowance has come to be based not on income from initial extraction but on income derived after application of several value-adding processes.

This experience can hardly be reassuring to those who advocate tax devices as nicely discriminating measures for governmental diversion of resources. Nor does it suggest that cost effectiveness analysis—the explicit formulation of quantitative objectives and systematic search for least cost solutions—will soon play a dominant role before congressional tax committees.

At least lip service continues to be given the effect of special treatment in undermining tax equity and in increasing complexity of administration and compliance. While these costs are certainly hard to quantify, they deserve attention in evaluating advantages and disadvantages of incentive proposals. Perhaps they are gaining more recognition. There seems to be considerable support for Senator Long's proposal to allow individual taxpayers the chance to ignore all of the inducements to conform their affairs in accordance with special provisions and to compute their tax on a simple tax base at lower rates.

Still there are many who believe not only that they know how the existing market and government allocation of resources can be moved closer to the optimum but also that this can be done effectively and efficiently by manipulation of the federal tax structure. They deserve to be heard, but as Slitor has made clear, they should be expected to carry a heavy and exacting burden of proof.

MARTIN J. BAILEY: Mr. Nelson's paper left the impression that little or no government research is on basic science and components, which is far from true. In the military field, substantial sums are spent on radar technology, inertial guidance systems, propulsion systems, and so on for unspecified or mul-

tiple future aircraft and missile types. How large are these sums and how can we tell whether their share is too small? It is undoubtedly true that in the development of a specific system the military project officer tends to specify parameters prematurely, as Nelson suggests, but that says little about the best way to manage R and D. Regrettably, his reflections on this topic add little to the published work of a decade or so ago at RAND and lack the clarity and substance of his own published work in this area.

Mr. Slitor's suggestion that Congress and the public should review proposals for tax concessions with at least the same care and skepticism as they do expenditure proposals deserves nothing but praise; and, I would add, it is about time, too. I suppose that the reason that tax privileges have been handed out so freely is that direct grants and subsidies appear in and add to the appropriations budget, as visible, vulnerable line items, whereas tax concessions do not. Hopefully the sophistication and responsibility of public thinking on these matters will in time improve, and Slitor's paper exerts a push in the right direction.

Although tax privileges have the superficial advantage over direct subsidies that they appear easier to administer, fitting as they do into an existing administrative structure, direct subsidies have what to me are decisive advantages: their relation to their supposed objectives is more readily checked, and their proper and uniform administration can be made the (exclusive) mission of those who administer them. Their greater exposure to public scrutiny—the principal reason they have often been avoided—is a further advantage. Accordingly, I would suggest that we consider the British system of an earlier era of granting prizes for successful invention and innovation. A possible way to do this would be to make licensing of patents compulsory for a nominal fee to the user, with a royalty to be paid by the government determined by carefully selected criteria relating to the social value (consumer's surplus) generated by the innovation. If applied carefully, this procedure would resolve the problem of resource misallocation associated with exploitation of patented innovations, as well as the problem of appropriate incentives for such innovation. Proprietary, nonpatentable innovations would not be affected by this procedure, but it is implausible that the search for such innovations drives significant quantities of investment.

These points lead directly to Mr. Goode's paper, in which he suggests a tax deduction for out-of-pocket educational expenditures. The logic of this proposal would be irresistible were it not for the existence of substantial direct subsidies and tax stimuli to education in the present fiscal system. In the case of state universities and colleges this subsidy appears explicitly in the form of line-item appropriations in state budgets; in the case of private ones, the grants are only slightly less obvious, in the form of contract research, NSF grants, and so on. (For example, about two-thirds of the current budget administered by the trustees of one outstanding private university consists of federal government funds.) Moreover, the deductability of gifts under the estate tax and individual income tax constitutes a powerful tax stimulus for bequests and gifts to educational and charitable institutions. If our objective is tax neutrality between education and other forms of investment, we have al-

most certainly erred on the side of favoring education. I can think of no valid reason why the present subsidies and tax stimuli to secondary and higher education should be increased. Inasmuch as Mr. Goode based his argument on straightforward, sensible, economic reasoning, which I applaud, I invite him to reconsider his position.

WILLIAM CRAIG STUBBLEBINE: By direction, I have the privilege of commenting on Richard Goode's paper. If I have understood his remarks correctly, both here and in the citations referred to in his footnote 4, we have before us a proposed change in the tax treatment of net income which is so well thought out that I am left wondering what, if anything, I could possibly say to strengthen his remarks.

The place to start, I think, is by denying Goode's contention that "the amortization plan differs sharply from the proposals for a deduction or tax credit for parents of college students" in that "there is a presumption in favor of the deduction of all legitimate costs of producing income but a presumption against other deductions and credits."

If the proposed change will be trivial, then quite appropriately one might accept Goode's implicit assumption that the definition of income to exclude recovery of invested capital has constitutional standing. "Constitutional" is used here in its widest sense to connote basic agreement on a principle, rule, or decision in terms of which subsequent decisions are made. The amortization of educational investment then might be viewed as a technical refinement designed to bring current practice in line with the constitutional definition. The difficulty with this approach is that, if trivial, it really does not matter whether the change is or is not made.

If, on the other hand, the change will induce significant shifts in the variables by which outcomes are characterized, it would be naïve in the extreme to defend the proposal as a technical refinement. Under the circumstances, bringing home the fact of discrepancy between current practice and the definition must lead to a reopening of debate at the constitutional level. What would be the result of such a debate is an open question. It might be a modification of current practice with regard to human capital; it might be a modification of the income definition to accord with current practice; it might even be a modification of the definition such that the recovery of investment in physical capital is accorded the same treatment now accorded investment in human capital.

The outcomes referred to may be characterized by several (conceptually) measurable variables. The percentage of the adult population educated in institutions of higher learning, the social rate of return on (higher) education, the rate of change in per capita standard of living, the distribution of income or wealth are examples, as are any of a hundred other variables—depending upon one's ingenuity and concern. The variables, in turn, are related, at least inferentially, to widely accepted social goals—freedom, efficiency, equity, and economic growth—which constitute the "good society."

Presumably, constitutional standing was achieved because the outcomes ex-

pected under the income definition would be more favorable than those outcomes predicted to occur under (all) alternative definitions. Informed choice regarding a nontrivial proposal now would involve comparison of outcomes believed attainable in the future under all alternative decision-making institutions. If we have learned anything as economists, it is the folly of attempts to evaluate the "goodness" of one alternative in isolation.

But, surely, these are the same thoughts one would wish to record with regard to a tax credit or tax deduction proposal for education—or research.

Now there may be good reasons for doubting the efficacy of the amortization proposal. A complicated tax law would be made even more complicated. It would make little sense if the investment benefits to individuals and society were offset by the resource costs of compliance and administration.

Present Treasury regulations, which allow the deductibility of costs of maintaining earning capacity, undoubtedly embrace many instances of new investment. (In this vein, one might ask whether the young man who is told by his college that he must complete the Ph.D. to retain his present position as instructor, may deduct his thesis expenses.) Goode has noted that, for most students, the tax saving in future years would amount only to 3 to 4 percent of his total private costs (including foregone earnings)—say, sixty dollars per year for ten years in the case of a four-year baccalaureate program. And there is a real question as to whether students or their parents, government, and colleges control the bulk of investment decisions.

Exploring the elasticity of demand by young adults for higher education, however, tends to divert attention from what in the last analysis may be the most important problem. Just a year ago at these meetings, a discussant (Milton Friedman) on the panel on "The Evolving International Monetary Mechanism" recorded a trenchant observation which, with but minor modification, would be fully applicable to the organization of higher education: "One respect in which governmental or intergovernmental agencies are unquestionably preeminent is their ability to snatch verbal victory from actual defeat. . . . The comments . . . on the recent rescue opertion for sterling are of this genre. All point to the rescue operation with pride. . . . Yet surely, by any disinterested evaluation, the need for such *ad hoc* action on such a scale . . . is a symptom of weakness, not of strength."

How should one regard reports of a crash program to double or triple capacity at State U. in the next five years? How should one regard reports of college presidents making their annual pleas before alumni, philanthropic foundations, and legislative committees for minimal funds to operate their institutions? The American political process and American higher education have responded magnificently in recent years, but by and large only in the sense of an *ad hoc* rescue operation. By any disinterested evaluation, these reports are a symptom of weakness, not of strength.

To obviate future crises in educational financing and operation it would seem imperative to consider institutional alternatives in terms of their ability to adjust to the dynamic changes that can be expected but whose precise form is not now predictable. It is this question in education and investment in

human capital which cries out for rational discussion. There is at least the hint that, for efficiency over the long pull, the body politic must adopt either an institutional arrangement which relies primarily upon and facilitates adjustment through the market or a political institution which facilitates adjustment primarily through the political process.

I lack the temerity to attempt to evaluate here the political alternatives from a national education czar with full taxing and spending powers to town meetings. Yet history suggests that adjustment to changing conditions through political consensus is both difficult and spasmodic.

Neither would I wish to expend whatever good will remains by arguing that an unaided market would be efficient in making resources available for investment in human capital. The excess of social rates of return over private rates of return on education, traceable to the family poverty of bright teenagers and a weak capital market among other things, suggests a (significant) role for extramarket funding in the foreseeable future. But surely, there are better and worse ways of making these funds available.

The system of public and philanthropic grants directly to some but not all institutions is competitive in the pejorative sense with a market-oriented resource allocation. Students face a range of institutions offering substantially equivalent educational products at different prices or different product qualities at substantially the same prices. Informed decision making under such circumstances is extremely costly at best. By contrast, loans and scholarships directly to students are conformable with a well-ordered market institution. The tax capitalization proposal is complementary to increased reliance on loans and full tuition to cover resource costs of education.

It is precisely in terms of making individual choices more rather than less rational, of making market outcomes better rather than worse, that the amortization of educational outlays is relevant. Even if only marginally, present treatment of these costs biases individual decisions in the market against investment in human capital at a time when relatively more, not less, investment seems desirable. In a market economy, there is a presumption that if tax requirements necessitate distortion of market signals, at least let the distortions be in appropriate directions—unless, of course, there exists an alternative arrangement yielding more favorable outcomes.

INTERNATIONAL ECONOMICS: PROGRESS AND TRANSFER OF TECHNICAL KNOWLEDGE

LABOR SKILLS AND COMPARATIVE ADVANTAGE*

By DONALD B. KEESING
Columbia University

In a previous paper[1] I suggested that patterns of international trade and location are principally determined, for a broad group of manufactures, by the relative abundance of skilled and unskilled labor. To demonstrate the relationship between skills and trade, I computed the skills required to produce several nations' exports and imports, on the assumption that every country used the same direct labor skill combinations to produce each product that were used by the United States. Under this assumption, differences in the skill intensities of two countries' exports could only spring from differences in the commodity composition of the trade. I attempted to justify this method in my other paper and will not repeat myself here; the key requirement is that there must be a strict similarity between American and foreign production methods, at least in one industry relative to the next. My approach must ultimately stand judgment against a systematic comparison of skill requirements in different countries.

My first computations showed strong and systematic differences in the skill-intensity of various countries' trade flows. Here, I shall summarize some rather fascinating new results obtained by this same method. But first, I would like to discuss a question about skills that is fundamental to their role, not only in trade theory, but also in development theory. The skill composition of any labor force can be modified through training. Skills available in each population also change as a result of demographic shifts, including migration; and skill requirements change with technology. Why, then, should skill differences among populations persist and shape rather stable patterns of trade?

To answer that skill differences among populations reflect or embody contrasting levels of economic development is merely to reformulate the question—to ask why some countries are poor. Moreover, trade in manufactures with different skill requirements persists among industri-

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[1] "Labor Skills and International Trade: Evaluating Many Trade Flows with a Single Measuring Device," *Rev. of Econ. and Statis.*, Aug. 1965.

al countries at a similar level of development. Which characteristics of skills and which obstacles to training are responsible for persistent patterns of comparative advantage based on skill supplies? Which skills are most responsible? How fast can we expect skill differences among populations—and the consequent basis for trade—to shift?

To assist exploration of these questions, I shall introduce a simple model in which I shall specify alternate forms of the training process and examine their influence on trade. I shall assume a world in which there are no transport costs or other trade barriers. Perfect competition reigns. Tastes are everywhere the same, independent of the level and distribution of income. Production functions are the same in all countries and are linear and homogeneous in the first degree. The only factors of production are unskilled labor and one or more types of skilled labor.

I shall further assume that this world starts at time zero when each country is endowed with different skill supplies, expressed as fractions of the national labor force. I shall return to this assumption later. After time zero, populations are constant and international migration is not allowed, though workers move freely within each country. Skilled workers die off—with the initial skilled and unskilled workers exhibiting the same mortality rate—and people who are born inherit no skills. Skills are replaced, acquired, or changed by learning processes specified differently in each variant of the model.[2]

To start with, consider three variants that all involve essentially the same outcome. First, suppose the learning of skills were a costless and instantaneous process. Identical wages would immediately become established for all skills. This would eliminate any locational cost advantage based on the initial distribution of skills. As a second variant, suppose that the learning of new skills were costless, disturbing neither production nor consumption, but merely required a greater or shorter period of time. At first, trade patterns would relate to the initial distribution of skills; skills that were scarce, relative to the demand, might command a wage premium. After sufficient time had elapsed for new workers to be trained in every skill, all skills would command the same wage as unskilled labor, eliminating the original locational cost advantages. As a third variant, suppose that acquisition of skills involved a cost, but a cost payable entirely in terms of unskilled labor. For example, the simplest form of training cost would occur if workers had to stay out of the labor force in order to teach themselves. In this case, let us assume that workers would train themselves in skills wherever they expected, over their lifetime, to be repaid, with appropriate

[2] Here and later, my approach borrows heavily from the work on "human capital" of Theodore W. Schultz, Gary S. Becker, Jacob Mincer, and others; the debt is gratefully acknowledged.

interest, the earnings that they would forego by not working as unskilled labor during their training period. Initially, the wage differential of skilled labor over unskilled labor and the pattern of trade would be governed by the supply and demand situation set up by the arbitrary initial distribution of skills. In the long run, if time preferences were the same everywhere, the wage of unskilled labor would be equalized everywhere through trade, training costs would dictate a common wage structure for all countries, and the initial locational cost advantages would disappear. The results would be the same if other workers besides the trainee were required in the training process, as long as only unskilled labor was required.

In each of these three cases, how labor skill supplies in each country would react to changing wage differentials and how trade would adjust to the elimination of its original locational cost basis are matters for futher specification. Under appropriate assumptions, some form of inertia would maintain an international division of labor related to the initial skill situation; under other assumptions, trade would disappear. From these variants, we see that transformation of the factor supply through training could serve to eliminate a need for trade, though trade could equally sustain an arbitrary, asymmetrical distribution of skills and training.

A modification in the original assumptions, of course, might create locational cost differences around which trade would appear. An interesting case would arise from frequent changes in technology and demand. Skill requirements would change continually, perhaps in unforeseen ways. Trade would emerge based on disequilibria between current needs and past training. Countries (and individuals within them) would reap windfall gains by having been trained in the skill that proved to be most needed as technology shifted. The length of the training period as well as the cost would help to determine the extent of these gains. Under conditions of stable technology and demand, by contrast, the introduction of transport costs into the model, while creating numerous theoretical complications, would almost certainly result in a tendency toward each country or region producing for its own consumption.

Now consider a case where the training of skilled workers could only be done by skilled workers of the same type. The country that started at time zero with a relative abundance of engineers would also possess a relatively great capacity for training engineers. How a country's supply of engineers would react to any level or change of the wage differential of skilled over unskilled workers calls for special assumptions. After all, if the rewards of joining a professions were to increase or decrease, part of the training cost (the pay of the teachers) would shift in the same way, though the other part, the opportunity

cost of the students, would move separately. Presumably a high wage differential, indicating a shortage of engineers, would cause an increased rate of training.

One possibility is to assume that an equal fraction of the engineers would be diverted into teaching in all countries having the same absolute wage differential, this fraction rising or falling with the wage differential. In this case, if the initial distribution of skills resulted in immediate factor price equalization through trade, each country would train engineers in proportion to their initial numbers, the world total swelling or contracting according to the demand for engineers. If, instead, the initial skill endowments were so unevenly distributed as to cause specialization and factor price divergence, training would take place more rapidly, relative to the initial numbers of engineers, in the countries with the greatest shortages; but this process would continue only until factor price equalization was achieved, which would occur long before the initial asymmetrical distribution of skills had been overcome. In either event, in the absence of further disturbances a stationary state would be reached, in which trade would continue indefinitely along lines related to the initial skill distribution. Even if, as a matter of government policy, countries with a low proportion of engineers in their labor force were to subsidize training and divert an extra large fraction of their engineers into training, they could do so only by temporarily increasing imports of engineer-intensive products and driving up the wages of engineers everywhere, or by artificially restricting consumption of engineer-intensive products.

In short, if skilled workers were needed to train skilled workers, unless we permitted an international movement of trainees or of trained workers, the original skill endowment situation would exert a prolonged influence on trade patterns and would greatly benefit any country that started with a generous supply of much needed skill. Substantially the same results would occur if we assumed more complex requirements for training skilled workers, as long as there was a recursive nature to the process, with skills figuring in their own and each other's training requirements.

To suggest another variant, suppose training requirements of the type just discussed were combined with changes of demand and technology over time, in ways not well understood in advance, which, viewed in retrospect, turned out to consistently benefit certain skills in which the initial international distribution had been very uneven. In such a model we would observe increasing income gaps between skill-rich and skill-poor nations.

These possibilities underline a need to explain the assumption that at time zero each country was endowed with different skill supplies.

What I have in mind is that initial differences in the availability of skills in different populations could have resulted from historical and cultural causes before good communications spread tastes and knowledge of production techniques worldwide.

Lingering effects of cultural differences might continue to enlarge skill differences among countries. Important cultural differences would include a difference in time preferences and, acting in the same way, special tastes for education and training. Culturally-induced or physically-induced differences might also occur in aptitude for technical training.

Relaxing other assumptions, time preference—and thus the propensity to "save" through additional training—might be a function of income. If so, high-income nations would have an especially strong demand for education. Migration might operate to reduce skill differences between populations—international movements of trainees would be especially important—but, conceivably, migration might sharpen skill differences instead. A country enjoying not only a relative abundance of skills but—to readmit other factors into consideration—abundant property per capita and economies of scale might enjoy especially high absolute wages and incomes. Such a country might prefer skilled people, and operate its immigration restrictions accordingly. Even if it did not discriminate, skilled people, having higher incomes in their first homes, would be better able to afford the costs of migration.

In view of the possibilities of learning by doing and the need for educational aids in the training process, a high-income country able to afford an abundance of mechanical equipment and other training aids may also enjoy advantages in training as a result of its material wealth.

To summarize, skill differences sufficient to produce persistent patterns of trade among nations may be caused by: (1) the lingering influence of historical differences in skill supplies, propagated down to the present moment by a need for skilled workers to train skilled workers; (2) cultural (or conceivably, physical) differences leading to contrasts in desire and aptitude for skill-acquisition; (3) unequal incomes, perhaps relating to the skill situation, combined with a functional relationship between income and education and also between material equipment and the learning process; (4) selective migration; and (5) an arbitrary division of labor that is sustained by trade.

Because skilled people are needed to train skilled people, national advantages in skills may cumulate and may be reinforced by technological change even if differences in the desire and income to acquire skill are offset as much as possible by national policies. It would follow

that only the international movement of skilled labor can furnish early relief to skill-poor areas, unless physical equipment can be made to substitute for skilled people in the training process.

Turning to new empirical evidence, I have applied U.S. skill coefficients, based on the 1960 census of population, to the 1962 trade of several countries. This time I included forty-six manufacturing industries—thirty-five that I consider "foot-loose" or market-oriented, plus eleven that involve natural-resource processing, such as primary metals, oil refining, and food processing.[3] I have divided labor requirements into eight skill categories, involving a more detailed breakdown than I used before, especially in the highly skilled categories:

I. Scientists and Engineers
II. Technicians and Draftsmen
III. Other Professionals
IV. Managers
V. Machinists, Electricians, and Tool- and Diemakers
VI. Other Skilled Manual Workers
VII. Clerical and Sales Workers
VIII. Unskilled and Semiskilled Workers

Tables 1 and 2 summarize the labor requirements by skill category of several countries' exports and imports, computed as though the goods that they traded were all produced with American skill combinations. The sharpest contrasts between export requirements and import-competing requirements and between different countries' exports appear in requirements for professional skills—especially scientists and engineers, but also technicians and draftsmen—and in the ubiquitous manual skills grouped in Class V, machinists, electricians, and tool- and diemakers. At one extreme, the exports of India and Hong Kong display low requirements for skilled labor together with high requirements for unskilled labor. The United States, at the other extreme, has the most skill-intensive exports and, generally, shows signs of having the greatest relative abundance of hard-to-acquire skills, notably professionals, and especially scientists and engineers.

Whether or not my method is valid with respect to foreign countries' trade, the direction of specialization by the United States is surely valid, since I am using U.S. coefficients. Rank order and linear correlations relating American skill requirements to an indicator of U.S. trade competitive power—U.S. exports as a percent of those of all fourteen countries shown in Table 1—are summarized in Table 3. These cor-

[3] The scheme shown includes about as broad a coverage of manufacturing as the data will sustain—considerably broader than in my previous measurements. Details of the calculations, including the technical coefficients used and related findings, will be reported in a technical monograph, to be issued by the International Economics Workshop, Columbia University.

TABLE 1

LABOR REQUIREMENTS BY SKILL CLASS TO PRODUCE 1962 EXPORTS OF FOURTEEN COUNTRIES, USING 1960 U.S. SKILL COMBINATIONS, FOR FORTY-SIX MANUFACTURING INDUSTRIES INCLUDING NATURAL-RESOURCE PROCESSING

Country	Man-years Per Billion Dollars of Exports	Percentage Distribution of Labor Requirements by Skill Class							
		I	II	III	IV	V	VI	VII	VIII
U.S.	48,194	5.02	2.89	2.74	4.85	8.38	14.96	15.73	45.42
Canada	34,881	4.17	2.33	2.43	4.76	5.39	16.45	14.70	49.76
U.K.	49,833	3.77	2.29	2.36	4.79	7.20	15.01	14.91	49.68
Austria	52,954	2.76	1.76	1.91	4.15	5.71	15.97	12.87	54.87
Belgium	48,611	2.83	1.71	1.98	3.86	4.67	17.35	12.75	54.85
France	49,381	3.15	1.92	2.15	4.58	5.28	15.55	14.14	53.24
Germany	50,459	3.89	2.48	2.33	4.69	8.44	15.84	14.54	47.79
Italy	52,304	2.75	1.75	1.97	4.33	4.32	12.78	13.24	58.86
Netherlands	44,519	3.62	2.39	2.31	4.65	5.04	15.62	14.50	51.87
Sweden	49,984	3.53	2.34	2.23	4.41	8.92	18.87	13.73	45.96
Switzerland	54,971	3.50	2.39	2.18	5.29	7.76	12.66	15.65	50.56
Japan	57,842	2.48	1.66	1.78	3.96	4.56	15.15	12.04	58.38
Hong Kong	74,304	0.69	0.49	1.13	3.75	1.34	8.48	10.39	73.73
India	66,517	0.71	0.58	1.06	3.47	1.33	11.13	9.62	72.09

Skill Classes are:
I. Scientists and Engineers
II. Technicians and Draftsmen
III. Other Professionals
IV. Managers
V. Machinists, Electricians, and Tool- and Diemakers
VI. Other Skilled Manual Workers
VII. Clerical Workers
VIII. Unskilled and Semiskilled Workers

TABLE 2

LABOR REQUIREMENTS BY SKILL CLASS TO PRODUCE 1962 IMPORTS OF THIRTEEN COUNTRIES, USING 1960 U.S. SKILL COMBINATIONS, FOR FORTY-SIX MANUFACTURING INDUSTRIES INCLUDING NATURAL-RESOURCE PROCESSING

Country	Man-years Per Billion Dollars of Imports	Percentage Distribution of Labor Requirement by Skill Class							
		I	II	III	IV	V	VI	VII	VIII
U.S.	43,719	2.77	1.71	2.02	4.63	3.88	13.87	13.74	57.38
Canada	50,078	4.09	2.37	2.60	4.70	7.05	14.74	15.32	49.12
U.K.	43,030	3.21	1.98	2.13	4.94	5.30	14.25	14.25	53.96
Austria	49,516	3.38	2.16	2.27	4.72	7.10	14.37	14.45	51.55
Belgium	46,151	3.71	2.26	2.34	4.58	6.10	14.99	14.48	51.54
France	51,090	3.62	2.19	2.33	5.22	6.56	15.65	15.55	48.88
Germany	48,389	3.02	1.88	2.00	4.48	5.26	14.57	13.54	55.24
Italy	48,642	4.22	2.53	2.53	4.59	7.86	16.20	14.58	47.65
Netherlands	50,400	3.89	2.39	2.29	4.41	6.17	14.93	13.91	52.01
Sweden	46,007	3.56	2.28	2.26	4.52	6.26	14.92	14.11	52.08
Switzerland	47,581	3.48	2.14	2.28	4.66	6.41	15.11	14.47	51.46
Japan	46,045	5.12	3.12	2.71	5.10	9.53	15.87	15.94	42.62
India	40,248	4.31	2.62	2.46	4.62	7.08	17.32	14.87	46.71

NOTE: For Skill Classes see Table 1.

TABLE 3

CORRELATIONS FOR THIRTY-FIVE AND FORTY-SIX INDUSTRIES BETWEEN THE PERCENTAGE OF THE LABOR FORCE BELONGING TO EACH SKILL CLASS, 1960, AND THE INDUSTRY'S TRADE COMPETITIVE POWER
(See Note)

Skill Class	Thirty-five Industries†		Forty-six Industries†	
	Type of Correlation		Type of Correlation	
	Simple	Rank‡	Simple	Rank‡
I. Scientists and engineers	.72*	.60*	.49*	.43*
II. Technicians and draftsmen	.55*	.52*	.37*	.47*
III. Other professionals	.58*	.73*	.41*	.55*
IV. Managers	.06	.21	.16	.17
V. Machinists, electricians and tool- and diemakers	.37*	.40*	.22	.29
VI. Other skilled manual workers	.21	.44*	.11	.29
VII. Clerical and sales workers	.44*	.65*	.35*	.54*
VIII. Unskilled and semiskilled workers	−.64*	−.67*	−.45*	−.54*

* Statistically significant at .05 level.
† Thirty-five industries exclude, while forty-six industries include, industries classified by the author as natural resource processing.
‡ Rank correlations show Spearman coefficients.
NOTE: Industries' "trade competitive power," in the context of this table, stands for 1962 U.S. exports as a percent of the 1962 exports of fourteen countries shown in Table 1.

relations confirm that U.S. comparative advantage centers in industries involving a high percentage of professional labor and a low percentage of unskilled labor.[4]

Now let us look at the net flow of resources, in the form of the services of skills embodied in goods traded, from individual industrial nations and from the industrial centers to the primary-producing nations. By net flow I mean the skills required to produce a country's exports of manufactures minus skills that would be required to replace its imports of manufactures. To perform this computation with my empirical method, it is necessary to consider, not the average skill content per billion dollars of trade (as in Tables 1 and 2), but the absolute number of American workers in each skill category that would be required to reproduce each country's total exports and imports of manufactures.

[4] Multiple regressions explaining trade in terms of several skills are unreliable because of the correlations among the skill requirements themselves. But an adjusted R^2 of .50 in thirty-five industries, and .22 in forty-six industries, is achieved simply by correlating scientists and engineers as a percentage of the labor force with U.S. exports as a percent of fourteen countries' exports. The adjusted R^2 can be increased to .54 and .24 by using in a regression two skill groups together—Class I and Class VIII; adding others does not help. In another experiment I have computed export- and import-competing industries' requirements for educated and specialized vocational training, applying the estimates given by Richard Eckaus in "Economic Criteria for Education and Training," *Rev. of Econ. and Statis.*, May, 1964. It turns out, for example, that 7.9 percent of the labor force required to produce U.S. 1962 exports, but only 5.8 percent of the labor force required to provide 1962 imports, consisted of occupations considered to require four or more years of specialized vocational training.

TABLE 4

NET FLOW OF LABOR EMBODIED IN 1962 TRADE, BY SKILL CLASS, FOR TEN LEADING INDUSTRIAL COUNTRIES BASED ON 1960 U.S. SKILL REQUIREMENTS

(Computations Covers Forty-Six Manufacturing Industries)

Country	Exports Minus Imports	Net Balance in Trade (in Man-years) Based on American Skill Requirements, by Skill Class							
		I	II	III	IV	V	VI	VII	VIII
U.S.	6,152	24,688	13,780	11,653	16,921	43,729	53,616	55,345	108,887
Germany	5,897	14,466	9,380	8,104	15,116	35,016	52,616	47,822	127,051
U.K.	4,177	9,398	6,257	6,236	11,324	21,706	38,165	37,689	111,320
Japan	2,595	2,191	1,709	2,302	5,890	3,916	25,167	17,450	111,247
France	2,347	2,516	1,567	2,008	3,764	3,289	16,818	12,722	66,705
Italy	582	−1,005	−419	257	1,418	−3,343	344	3,531	40,590
Belgium	770	168	79	435	813	259	10,532	3,486	28,186
Netherlands	−124	−1,357	−600	−530	−760	−3,143	−2,749	−2,644	−13,197
Switzerland	−177	246	415	35	973	−1,884	−1,728	2,192	2,250
Sweden	−392	−391	−169	−254	−568	2,013	2,416	−1,811	−11,556
(EEC)	(9,710)	(15,507)	(10,417)	(11,190)	(21,363)	(34,923)	(79,917)	(67,533)	(254,909)
Total	21,829	50,900	31,999	30,246	54,891	105,326	195,197	175,782	571,483
U. S. % share	28.2	48.5	43.1	38.5	30.8	41.5	27.5	31.5	19.1

For Skill Classes see Table 1.

To demonstrate this method, in 1962 the United States exported manufactures (in forty-six industries) worth $14,219 million and imported manufactures worth $8,067 million. By my measurements, 34,430 scientists and engineers were required to produce the commodities exported compared to 9,762 who would have been required to replace the imports. This leaves a net balance of 24,668.

By a similar application of American requirements to the trade of several countries, I obtain the results shown in Table 4. The value of the trade balance shown in this table is influenced by the natural-resource content of the products being traded, whereas the skill balance is limited to direct requirements of the manufacturing activities involved. The table reveals that, in highly skilled categories, the United States provides almost one-half the apparent net "flow" of skills from the industrial countries.[5] Three industrial countries in combination meet about nine-tenths of the net total requirements of the skill-deficit countries for hard-to-acquire skills: the United States, West Germany, and the United Kingdom.

The totals give some indication of the net flow of the services of skilled labor in trade from the industrial countries shown to the skill-deficit countries not included in the table. The apparent flow to the countries not shown in Table 4 has the following skill requirements, expressed as a percent of the total labor requirements: Class I, 4.19; II, 2.63; III, 2.49; IV, 4.51; V, 8.66; VI, 16.05; VII, 14.46; VIII, 47.00. By the standards of the first two tables, this is a skill-intensive pattern.

I expect that other interesting findings can be gleaned from this approach. As a longer-term undertaking, these results seem to me to point to a need for a major effort to compare disaggregated skill requirements in the production processes of different countries. Existing data are woefully deficient for this task, and the cooperation of governments will be required.

[5] Only the United States, of the countries shown, reports imports f.o.b.; the others report them c.i.f. This results in an underestimate of the net flow from these countries, and causes a slight exaggeration of the American relative share.

TRANSFER OF TECHNICAL KNOWLEDGE BY INTERNATIONAL CORPORATIONS TO DEVELOPING ECONOMIES

By Jack Baranson
Indiana University

The international transfer of technical knowledge and the industrial application of it in new locations are important factors in the economic growth of the developed countries. This is evidenced by the substantial volume of direct international investment and of international payments for licensing, royalty, and management fees. Such transfers, which typically occur as part of the international operations of large corporations, are also of great importance for the developing countries. This is especially true in view of the emphasis these countries place on industrialization, and more recently upon their need to develop exports of manufactured goods. But developing economies encounter a number of practical difficulties in absorbing foreign techniques. These difficulties are the subject of this paper.

I first discuss the nature of what it is that must be transferred, and then the obstacles to transference inherent in the environment. (These obstacles are basically of two kinds: economic obstacles inherent in the fact of underdevelopment and obstacles created by development and balance-of-payments policies.) I then discuss the major difficulties encountered by corporations attempting to transfer and implant industrial technology. Contrary to what is commonly believed, shortages of capital and machine skills are not the main source of difficulty; rather the problems are associated with limitations on the scale of the market, the existence and competence of local suppliers and managerial capabilities, and with the length of the "running-in" periods usually required. Against this background, I analyze the potential role of international corporations in the transfer and implementation of technical knowledge. In conclusion, I discuss the major implications of my analysis for trade and industrialization policies and policies toward international corporations. Throughout, I shall illustrate my argument by reference to the problems of initiating the production of diesel truck engines in developing countries—a subject on which I have been conducting research during the past two years [7].

Nature of Technological Transplants

Technological transplants consist of product design, production techniques, and industrial systems to plan, organize, and carry out a

production plan. The difficulties involved in such transplants are a function of: the quantum and complexity of the family of components and parts required to reproduce the industrial product; the engineering gap between transmitting and recipient firms; the industrial gap between the economically advanced and the developing economy; and the extent to which policies of economic autarky in force deny entry to imports of producers' goods to offset domestic deficiencies.

1. *Product Design.* Let me turn first to the problem of designing a product for a particular set of environmental factors and conditions. In the design of diesel engines for commercial trucks, for example, legal restrictions on truck loads, minimum speeds to keep traffic moving, and average grades and types of road surfacing affect horsepower requirements and transmission design. Engine horsepower and acceleration features have to be adapted to differences in transmissions systems and truck driver habits [1].

In underdeveloped areas, a more efficient use of products and equipment often calls for gearing down the scale and sophistication to the lower volumes in the various categories of demand (and the related need to minimize initial capital outlays) and the dearth of maintenance and other support facilities and services. For example, the development of an air transport carrier particularly suited for underdeveloped areas would call for low initial cost, ruggedness and reliability, short-take-off-and-landing features (to compensate for inadequate airport facilities), multipurpose adaptability (to accommodate a diversity of demands, each of low volume), simplicity in aircraft handling, and independence from ground support (to accommodate inadequate operational skills and support facilities) [2] [3].

The product mix of an international corporation serving world markets reflects both the variety of consumer demands and the engineering and production costs necessary to fill these demands in the appropriate quantities and model variations. A basic dilemma encountered in underdeveloped areas is that the scale of markets will not support even a moderate range of product variation without heavy production cost penalties. Product mix and production costs depend upon the firm's engineering ingenuity, commercial resourcefulness, and the market risks it is willing to take.

2. *Production Techniques.* Production techniques are a second element of the technological transplant. A vast array of technical knowledge is often required to duplicate a single industrial product. For example, in the manufacture of a diesel engine for commercial trucks, there are approximately 750 parts ranging from cylinder blocks to fuel injector pins. In the United States, close to 200 plants supply materials, raw castings, forgings, components, and parts to the diesel engine manufacturer. To produce these parts, as many as 300 different mate-

rials are required, each with narrow standards on physical and chemical characteristics and shapes or finishes. Over 10,000 separate manufacturing steps are required to convert materials and castings into finished parts for a single engine model.

The volume of technical knowledge that has to be transmitted in the form of process sheets, blueprints, and manufacturing specifications is governed by the complexity and technical tolerances of the transplant. Typically, hundreds of small details involving manufacturing anomalies or difficulties must be worked out. These include precise instructions on how to operate certain equipment, machine a particular part, or handle particular materials. Included are machine speeds and tolerances, minimum standards for finished parts, and detailed materials specifications. The transmission of technical knowledge on industrial techniques requires a high caliber of engineering and technical personnel at both the dispensing and receiving ends. One measure of a product's technological suitability for an underdeveloped area is the economic costs and time lag involved in transmission and adaptation.

3. *Control Systems.* A third element of modern production systems comprises the controls and mechanisms required to plan and schedule the flow of materials and assure minimum standards in materials and parts. The adequacy of engineering and managerial capabilities to handle these planning and control systems associated with production often determines the efficiency of transferred techniques. Systems include budgetary controls for planning and executing a production plan, production standards to formulate equipment requirements and schedule production, controls over the flow of materials and output, and quality control to assure minimum standards in purchased materials and parts and programmed performance of end-products.

Obstacles to Transference

I now turn to a consideration of the obstacles to transference. Much of the advantage of modern techniques may be lost when a firm is forced to accommodate to the scale of demand and the conditions of supply that prevail in newly industrializing areas—conditions which are in part a reflection of the stage of development and in part the result of governmental policies and regulations. Less developed countries are characterized by a narrow range of industrial capability, a necessarily small scale of production for domestic markets, and a dearth of the technical and managerial skills necessary to adapt and absorb modern techniques in a broad range of industrial activities. Typically, governments in such countries impose additional obstacles to technological transplantation. These include: trade and industrial measures that limit access to external supply and influence the internal supply structure, economic planning which influences both demand and supply

conditions, and regulations governing foreign investment that indirectly affect the quality and efficiency of production.

On the matter of trade and industrialization policies, most developing economies have relied heavily upon import substitution—a path to industrialization that has been criticized as just another form of economic nationalism detrimental to world trade and development [6]. More directly, import-substitution policies now in effect in many newly industrializing areas, while providing protection for domestically produced products, have at the same time intensified supply difficulties and restricted demand to domestic markets. The tendency has been to foster small-scale, high-cost plants producing for domestic markets of limited size and insulated from competitive forces. A proliferation of these small, inefficient factories has had the self-defeating effect of spreading scarce resources, especially technical and managerial personnel, too thinly and of intensifying balance-of-payments difficulties (by generating new demands for imports of materials and parts to maintain established plants or their suppliers).

The combined effects of the policies and conditions described has been to raise manufacturing costs considerably above those in industrially-advanced countries. In India, for example, it will cost at least three to four times as much as the import price at the current exchange rate to manufacture diesel engines for commercial trucks. Capital costs run between 3.3 and 5.8 times comparable costs in the U.S.—even at full capacity the Indian plant is designed to turn out only 2.5 percent of the value added by the comparable U.S. plant with about 8 percent of the plant and equipment. Indirect labor and other overhead charges run between 4.6 and 9.6 times U.S. prorated costs, depending upon plant capacity utilization. Procured materials and parts average two to four times U.S. costs [7]. The Edelberg study of the Mexican automobile parts industry found that the effect of import substitution policies and mandatory domestic content requirements has been to increase procurement costs on such parts as shock absorbers, battery cables, and tires to two to five times U.S. procurement costs [8, pp. 34-40].

Economic planning, which is typically preoccupied with supply and demand balances rather than comparative costs, reinforces the tendency toward indiscriminate support of high-cost industry [4, p. 392]. Misallocations of resources are further intensified by price distortions and by allocations systems for foreign exchange, capital equipment, and scarce materials. Without the pervasive influence of a competitive price system, there is no general tendency for the economy to specialize in industries based upon comparative advantage or select production techniques that economize on the real scarcities in production factors [4, pp. 391-94] [9, pp. 151-63].

An ambivalent attitude toward foreign enterprise—on the one hand encouraging foreign investment in order to acquire modern technology and additional foreign exchange and on the other hand limiting foreign participation and control—has also resulted in conflicts over such dimensions of technological transplants as the standard of quality to be maintained in domestic production [10, pp. 63-66].

Difficulties Encountered by International Corporations

So much for the nature of technological transplants. I would like next to consider the difficulties encountered in transplanting industrial systems. Manufacturing affiliates experience technical difficulties in adapting to the smaller scale and narrower range of supply and industrial capabilities. They also encounter financial difficulties in surviving during phase-in periods.

To begin with, manufacturing plants serving the domestic markets of underdeveloped areas are characteristically only a fraction of the size of internationally competitive plants. In Mexico, for example, the automobile industry currently operates at less than 1 percent of U.S. output, and this production is divided among twelve auto assembly plants [8, pp. 30-34]. A particular diesel engine plant manufacturing for the Indian market was built to produce less than 3 percent of a U.S. plant's output. The adversity of the scale factor has prompted the Mexican government to "rationalize" automobile production in Mexico by taking measures to reduce the number of automobile and truck manufacturers and, wherever possible, to standardize parts such as crankshafts and transmissions and consolidate their production in larger scale units [8, p. 176].

Small-scale production throughout the economy also means higher procurement costs on materials and parts. In India, for example, many materials, such as various shapes and kinds of steel that can be purchased as standard mill stock and are volume produced in the United States, must be ordered in small quantities at much higher costs. For example, for such a simple item as capscrews (used in diesel production) a reasonable price can only be obtained for a quantity that would suffice for several years at current rates of production.

Developing local suppliers is a second major problem. Because of the dearth of technical capabilities in supporting industries, considerable managerial talent has to be devoted to rendering technical assistance to domestic suppliers. There is usually an almost complete lack of local engineering capabilities to initiate the required quality control, materials standards, and laboratory testing [8, pp. 57-68, 125-35]. Quality of supplied materials and parts has also been a major problem with rejection rates that run anywhere from 10 to 50 percent. For ex-

ample, capscrews manufactured in India had been improperly heat-treated, which, in combination with the lower quality of the steel, resulted in the screw snapping or the shape distorting under tension. Similarly, high porosity in iron castings for pump housings have resulted in leaking parts. In Mexico, quality standards in auto parts were also found to be substantially lower than those in the United States [8, pp. 57-68, 125-35].

A third problem faced by international corporations involves the adaptation of manufacturing techniques to local conditions. This adaptation requires the very capabilities and industrial organization that developing economies lack and are anxiously trying to develop. There is a marked difference in this respect between the absorptive and adaptive capacity of a country like Japan and that of newly industrializing societies such as India or Mexico. Japan has the engineering and technical skills required to convert foreign techniques; an abundant and highly skilled industrial labor force able to read blueprints, set up tools, and in other ways substitute human skills for machine capabilities; and a well-integrated sector of small subcontractors that can produce according to prescribed engineering standards and under tight production schedules. It is for this reason that the small-scale, labor-intensive sector in Japan is able to coexist with the industrially-advanced, capital-intensive sector [12] [13]. This kind of efficient industrial substructure is lacking in most less developed countries.

There is also the related problem of adapting systems to plan and control production. Characteristically, great care and effort are required to introduce the concept and use of process sheets, production standards, machine-load studies, materials control, and other plant engineering and production control procedures. When adopted, techniques are rarely applied in a systematic and comprehensive way, and the results are inevitably bottlenecks, shortages, and idle capacity, while machines or labor or materials wait on one another. Too often, managers in newly industrializing areas view rigorous production planning and control as an exaggerated fetish on the part of foreign engineers or as systems inapplicable to their environments.

Corporate survival during adjustment periods, which may run several years, is the fourth major problem faced by international corporations. The combination of import restrictions, domestic procurement difficulties, price controls, credit restrictions, and market uncertainties often results in a cost-profit squeeze that threatens corporate survival during phase-in periods. The Indian affiliate of a large diesel engine manufacturer incurred losses amounting to 20 percent of net worth during the first three years of operations as a result of the failure of demand to materialize and of supply and production difficulties. Shifts in planning emphasis and production difficulties experienced by equip-

ment manufacturers requiring diesel engines resulted in drastic cutbacks on orders to less than 40 percent of original estimates. In Mexico, the combination of government price controls and high procurement costs has resulted in a similar cost-profit squeeze impinging upon corporate survival [8, pp. 24-26]. Manufacturing firms that attempt to introduce products and techniques that are too advanced for the economy's industrial capabilities are especially vulnerable to phase-in difficulties. A dilemma arises from the fact that many international corporations are either unwilling or unable to make the necessary product design adjustments.

Policy Implications

The foregoing analysis of the difficulties of making technological transplants—and the reasons for them—indicates that the feasibility of such transplants could be increased by certain changes in the economic policies of the developing countries and the corporate policies of international enterprises engaged in them. There is an obvious need to amend import substitution policies that have indiscriminately fostered inefficient industries. Characteristically, the effect of ambitious domestic content regulations has been to spread scarce technical and managerial resources too thinly in small-scale, uneconomic plants. A side effect has been to price domestic industries out of world markets; thereby precluding any possibility of eventually balancing foreign exchange flows in the manufacturing sector.

Improvement in economic efficiency calls for the progressive abandonment by the developing countries of indiscriminate import substitution in too wide a range of industrially sophisticated activities, in favor of more specialized production for both foreign and domestic markets based upon principles of comparative advantage [6, p. 14] [15]. Concentrating resources upon complementary segments of industry in the technically feasible range could improve foreign exchange positions by achieving export earnings in excess of new import requirements.

A more selective industrialization would also help to reverse the tendency toward external diseconomies created by high-cost suppliers of producers' goods. Wider latitude should be given manufacturing and assembly plants by planning and development authorities on production decisions to make, buy, or import particular materials or parts. This would be especially advantageous to firms operating in a multinational marketing and production context. It must be recognized, however, that if developing economies are to expand their exports of manufactured goods, the industrially-advanced countries will have to be willing to expose their home industries to imports in the cost-competitive range.

Measures by international corporations to downgrade technological transplants to match the emerging levels of industrial capabilities and the available factor supplies would also contribute to improved economic efficiency. (Maximum increases in national product may be realized through a combination of social and private investments to enlarge transformation capabilities, to convert techniques, or upgrade and shift factor resources.) From the corporate viewpoint, there is the question of absorbing the engineering costs required to adapt product design and production techniques. In view of the substantial losses incurred during run-in periods, expenditures on research and development designed to mimimize the losses incurred may be commercially justifiable.

Multinational corporations with their manufacturing and engineering capabilities combined with their access to overseas markets are in an especially favorable position to assign and help develop manageable production roles to manufacturing affiliates in developing economies. International manufacture-and-interchange systems could provide underdeveloped areas with an opportunity to specialize in the production of parts that are within their industrially-feasible range and exchange them for imports in the technically-sophisticated range [7] [16] [17, pp. 14-15]. In the case of diesel engines, a newly industrializing economy would avoid such parts as crankshafts and cylinder liners, which require special castings or materials that are generally costly or of inferior quality when procured domestically.

International firms can utilize their research and engineering capabilities to integrate into their multinational production and marketing systems product and process design parameters drawn from manufacturing affiliates in underdeveloped areas. Adjusting to local manufacturing and supply capabilities would mean choosing products and techniques in the less exacting range of materials standards and manufacturing specifications. The downgrading of technological transplants to more nearly fit an economy's resources and capabilities is perhaps the outstanding contribution that multinational corporations can make to developing countries. Even more important, they can help implant engineering capabilities to adapt and transform products and techniques in response to changes in supply and demand. From the corporate viewpoint, this would minimize the problem of a cost-profit squeeze during hazardous transitional periods. Further thought needs to be given to the question of public and private investments in the conversion of technical knowledge [5].

A combination of adjustments in the economic policies of developing countries and the transplantation policies of international corporations would probably result in the most advantageous technological transmission. Difficulties in technological adjustment are in part a function

of economic autarky: the narrower the sources of supply and of alternative uses of factor resources, the more difficult are such adjustments. Conversely, the more limited the choices among product designs and alternative techniques, the lower the efficiency level in utilizing existing factors and industrial capabilities. In this regard, a more realistic attitude toward domestic content schedules would be advisable. Licensing agreements should be drawn based on more realistic cost and feasibility studies, that would determine beforehand domestic supply capabilities and continuing foreign exchange requirements. Planning and exchange controls also need to be modified to permit wider latitude in production decisions at the plant level to procure domestically or import from abroad.

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THE INTERNATIONAL FLOW OF HUMAN CAPITAL*

By HERBERT B. GRUBEL, *University of Chicago*
and ANTHONY D. SCOTT, *University of British Columbia*

I

We have been drawn to the subject of this paper by recent strong manifestations of public interest in two major problems in international relations: first, the migration of highly skilled individuals to the U.S.—often referred to as the "brain drain"—and, second, the large-scale program of training foreign students in the U.S. Both of these problems have in common that they involve an international transfer of resources in the form of human capital that goes completely unrecorded in any official balance-of-payments statistics. This common feature clearly defines our field of analysis and excludes problems associated with the transfer of human capital services, such as occur in connection with the Peace Corps, programs of technical assistance through governmental agencies, technical and scientific advice by private corporations, etc.—all of which are reflected in official balance-of-payments statistics.

We have prepared some empirical estimates of the U.S. balance of trade in human capital from foreign student exchange and the immigration of scientists and engineers, which will shortly be published. These studies, while involving interesting conceptual problems of measurement, produced no startling results and suggest that in comparison with the size of the U.S. economy these capital flows are quite small. We present here only a few summary statistics to give an impression of the nature of the empirical results we have obtained. First, it turns out that the total U.S. program of foreign college student exchange, involving 58,000 foreign students in the U.S. and 11,000 American students abroad, resulted in a maximum net U.S. cost of only $17 million in 1962, after appropriate adjustment of the gross cost for the human capital value of students electing to remain in the U.S. Second, the total human capital value of scientists and engineers immigrating to the U.S. during the thirteen-year period from 1949 to 1961 came to $1.0 billion. Third, the role of foreigners in the American economics profession estimated on the basis of the National Science Foundation survey statistics is as follows: 12 percent are foreign born, 9 percent had also foreign high school training, but only 3 percent earned their

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highest professional degree abroad. Fourth, the shares of annual output of first-degree engineers lost by emigration to the U.S. by some major individual countries were found to differ widely between countries and tended to be surprisingly high in some instances. For example, Norway lost 24.1 percent, Greece 20.9 percent, Germany 9.5 percent, and France 1.2 percent of their annual output of first-degree engineers to the U.S. Finally, scientists and engineers are from six to twelve times as likely to emigrate to the U.S. as people in other professions, judging from the occupational composition of all immigrants and that of the labor force in the migrants' native countries.

While such empirical work sheds light on the quantitative importance of issues which all too often are argued in complete ignorance of any facts, we have found that nearly all discussions of the brain drain and exchange student programs suffer most seriously from the absence of any theoretical framework. The main part of this paper is devoted to a theoretical analysis of issues surrounding the international flow of human capital embodied in highly skilled migrants to the U.S. and in foreign students electing not to return to their native countries.

II

The argument that a country "loses" by the emigration of highly skilled individuals is most nearly always valid when we consider the "country" to be a nation state whose national objective is to maximize its military and economic power. From this point of view, a person's emigration absolutely reduces his country's mobilizable manpower, and its national output is lowered by the amount the emigrant contributed to it.

While this view of national losses is held quite widely, it is sorely outmoded in our age. The identification of military power with the number of a country's inhabitants, even if they are highly skilled, is very vague and precarious. Wealth, science, and technology dominate modern warfare, and it is quite easy for most nations to purchase military equipment on the world market at costs much below those that would have to be incurred in the development of individual national weapons systems. Economic power, in turn, depends not so much on aggregate national output as it does on per capita income, which may or may not be affected by an individual's emigration.

In place of this outmoded nationalist concept of a country, we suggest the use of another one, according to which a country is an association of individuals whose collective welfare its leaders seek to maximize. While the level of individual welfare is determined by many factors, including items of collective consumption such as military might and foreign economic influence, the most important determinant of

human welfare in the long run is the standard of living; that is, the quantity of goods and services available for consumption. Therefore, in the following analysis we will focus our attention on the changes in income brought about by the emigration of highly skilled individuals.

If a country wishes to maximize the income available to all its people, then emigration should be welcomed whenever two conditions are met. These are, first, that the emigrant improves his own income and, second, that the migrant's departure does not reduce the income of those remaining behind. The first condition is normally met when emigration is voluntary. Specification of the circumstances under which the second holds true will occupy the rest of this paper.

III

According to the traditional analysis of the migration of labor, the departure of a person normally raises the long-run average income of the people remaining, because it results in an increase in the nation's capital-labor ratio. In the case of the migration of a highly skilled person, however, this conclusion does not hold if the human capital embodied in the emigrant is greater than the country's total per capita endowment of human and physical capital, assuming perfect substitutability of the two forms of capital in the long run. In this case the emigration of a highly skilled person reduces the total income to be distributed among the residents of a country and it follows that in societies where this distribution occurs through planning or other nonmarket means the remaining population suffers a reduction in welfare.

In a market economy where persons are paid their marginal product, however, such a reduction in per capita income is only a statistical phenomenon which has no influence on the welfare of the remaining people: the emigrant removes both his contribution to national output and the income that gives him a claim to this share, so that other incomes remain unchanged. There may be income redistribution effects through changes in the marginal products of the remaining people, but since the brain drain involves rather small numbers of people, these effects are likely to be small enough to be safely considered negligible.

Thus it follows that in a market economy any effects that the emigration of a highly skilled person is likely to have on the welfare on those remaining behind must be sought either in short-run adjustment costs or in market failures.

The short-run costs are due to production losses—specifically those created by the unemployment or inefficient employment of factors of production whose effectiveness depends on cooperation with the skills the departing person takes along. The size of these costs depends on two elements. First, the greater the short-run substitutability of other

factors of production or skills for those that have emigrated, the smaller the inefficiencies and loss of output. Second, the more rapidly a replacement for the emigrant can be trained, the smaller the losses. It is difficult to generalize about the characteristics of individual professions or national education systems in regard to these qualities, but it seems reasonable to expect that the emigration of a well-established, experienced professional will cause greater frictional losses than would the emigration of a common laborer or the decision of a student not to return home. Also, we would expect that bursts of heavy emigration alternating with periods of low emigration rates present more difficult adjustment problems than do steady flows, even if the latter represent a greater long-run average than do the former, because of the economy's likely structural adjustments to predictable changes.

Of greater analytical interest than these short-run costs of adjustment to emigration are the long-run effects on welfare associated with failures of the free market to allocate resources efficiently. There are two main sources of such inefficiencies which appear to underlie most of the arguments about losses from the emigration of highly skilled persons.

The first category of losses has to do with genuine externalities, where the market fails properly to compensate the individual for the contributions he makes to society. It is important to note that these externalities must be directly associated with the personal characteristics of the emigrant and not his profession. Thus, if a typical doctor's work contains a large measure of social benefits for which he does not get compensated, these benefits are lost to society only for the length of time required to train another person to take his place as a doctor. It therefore follows that in many of the well-known instances of genuine external effects in consumption or production, emigration imposes only short-run frictional costs to society which disappear in the long run.

While it is difficult enough to find genuine cases of economically significant externalities in the real world, it is even more difficult to find cases which have the added limitation of being associated with a specific person. Examples coming to mind are the external diseconomies from alcoholism or the nonmarket benefits accruing to others from a person's propensity to engage in political or charity work without monetary compensation. The difficulty of finding meaningful examples may legitimately be taken as an indication of the relative unimportance of most externalities given the size of the resources allocated through properly functioning markets.

The second category of losses stems from market failure remedied through activities of the government. It is alleged that the emigration

of highly skilled persons affects others most significantly through changes in the cost of providing such government services.

In this connection, it is frequently suggested that public education is a social investment in individuals which emigrants fail to repay, and that therefore the highly trained in particular ought to be forced to repay this investment before they are allowed to leave the country. Such suggestions and the entire idea of a "debt to society" due to publicly-financed education appear to be based on misapprehension.

Society is a continuing organism, and the process of financing education represents an intergeneration transfer of resources under which the currently productive generation taxes itself to educate the young, who in turn upon maturity provide for the next generation of children and so on. What is relevant for our purposes of analysis is that the average burden of financing education falling on the emigrant's generation is not changed by his departure, because he takes along not only his contribution to tax revenue but also his children, on whom this share of revenue would have been spent.

Analogous arguments can be made for the financing of other government services such as defense, police protection, judicial services, etc. However, in all of these instances, including education, the conclusion that no adverse welfare effects result from a person's emigration is valid only if the incidence of taxes is equal to the incidence of benefits from government services.

There is evidence that the enjoyment of the quantitatively most significant services provided by governments is largely proportional to the taxpayers' income, which includes return on human capital. Defense—the largest item in the budget of many nations—benefits more those persons who, as a result of foreign conquests, would lose sizable stocks of assets than those who do not. Roads are used more by those who drive cars than those who walk. The amount of education demanded by the offspring of the highly educated is likely to be above that demanded by the children of people with average education. Only relatively few government services, such as public parks and those related directly to the welfare of the poor, contain elements of subsidy by high-income taxpayers. Therefore, the presumption is strong that the government can reduce many of the services it provides by nearly the same proportion by which tax revenues decline when a highly skilled person emigrates, changing the tax burden or income of the remaining people only marginally and certainly by much less than the gross reduction in tax revenues suggests.

It is true that if government services are provided through lumpy investment projects, reductions in government services may not be possible without increases in average cost. However, such increased burdens from reduced population are short run and last only until ei-

ther a new, optimum-scale plant replaces the old or as population returns to its old level. At any rate, in most countries complaining of the brain drain the problem is not one of possible excess capacity in public projects but rather one of overcrowding.

It is often argued that a country loses because the highly skilled emigrants would have worked on projects of great importance to the development of the country had they stayed at home. This argument is valid either if we take the nationalistic view of the country or if the person's work would have been associated with large external effects. In this case, also, the nationalistic view is to be rejected for the reasons presented earlier. While it is popular to argue that external effects are frequent in market economies, we have ben unable to discover economically significant instances where individuals provide social services associated with their person rather than profession for which they are not paid—including in work fostering economic development.

Another frequently heard allegation is that the emigration of the highly educated is equivalent to a Darwinian process of selecting the best, which causes a reduction in the genetic "quality" of the country's human stock and influences national welfare in the long run. This is probably a valid argument in principle but its empirical significance is likely to be quite small, given the small relative size of the migratory flows and the population stocks. It should also be noted that the transmission of human characteristics through the genes is a rather unreliable process, and that the offspring of many intellectually distinguished emigrants never achieve their parents' level of attainment.

IV

While our analysis so far suggests that the emigration of highly skilled persons reduces the welfare of the remaining people only under rather rare circumstances, we can make a good case for the proposition that these types of emigrants in fact tend to increase the welfare of their former countrymen in several important ways.

Historically, emigrants have been known to raise significantly the incomes of their families at home through remittances. In more subtle ways emigrants can influence policies in the country of their new residence towards their native country, and often the emigrants retain an interest in their home countries' affairs, giving counsel and advice, which carry great weight because of the positions of independence and prestige they hold in the foreign country. Furthermore, the very act of emigration may be beneficial to those remaining behind just because of the public attention given to the individual's departure, which can lead to critical reappraisals of institutions and procedures and their ultimate modernization and improvement.

The potentially largest benefit to the people remaining behind, how-

ever, may accrue through the pure research of scientists and engineers in the foreign countries, contrary to the often heard allegation that the emigration of people in these fields is the source of greatest material losses. The product of basic research, knowledge, is a free good becoming available to all as it is published. Since most scientists move to countries where conditions of work are better for them, either because the new country is better able to furnish research equipment or because of stimulating colleagues, the probability is great that such moves increase the scientists' overall productivity. As a consequence of such emigration by scientists, the native countries not only obtain the scientific knowledge free, but they are actually likely to get more than they would have had the men stayed at home. Applied research also tends to benefit countries other than the one in which it is first put to use. Reductions in the cost of production or new product developments tend to spread through the world as a result of competition. As far as national prestige from scientific achievements is concerned, the scientists' native countries are perfectly free to claim these men as native sons, which in no way reduces the host-country's right to be proud that the work was done within its borders.

V

We conclude from this analysis that the transfer of human capital occurring when highly skilled people emigrate between countries always reduces the economic and military power of the migrant's native country, though by a smaller amount than it is often alleged. We have argued, however, that such concern with the effects on economic and military power is anachronistic and that a concern with the individual welfare of the population ought to take its place. From this point of view it was seen that the emigration of highly skilled persons is likely to cause economic losses in the short run until replacements for the emigrants can be trained. Long-run losses in a market economy are likely to be small and are primarily associated with externalities and with elements of income redistribution, in the government's tax and expenditure policies. Benefits to the native countries of the emigrants may be sizable, primarily because much of the output of highly skilled persons, especially scientists and engineers, tends to benefit the people of all countries. A good case can therefore be made for a continuation of present policies and the free movement of human capital throughout the world.

DISCUSSION

STEPHEN HYMER: In the developed world, technological progress is rapid and continuous, spreads quickly from country to country, and is the major source of economic growth. In most of the underdeveloped world things are very different; there is little indigenous technological change and the gains from the advanced countries are transmitted only very slowly. I wish there was time to discuss all the points raised in these papers about why this is so and what can be done about it, but I plan to comment mainly on the issues in Professor Baranson's paper on the potential role of international corporations as vehicles for transmitting technical knowledge to underdeveloped countries.

Professor Baranson has analyzed for us the great inherent difficulties of applying advanced technology in underdeveloped countries and the additional obstacles created by government errors in planning and regulating foreign trade and investment. In my comments, I would like to extend Professor Baranson's analysis to deal with some of the limitations of international corporations as means for speeding up technological progress in underdeveloped countries. If governments make errors in judging shadow prices, so do firms. Ignoring for the moment the important question of whose mistakes are greater, I thought it might be useful to add to Mr. Baranson's list one or two difficulties on the side of the firm and to relate them to policies the government might take.

On the surface, international firms would seem to be ideal vehicles for helping underdeveloped countries. They are large and efficient and have access to markets, capital, and technology in many countries. They should be powerful trade creating forces in both commodity markets and factor markets. Yet historically they have played only a minor role as far as underdeveloped countries are concerned. This poor record cannot be attributed to obstacles created by government planning and control, for in many underdeveloped countries, the government was doing everything in its power to encourage foreign investment and development for foreign markets. The result was nonetheless far from satisfactory. The volume of investment was small and its impact limited. The modern techniques it brought, instead of spreading widely, were restricted to enclaves: countries remained backward and outside the mainstreams of the world economy despite their high level of trade.

Professor Baranson's research on the importance of scale and supply conditions helps to explain both the basic predicament of the underdeveloped countries and why the contribution of international firms was so limited. As Adam Smith so correctly anticipated, technological progress comes about through increased specialization and division of labor. The superior techniques used in the advanced countries have the disadvantage for underdeveloped countries of being highly specialized; when used in isolation, they do not work nearly as well as in their original environment. One implication of the law of division of labor is that the underdeveloped countries must plan for increased specializa-

tion and integration into wider units if they are to achieve higher levels of productivity, as Professor Baranson has correctly pointed out.

Another implication is that international corporations may have less to offer underdeveloped countries than would at first seem likely. The reason why there has been so little direct investment in manufacturing in underdeveloped countries in the past may be that the techniques of international firms are so highly specific to developed countries that there was little they could usefully transfer without transferring the whole system of division of labor. This may, however, be unduly pessimistic about their future possibilities. The international firms may well have been biased in their choice of technique and not fully maximizing profits. If so, there is hope that government prodding will lead them to transfer technical knowledge more quickly in the future. This is the rationale for restricting the international firm's freedom to import. Naturally the firms view these restrictions as obstacles and resent them greatly; they would find it much easier to import than to search for ways to use domestic resources. But the firms may be very bad judges of comparative advantage, and the restrictions may lead them to search for and discover new methods which would be profitable even in the absence of these restrictions. Of course, the strategy may not work and it may simply deter foreign investment, but in evaluating this loss, one must keep in mind that most of the new industries are highly protected, and it makes little sense to place tariffs on final goods without restricting intermediate goods as well. To allow the firm wide latitude in choosing between importing or buying locally can convert a small nominal tariff on a final good into a high effective tariff and saddle the country with high import requirements and little increase in value added.

A second problem associated with international firms is that they may be charging too high a price for what they bring. They operate in highly imperfect markets and the price they charge is usually above their marginal cost. The government may be able to use its regulatory power to countervail their monopolistic position.

For example, the important markets for technology are far from perfect. Where the technology is possessed by large numbers of firms, the underdeveloped country will usually not need international firms in order to obtain it; hiring a few good engineers will do as well. But many of the most important technological advances are in the possession of a relatively small number of firms. These firms naturally try to maximize the rent they obtain on their advantage, and they find wholly-owned branch plants and subsidiaries a useful way to accomplish this end. This may not, however, be in the best interest of the country. The government, if it made subtle use of its regulatory powers, might be able to obtain the technology at a lower cost. The issue is similar to the question of the appropriate patent policy in the domestic economy. In the absence of empirical studies, it is difficult to know what is feasible, but I might somewhat cavalierly throw in the fact that Japan, one of the few cases of successful industrialization, pursued a policy of the most stringent limitation on direct investment.

My comments here have stressed the negative rather than the positive side.

Judging from the past, international corporations do not seem to have had much to offer to the less developed parts of the developing world, and they may charge too high a price. Basically, the problem seems to be that the underdeveloped countries need most of all not the technology used in the advanced countries, which is often ill-suited to their resources, but the ability to discover and develop techniques of their own. I am doubtful that firms whose center of gravity is in the developed world will be of much use in the task. They are not truly international corporations but are really national firms and their horizons are limited by their environment. Perhaps with time some of the present international corporations will develop into organizations with major focus on the underdeveloped world. Or perhaps they will act as a catalyst in stimulating the growth of firms native to the underdeveloped countries. On the latter point, the historical record is again not very promising. One of the empirical characteristics of direct investment is that once an international firm establishes itself in a country it tends to maintain its dominant position. Though this is far from conclusive, it does suggest that the demonstration effect is rather a weak one and provides some justification for a policy which compels foreign firms to hire nationals of the underdeveloped country and train them to replace itself. Like the other policies suggested in these comments, it will probably discourage some foreign investment, but it may also increase the benefits from the investment that does come and speed up the process of removing backwardness—the chief problem of the underdeveloped world.

BURTON A. WEISBROD: When skilled, educated people leave their home country, does that country sustain a loss? The analysis by Professors Grubel and Scott has made it clear that the question itself is ambiguous. What does it mean for a "country" to sustain a loss? What forms do such losses take? Are they technological or distributional? The authors' attempt to clarify the issues has been largely successful. However, in the process of writing their thoughtful paper, they have blurred some issues which I shall try to sharpen.

To begin with, I would like to record my full agreement with the authors' analytic separation of the effects of migration on the migrants from the effects on those who are left behind. It is important to ask whether each of these groups is hurt or helped by migration. Resort to such overall measures as income per capita can lead to foolish results, as the authors showed. Even if the income of every one of the nonmigrants were unaffected, income per capita would fall if those who emigrated had above-average income. Grubel and Scott argue that this would constitute no "loss" to those who remain, and I tend to agree. Yet in doing so I am reflecting—and the authors were reflecting—an assumption about individual utility functions in the real world. The assumption is that the welfare of each individual depends only on his own income and not on national aggregates such as total or per capita income. But a question is in order: would citizens of a nation with the nth highest standard of living, as measured by per capita income, be saddened—that is, have their utility indices reduced—if the nation fell to $n + 1$th position because of the

emigration of persons with above average income? Grubel and Scott implicitly assume the answer is no, but this is a question of fact—which needs to be explored.

Before proceeding, it is worth noting that although the title of the paper refers to "international" flows, the analysis can be applied usefully to interregional, interstate, or intercommunity mobility just as effectively. Moreover, much of their analysis applies to the effects of any migration, whether of highly skilled or of unskilled people. There is little in their paper that is uniquely applicable to the emigration of the highly skilled. Unfortunately, the authors do not point out the generality of their arguments, nor do they do much to point out explicitly why a "brain drain" is different from a "people drain."

The heart of the Grubel-Scott analysis is in the rule which they offer for determining whether "emigration should be welcomed." In effect, they suggest the following social welfare function: economic welfare is enhanced whenever two conditions are met: (1) "the emigrant improves his own income" and (2) "the migrant's departure does not reduce the income of those remaining behind." If two short words are added to the specification of condition "2," the Grubel-Scott welfare rule would become a statement of necessary and sufficient conditions for Pareto optimality. Let condition "2" read: "the migrant's departure does not reduce the income of [any of] those remaining behind." Certainly, if no one in the unit at time *t* is made worse off by the emigration and anyone—including the emigrant—is made better off, emigration may be regarded as good.

The amended statement eliminates an ambiguity in the Grubel-Scott statement—since from their statement we do not know whether it is aggregate income of those left behind or each individual's income which must not be reduced. The amendment also focuses attention on the possibility—perhaps even the likelihood—that whatever happens to aggregate real income of those remaining behind, redistributions of that income can occur. I am sorry that the authors dealt so gingerly with income redistributional effects. Whether they are correct in asserting that "these effects are likely to be small enough to be safely considered negligible" needs further consideration. If, for example, the physicians who emigrate constitute a "sizable" fraction of all physicians in the "area"—as in the case of a rural town that loses one of its two or three physicians—the redistributional effects among the remaining people may be substantial. Even if the emigrants are a tiny fraction of the national supply of persons with some particular skill, they may be a large fraction for some relevant subnational region.

Thus, real income redistributional effects are one of the possible economic consequences of a brain drain. My hunch is that they are, in general, the main consequence. However, some empirical studies are needed to provide evidence as to what kinds and magnitudes of redistribution take place in response to the emigration of skilled people.

Aside from redistributional effects, emigration may bring real output effects—costs from the viewpoint of the unit from which the emigration takes place. These may be divided into short-run adjustment costs and long-run allocation

costs. I agree fully that in an analysis of the costs of emigration it is useful to separate costs of adjustment from long-run equilibrium effects. However, I see no reason to agree with the authors that the long-run effects are "of greater analytical interest." Their preference for emphasizing long-run effects reflects a normative position, not a scientific judgment. Moreover, the implication that short-run adjustment costs are quantitatively unimportant involves factual issues for which the authors cite absolutely no evidence. The short run may be long with respect to the lifetimes of the persons affected; and the short-run costs, including the costs of obtaining or training a replacement, may be large relative to the incomes of the residents, particularly if subnational units are considered, as was suggested above.

I turn now to the long-run resource allocation effects of emigration. If migration brought neither adjustment costs nor redistributional effects, there might still be adverse effects on those remaining in the origin area. The emigrant might have produced real external economies with respect to either the productivity of other inputs or the utility levels of other consumers. Here I am in agreement with the authors, except that they considered only the effects of emigration on production functions, not on utility functions.

However, Grubel-Scott and I differ with respect to their second category of losses: those associated with government services. This type of loss does not involve resource allocation inefficiencies. Rather, it involves income distributional matters. If an emigrant paid more for public services than the marginal cost of what he consumed, then his emigration would shift—that is, redistribute—more of the tax burden to other taxpayers. Since those services that are provided publicly tend to be those for which marginal cost is less than average cost, taxes will tend to exceed marginal costs. Thus, net emigration is likely to increase taxes on those who remain. Whether this conclusion is more or less likely for the highly skilled—who tend to have above average incomes—is another factual matter remaining to be explored. But I suggest that the relevant concept is the marginal cost of the public services, not the benefits (value) to the emigrants, as Grubel and Scott maintain. In any event, the authors' hunches about the magnitudes of these variables or the magnitudes of real externalities are not good substitutes for empirical research, as they surely would agree—particularly since the context of international migration necessitates judgments about these matters for various specific nations!

The entire Grubel-Scott analysis has dealt with the question whether any given nation should welcome emigration, even of highly skilled people. Their concluding sentence answers the question by urging "free movement of human capital throughout the world." I suggest that this conclusion does not follow logically from their analysis, even granting their judgments about empirical data. The reason is that while they have asked whether an origin nation should welcome emigration, they have not asked whether the destination nation should welcome immigration. Neither have they asked whether third parties have any stake in migration. I do not quarrel with a decision to separate these questions for analytic purposes. Yet the policy conclusion that the authors reach should await some consideration of these other aspects of the total,

general equilibrium problem—aspects which the authors do not even mention. To illustrate, the attitude of an origin nation toward emigration may depend in part on the level of external technological economies being generated by the prospective emigrant. The authors recognize this. But the attitude of people in the destination country would depend on the level of externalities that they would reap. And from the global point of view it may be the difference between the two levels of externalities that is relevant (among other things) for determining whether migration would represent an efficient (Pareto optimal) reallocation of resources.

A second illustration. Assuming no technological externalities at all, if there were international deviations among the ratios of a given man's wage to his marginal value productivity, then unrestricted migration would tend to be inefficient. International differences in tax structures might easily produce such deviations.

In concluding I would say that even if Grubel and Scott have jumped too quickly to policy recommendations concerning international mobility, they have surely left us with an improved understanding of the economic issues relevant to making such recommendations intelligently.

HARRY G. JOHNSON: The three papers presented in this session were commissioned in accordance with the theme of this year's meetings—the production and distribution of knowledge—and were intended in particular to illustrate how the problems of knowledge are being tackled, and the new concepts centering on human capital are being applied in current empirical work in the field of international trade. In my judgment, the authors have fulfilled this function admirably, even though in two out of three cases the urge to theorize has proven irresistible. The three papers deal with quite different aspects of the international commerce in knowledge: Keesing with the role of differences in knowledge embodied in human beings as a determinant of international trade, and with trade as a means whereby knowledge accumulated in one country is indirectly made available to other countries; Baranson with the nature of the problems of transplanting industrial knowledge to new national environments and the inherent and policy-created obstacles to such transplantation; and Grubel and Scott with the international exchange of knowledge in a form not recorded in balance-of-payments and international-indebtedness accounts, the international migration of knowledgeable persons. Nevertheless, the three papers complement one another admirably, both in subject matter and, more interestingly, in method of approach: Keesing has explored a novel dimension of the long-standing Heckscher-Ohlin approach; Baranson has used the approach of the industrial organization expert to illuminate a variety of problems in the international flow of capital slurred over by the macroeconomic approach of international trade theory; and Grubel and Scott have applied capital theory, welfare economics, and the theory of public finance to clarify an extremely muddled contemporary policy issue—the so-called problem of "brain drain." Considered together, these papers both reflect substantial progress in our knowledge of the role of knowledge in international eco-

nomics and indicate a variety of avenues for further exploration as well as integration.

Keesing has really presented two papers: a theoretical justification for the basic assumption of his research, that skill differences constitute a persistent source of differences in comparative advantage and consequently of stable patterns of trade, and a report of empirical results additional to those he has recently published. The theoretical paper seems to me to pose a spurious question and to attempt to answer it with models whose assumptions are too simple and static and whose application is insufficiently rigorous to be very illuminating. There is overwhelming evidence that trade patterns do shift over time, albeit slowly; and Keesing's empirical work is concerned with the reasons for trade at a particular point of historical time and does not depend for its usefulness on the assumption that the trade pattern at that point of time is historically stable. Further, if one wants to account for the sustained dominance of certain national industries in international trade, it would seem to me more enlightening to appeal to other aspects of industrial knowledge than the human capital involved in skills, specifically to the complementarities among technology, material capital, and skill, to economies of scale including the input-output interrelationships and specialization of a large industrial complex analyzed by Baranson, and to competition through the development of new and improved products. As to the models, it seems to me that a minimum of three factors—unskilled labor, skilled labor, and capital—is required, and probably a fourth—natural resources—should be added; and I would myself attack the problem in terms of stages in a general dynamic process of capital accumulation, on the lines of Bensusan-Butt's model of trade and accumulation rather than in terms of the long-run static equilibrium implied by the somewhat ambiguous notion of time-preference. Keesing's use of this concept I find rather elusive and inconsistent with his assumptions: for his basic model implies equalization of the interest rates prevailing in the different countries, so that one must interpret "differences in time preference" as differences in the quantities of human capital per head that different nations would accumulate at the world interest rate. Finally, I am puzzled by his remarks on policies to increase national stocks of skills: the wage-raising effects of diverting engineers from production to training would seem to be "of the second order of smalls"; and it does not necessarily follow that immigration of skills is the only early solution for skill-poor countries, since they could concentrate their existing skilled people on training and rely on imports for skill products, if they arranged to increase saving to finance the educational investment they would be undertaking.

Turning to the empirical findings, these certainly confirm the importance of skill differences in the determination of trade patterns, at least for the United States. (Keesing himself emphasizes the dependence of the whole exercise on the crucial and empirically unverified assumption that American input coefficients characterize the production processes of other countries.) I am, however, somewhat mystified about the relevance of his final computation, which measures the net flow of resources, in the form of services of the various

skills embodied in goods traded, from the industrial to the primary-producing nations. This net flow presumably is accounted for partly by the exchange of manufactures for primary products and partly by net capital outflows, as well as by the differences in the natural resource contents of products traded that Keesing mentions. Apart from the capital movements, which raise some difficult conceptual problems, what is involved is a fair exchange on commercial terms of the services of skilled people for natural resource products and the services of unskilled people. It is technically correct to regard it as a trade flow; but to describe it as a "net flow" may convey the misleading impression that it somehow represents a special contribution by the developed to the less developed countries rather than a payment for what the latter provide. I think that it would have been more enlightening, and more scientifically objective, to conceive of trade as an exchange of the services of factors—skilled labor, unskilled labor, and natural resources—for factor services and assets, and to measure the net exports and imports of each involved in the trade between industrial and primary-producing countries.

I shall be briefer, and less critical, in my comments on the other two papers, since these have each benefited by receipt of a net flow of skilled discussant's services.

Baranson's paper underlines by detailed analysis the importance to comparative advantage of the professional, technical, and managerial skills whose influence at the macroeconomic level has been demonstrated statistically by Keesing; on the other hand, he calls attention to aspects of industrial competitiveness, especially the competence of suppliers of materials and parts, that have been largely ignored by conventional trade theory. His findings lend additional empirical support to the growing belief among development experts that the traditional approach to development policy, through import substitution based on the infant-industry argument, is wasteful and ineffective and needs to be supplanted by an export-oriented, comparative-advantage-based approach to industrialization. On the other hand, he fails to notice an obvious conclusion from his findings: that the international diesel and automotive manufacturing companies which provide most of his evidence have apparently behaved in a highly irrational and nonprofit-maximizing way, perhaps because they too have been the victims of an empirically unsupported belief in the infant-industry argument. An important implication for policy is that current proposals for providing tax incentives for direct investment in less developed countries need more careful consideration than they have yet received, and more generally that current efforts to curtail private direct foreign investment may not involve the loss of real national income that free enterprise theory would attribute to them.

The Grubel-Scott paper, though it presents some interesting statistics, is essentially theoretical, concerned with the welfare effects of migration of skills. As such it complements Keesing's paper, which touches on the effects of migration only briefly, though suggestively. Grubel and Scott attack, quite correctly in my view, the emotive nationalistic nonsense that motivates current concerns about brain drain. Much of their analysis has wider application to

the policies responsible for the phenomena they have studied empirically, especially to the policy of subsidizing the training of foreign students in the United States; I would have liked them to discuss these theoretical issues as well. While I agree with their general analysis, I should like to quibble with one part of their argument: the contention that there is no "debt to society" involved in publicly-financed education. I admit that in my own treatment of the subject, which they are too polite to mention, I was misled by an excessively individualistic approach into conceding too much to the "debt to society" notion; but I am not convinced that reference to the on-going nature of society suffices to dispose of the argument. Consider, for example, a society in which the retired are supposed to be supported by tax-financed pensions. Surely the emigration of the young would involve a loss to society in that case.

ANTITRUST AND PATENT LAWS: EFFECTS ON INNOVATION

ANNIVERSARIES OF THE PATENT AND SHERMAN ACTS: COMPETITIVE POLICIES AND LIMITED MONOPOLIES

By Mark S. Massel
Brookings Institution

The observance of these anniversaries can provide the occasion to pay slavish respect to the founding fathers who passed the Patent Act in 1790, to the farsighted men who set a competitive course for our economy by passing the Sherman Act in 1890, and to their legislative accomplishments. This mood can lead to impressive eulogies about the happy combination of a competitive policy and a limited monopoly—a combination which encouraged innovation and stimulated the remarkable growth of the economy of the United States.

However, the joint anniversaries should call for something more constructive than eulogies, which after all are meant for what is dead. They call for a serious review of the current situation and of the pressing need for a policy reevaluation. Fundamental changes in the nature and shape of our economy since these statutory enactments require a careful stock-taking of our current policy needs, of how effectively the two acts fill these needs, and of the adequacy of the present institutional framework for growth and innovation.

The changes in our economy since these enactments are dramatic. The shift from a small country, primarily an agricultural one, to an economy of our present size—in geography, population, and output—has created a difference in kind. The increase in size was accompanied by several other strategic developments: the growing importance of international trade and of our interest in the welfare of other countries; the revolutionary alterations in the nature of invention and innovation; the burgeoning role of government in research and development; and the surprising proliferation of government activities in the economy through regulation, purchases, and fiscal operations.

These changes have created problems which are much more complex than those which confronted the drafters of the Patent and Sherman Acts. Theirs were relatively simple tasks. Basically, the enactment of the Patent Act in 1790 was a codification of a well-known device which had been applied in England and the colonies. Its major contribution was the establishment of a patent system on the federal level, one

which was provided for in the Constitution. The Sherman Act of 1890 was derived from existing common law principles and was passed in response to pressures to alleviate a fairly direct industrial problem. Indeed, there is evidence that the Congress did not envisage the far-reaching implications of the statute. Neither act required the consideration of problems which were as serious or complicated as those which we must face today.

Changes in Conditions

The complexities of today's public policy problems could not be visualized in 1790 and 1890. A realization of the profound interdependence of the various sectors of the economy had only begun to emerge at the end of the nineteenth century. There seemed to be little need to consider those governmental functions which could stimulate and support the operations of a relatively free market.

In 1790 the Jeffersonian philosophy of laissez faire, ably supported by Adam Smith's intellectual contribution, produced the tenet that the government which governs least will govern best. Although this attitude affected federal activities only, it dominated the political orientation of the day.

Therefore, the grant of a patent right was regarded merely as a logical extension of conventional rights in industrial property. There existed no need to devote serious consideration to the needs of the economy, to federal stimulation of invention, or to the relations between patents and competition. Nor was there any concern about the relations between private patent rights and government-financed development.

In 1890 public consideration of the antitrust problem rose out of the Granger movement. Farmers were concerned about the influences of the big trusts on prices they received and the prices they paid. They pushed for the enactment of the Interstate Commerce Act of 1887 to regulate railroad rates and the Sherman Act. In line with the spirit of the 1880's, the legislative objectives were confined to limitations on the power of the railroads and the trusts.

The objective of the Sherman Act was direct: to break up the trusts. Its passage was not wrapped in concern about the implications of the emerging industrial economy. Trust-busting merely reflected the farmers' protest against the concentrated industrial power which affected them as producers. The Sherman Act was intended to eliminate abuses in a simple agrarian economy. It constituted only a minor departure from the Jeffersonian thinking. Indeed, it bore the imprint of laissez faire—if the industrial combinations were destroyed, competition would produce such equitable results that regulation could be avoided.

Against this background, economic and technological developments have forced great changes in the requirements of public policies affecting patents and competition. The tremendous growth of interdependence in the economy has driven us into a proliferation of federal activities in the last several decades. Interdependence affects all elements in the economy: automobile production in Detroit affects fruit farmers in California; total wages paid have an influence on corporate profits and investment; prices and wage levels in key industries affect inflationary pressures; government expenditures and budget balances have a bearing on the level of industrial activity and farm prices.

At the same time, the process of innovation has undergone profound changes in important industrial sectors. Such fields as atomic energy and space have required organizational frameworks and expenditures which surpass the capacities of any industrial complexes. Instead of an inventive process which depends on the work of others, many current developments require coordinated progress on many fronts. The success of the Wright brothers rested on the invention of an internal combustion engine which generated enough power relative to its weight. In contrast, the success of the Gemini program required a planned group of many projects in metallurgy, propellants, electronics, computers, and medical science. Such balanced progress must depend on the resources of the federal government.

In response to the profound changes in our economy and technology during the last several decades, there has developed a pattern of federal activities which cannot be reconciled with the relative simplicity of the problems which affected congressional action in 1790 and 1890. The first antitrust statute, passed in 1890, did not provide for an enforcement agency. Today we find important antitrust provisions in about forty-five statutes (not including amendments) and over twenty federal agencies with enforcement functions. The drafters of the Sherman Act believed that they were striking a blow for laissez faire. Today, that Act and its associated legislation supply the underpinning for a substantial regulatory process. Antitrust provides a regulatory framework for pricing, methods of distribution, types of investment, and patent protection.

Related to general antitrust problems are the many types of direct regulation. The first substantial program of direct industrial regulation by the federal government was provided in the Interstate Commerce Act in 1897. Today federal agencies regulate, in some way, the interstate and foreign activities of: railroads; truck lines; water transportation; airlines; electric power companies; natural gas companies; pipelines; telephone, telegraph, radio, and television communication; various types of drug and food producers and distributors; banks; se-

curity exchanges; commodity exchanges; and stockyards. In addition, the federal agencies supervise aspects of wage rates, hours of work, minimum standards of foods and drugs, and labels of hazardous substances and textiles. The federal departments exercise a major influence on the uses of many natural resources (timber, water, airways, and various minerals) through their regulations, promotional activities, the construction and operation of dams and navigable aids, ownership and operation of land areas, and subsidy programs. Through direct controls, subsidies, and purchases the government regulates many types of agricultural production. Through its numerous research and development activities and its vast purchases, the federal government has a profound effect on the developing structure of such important industries as atomic energy, space, computers, and electronics. Coincidentally, many of these purchases exert profound regulatory pressures.

These far-reaching changes in the economy and in the activities of the federal government create the need for two major areas of review affecting the acts whose passage we are celebrating: the influence of regulatory and other governmental activities on innovation and growth, and the international implications of patents and antitrust.

Growth and Innovation

A basic set of issues affecting the Patent and Sherman Acts concerns growth and innovation. We need far-reaching empirical analyses of many current policies. Despite a plethora of studies, we have not yet found the strategic elements in the definitions of these issues. Nor have we advanced much beyond theological doctrine in most of the public discussions of these problems.

We need analysis which combines a broader perspective and more empirical content than may be found in most of the discussions. Competitive problems encompass a broader area than the antitrust laws. They are affected by many forms of regulation and by patents. At the same time, growth and innovation are influenced by direct federal purchases and research as well as the market framework, which is, in turn, partly dependent on government activities. Yet, we have had comparatively few field investigations of these influences.

Continuing progress of invention and innovation of products, production methods, and distribution seems to depend on a balance between competition and the limited monopoly of the patent system. How close are we to that balance, and what is needed to improve it?

There are many signs that innovation is stimulated by competitive pressures. For example, the competition of foreign-made machine tools has compelled our domestic industry to institute major cost-reduction

programs—programs which inevitably breed innovation. Some estimate the possibility of reducing costs in the industry by 30 percent, and one company plans to effectuate a 37 percent reduction. What are the competitive effects of government regulation (including antitrust) and of government purchases, research, and development? Do the Robinson-Patman Act and other regulatory activities retard innovation in marketing? Are our barriers against imports reducing the innovative effects of foreign competition?

The influence of the patent system itself is unclear. Today we have several patent systems in the United States. While one government office issues letters patent, the enforcement of patent rights is administered by the courts. Yet, the lack of uniformity in judicial interpretation of patents has created not one but a number of systems. An illustration of this diversity may be found in the rulings of two federal judicial circuits. During the period 1948 to 1954, the Fourth Circuit upheld 37 percent of the patents that were litigated. In contrast, the Second Circuit upheld 2.6 percent of the patents it considered.

The federal government's tremendous involvement in research and development has produced a new set of problems affecting patents on inventions produced through federally sponsored projects. There are many alternative solutions: royalty-free patent licenses for the government; dedication of the patents in the public domain; federal ownership; royalty-free licenses for the contractor; ownership by the contractor; federal licenses issued to one company or to all. Many public debates have been heard. Yet none has been based on substantial empirical analysis. Indeed, it seems fair to say that the discussion has been notable for considerable cant about patent tradition, property rights, and the "public interest" with little analytical substance. Comparatively little attention has been devoted to an objective, empirical evaluation of these policy alternatives on growth and innovation.

The federal patent problem has been complicated by the differences in the policies of various government agencies. Policy uniformity is needed. Such uniformity does not imply rigidity. It seems reasonable to assume that an analytical solution would include a set of criteria for determining which rule should be applied in individual situations.

The great increase in the number of patents issued has created a new problem affecting innovation: how to make the vast body of knowledge produced by the patent system available to researchers and developers. An underlying justification for the patent system is that it provides a monopoly right in exchange for the public promulgation of new knowledge. Yet, there are signs that the knowledge-producing element of the system is not very effective. Examiners in the Patent Office, applicants for patents, and their competitors have to engage in laborious, uncertain research to test the novelty of the "invention."

The description of the new feature in a patent application may be calculated to conceal essential elements affecting workability. After the patent has been issued, there is no effective mechanism for transmitting information about the new addition to the body of knowledge to inventors, scientists, engineers, and enterprises. In this respect, the inputs of the patent system into the process of innovation are clearly limited. As the number of patents is increased further, this deficiency will become a more serious obstacle to potential innovation.

International Implications

Our increasing involvement in international trade and in the economic welfare of other countries adds a new dimension to the public policies which affect patents and competition. Both sets of policies influence free trade in an important way. Both sets can exert significant influences on international economic integration and on the further development of many countries.

Despite any considerable progress in reducing tariffs and eliminating quota systems, international trade may be held back by effective nontariff barriers. Prominent among these barriers are national patent systems and restrictive business practices.

National patent systems, as well as trademarks, provide an effective mechanism for implementing tacit agreements to maintain exclusive markets. The owner or exclusive licensee of a United States patent can prevent the imports of the patented product; his German counterpart can keep out the United States goods, as well as the French product.

Similar results may obtain through international business agreements to restrict output, prices, or markets. Such arrangements may reduce imports and exports. They may serve to raise prices collected by the members of the cartel or to reduce the prices they pay for materials. They may be employed to restrict product and process innovation in order to protect the values of fixed plant and equipment or to safeguard market positions.

Regardless of which methods are employed to restrict the flow of international trade, they can suppress potential innovation either by direct control or by eliminating competitive pressures. A free movement of imports can compel producers to seek ways and means of reducing costs in order to meet import prices. The limitation on imports permits the continuation of domestic production methods without seeking greater efficiency and lower costs.

The growing international trade, together with the development of transnational corporations, has produced an interesting challenge to the patent system. Almost one-fourth of the many patent filings in the United States are made by foreign companies. In turn, our enterprises filed more than 83,000 applications in other countries in 1963, ac-

counting for almost one-third of their total filings. These cross-filings compel considerable duplication of work in patent searches and procedures. They demonstrate the impressive potential uses of patents to restrict international trade and the increasing requirement for a more effective system of information retrieval and promulgation.

Policy Needs

The twin anniversaries should be an occasion for serious analysis of present and future problems. Their solutions require a program of research and of action. In this activity several steps are indicated.

1. Extended research into the effect of our patent system and government regulation (including antitrust) on growth and innovation.

2. Empirical analysis of patents which cover the results of government-sponsored research and development. This work should help to develop principles which might be followed in order to stimulate innovation.

3. The development of improved systems of information retrieval and promulgation in order to promote more effective use of the new knowledge created by patent applications.

4. The revision of our judicial treatment of patents in order to create one patent system for the entire United States.

5. The development of an international patent system with one set of criteria for patentability, a central or coordinated administration, and a coordinated juridical system. Pending the organization of such a system, we need further coordination and harmonization of the national systems in effect.

6. An international program for the treatment of restrictive business practices. Such a program would require improved coordination of statutes, interpretations, exchanges of information, and investigations. It would call for special attention to international restrictions which are beyond the scope of the existing national authorities.

7. A continuing federal program of policy research and coordination covering government regulatory and related activities which impinge on competition and innovation.

These programs, it should be noted, call for effective interdisciplinary activity. They require a coordinated application of economic analysis, legal research, political science, and natural science. They call for research, product innovation, and policy development.

In brief, the passage of periods of 175 years and of 75 years in a world of quick transitions calls for a fresh look at old policies.

THE JOINT EFFECT OF ANTITRUST AND PATENT LAWS UPON INNOVATION*

By Jesse W. Markham
Princeton and Harvard Universities

I. *The Problem*

Quantitative analysis has now been in its ascendancy in economics for several decades. It would therefore be highly appropriate on this occasion to marshall conclusive factual evidence showing the measurable impact United States antitrust and patent laws have had on innovation. The reasons why this approach can be pursued on altogether too limited a scale are painfully apparent, but perhaps they merit brief mention.

Innovation, or, more precisely, the economic value of innovations, is not readily measurable. Even for simple cost-reducing innovations the problem of measurement has not been satisfactorily solved, and anyone who has followed the growing (and imposing) attempts at measuring quality-improving innovations is still left with that nagging feeling that a 1965 and a 1930 automobile not only perform the same functions differently, they also perform different functions. A comparison between the new and the old often is in part a matter of more or less, but in larger measure it is a matter of incomparable differences. And for truly new products there is no appropriate analogy to be found in the preceding time period.

However, this is only the beginning of the problem. Innovation as used in economics is dependent in no small measure on invention in a scientific sense. Scientific discoveries are obviously determined in part, possibly in large part, by the scientific effort devoted to inventive activity. But it is not equally as apparent that inventive effort is entirely, or even largely, determined by the economic incentives incorporated in social policies designed to encourage technical progress. In fact, as Professor Schmookler argued persuasively at the Seventy-seventh Annual Meeting of the American Economic Association one year ago, the linkage between "the economy" and technological change has received very little attention because for the most part several gen-

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erations of economists have regarded technological change as an exogenous variable [12, p. 335]. While Schmookler goes on to argue that recently developed evidence indicates that the endogenous component in technological change is probably dominant in modern economies, the parametric value representing the degree of dominance has not yet been computed, nor is it likely to be computed in the forseeable future. And even if it were, the linkage between technological change and the patent system and antitrust policy—only two of the several environmental factors affecting it—would still have to be determined. Again, the prospects of establishing these linkages in precise terms seem remote [5]. In short, it may be stated that innovational activity is a function of certain variables, some specified and some as yet unspecified, but the parameters of this equation are unknown. For this reason the analysis that follows of necessity leans heavily upon a priori postulations which, while they may be examined for consistency with certain historical facts, can be neither verified nor disproved.

Since so much has been written on the broad subject of public policy and innovational activity, it should be emphasized that this paper is concerned with the joint effects of the patent and antitrust laws. Each set of laws unquestionably also has its independent effects, although exactly what these effects are may not be apparent. For example, antitrust policy has surely prevented firms from attaining the size they otherwise might obtain, but has nevertheless permitted the creation of very large firms. If there is a relationship between firm size and innovational activity [9], antitrust policy has affected the level of such activity through its effect on the size of the firm. Similarly, since patent systems have existed for many years in societies that have virtually no antimonopoly policy, there is a presumption that the patent incentive also exerts its own independent effects.

II. *The Hypothesis*

We start then by hypothesizing what the joint effect of our antitrust policy and patent laws on innovation logically should be. The problem may be broken down into three components: (1) are the effects of the two public policies on innovation complementary or offsetting, (2) is their total (or net) effect on innovation positive or negative, and (3) if the net effect on innovation is positive, is the contribution of the policy-induced innovations to public welfare greater or less than the price society pays for them in the form of inefficient resource allocation resulting from the monopoly the patent system legalizes?

While I have not examined all—or even a large fraction—of the literature concerned with these three components, I rather suspect that most of it weakly subscribes to the view that the patent system, in le-

galizing certain forms of monopoly, is a logical incongruity in an economic system generally dedicated to the proposition of maintaining effective competition through our antitrust policy [cf. 11, p. 57]. It would follow from this that in the absence of the relevant quantitative data, the net effect of patent and antitrust policies on welfare-improving innovations is speculative. The inconclusiveness of the received opinions on this issue, however, stems in large part from our failure to have distinguished between the operative mechanics of static welfare models and dynamic processes of the economic system.

To return to the first of the three components of the problem enumerated above, it can be persuasively argued a priori that the patent laws and antitrust policy interact to stimulate the rate of innovation; that is, the two policies reenforce rather than offset each other. This follows from the simple and presumably valid assumption that in a market economy business firms willingly incur costs to establish a monopoly position commensurate with the difference between anticipated monopoly and competitive profits. A retrospective view of the long and varied history of monopolistic practices in Western-type market economies surely confirms the validity of the assumption that there is a high propensity to monopolize. Even in the United States, where the most flagrant forms of monopoly and monopolistic practices have been proscribed by law, business firms have pursued the prize of monopoly gains in a wide variety of ways. Presumably had there been no legal restraints of any kind imposed on the means firms employed for obtaining such gains, the variety would have been even more impressive.

It should follow logically from this that as certain means of seeking the rewards of monopoly are foreclosed by law, there will be a shift in efforts to monopolize from those now illegal to the remaining legal means. In a society generally conceded to be law-abiding this shift would be expected simply out of considerations of morality. But even if the "morality effect" were ignored, the economic incentives alone would assure a shift from the illegal to the legal means of monopoly. As most manufacturers now must know, when a given means toward monopoly reward is transferred by appropriate statutes from the legal to the illegal classification, the costs of using it are increased. In fact, if all the possible routes toward monopoly reward except one were effectively blocked by legal barriers, the single remaining open route should be traveled much more heavily than were all routes left open.

In the United States the patent laws, at least in theory, have for 175 years made invention and its attendant innovational activity an open road to monopoly gain; contrariwise, antitrust statutes enacted over the past 75 years have gradually foreclosed most of the remaining more obvious means for attaining such gains. Accordingly, it can be rea-

soned a priori that the two policies have reenforced each other in stimulating innovational activity. There is one possible exception to this generalization: the mass of innumerable incremental product and process changes that individually do not constitute a sufficient break with the prior art to be patented. To the extent it is known beforehand such inventive effort is not likely to produce patentable inventions, it is not directly attributable to the patent system. On the other hand, when these incremental changes are a prelude to, or a "fall-out" effect of, more significant inventions and innovations that are patentable, they are stimulated by the same system of incentives.

III. *The Empirical Evidence*

As pointed out earlier, neither the appropriate techniques of measurement nor the requisite data for factual verification of this proposition are available, but what crude tools and data as are available tend generally to support the hypothesis.

First, as antitrust policy in the two decades since World War II has been extended to encompass a wider range of business practices (e.g., merger through asset acquisition, joint venture, etc.) and administered against those it has traditionally encompassed with increased vigor, business firms have greatly stepped up their commitment to inventive effort. By any yardstick of measurement antitrust policy has become a more much important force on the contemporary United States scene than it once was [1] [7]. Government and private antitrust cases initiated per year are currently running slightly over five times the annual average for 1945-48 and in sharp contrast with the first 50 years of United States antitrust experience the government can virtually assume it will win any case, especially any merger case, it carries as far as the Supreme Court.

As the constraining forces of antitrust in the business community have grown in number and strength, business firms have substantially increased their outlays on research and development. Between 1945 and 1963 industry's R and D expenditures rose from $0.9 billion (0.42 percent of GNP) to over $5 billion (slightly over 1 percent of GNP).

Second, private business firms in other Western economies, operating under antimonopoly policies that constrain a great deal less and under patent policies that, in general, provide less protection to the patent monopoly, spend much less on R and D than their United States counterparts. As the data in the accompanying table show, only the United Kingdom out of all the major Western countries spends even close to as high a percentage of its GNP on R and D as the United States. The United Kingdom's antimonopoly policies since 1956 have probably been the strongest in Europe, and substantial outlays on

R and D are apparently of very recent origin [3]. Similarly, in other Western countries the lag in R and D expenditures behind the United States was more pronounced in the early than in the late 1950's. It may be of some significance that as anticartel policies mushroomed in Europe at the national and supranational level in the late 1950's, European business firms stepped up their expenditures on R and D.

RESEARCH AND DEVELOPMENT EXPENDITURES* RELATIVE TO GROSS NATIONAL PRODUCT AND PER INHABITANT, VARIOUS COUNTRIES

COUNTRY	YEAR	R AND D EXPENDITURE	
		% GNP	$ Per Capita
U.S.	1960–61	2.8	78.4
U.S.S.R.	1960	2.3 (?)	36.4
U.K.	1958–59	2.5	26.0
Sweden	1959	1.8	24.3
Canada	1960	1.2	21.9
West Germany	1959	1.4	15.7
France	1961	1.3	15.2
Norway	1960	0.7	10.0
Australia	1960–61	0.6	8.9
Japan	1960–61	1.6	6.2
New Zealand	1961–62	0.3	5.3
Poland	1960	0.9 (?)	5.3 (?)
Yugoslavia	1960	0.7	1.4

* Data are for both public and private outlays on R and D. The difference between the United States and other Western countries would probably be more pronounced were only R and D outlays of private firms included.

SOURCE: Adapted from Stevan Dedijer, "Measuring the Growth of Science," *Science*, Nov., 1962, p. 783.

Finally, there is some scattered evidence that individual large corporations, in the face of the high probability that any acquisition they may make would be declared illegal if challenged by the government, have begun to substitute internal for external means of growth. Generally, higher R and D expenditures precede internal expansion than external expansion.

Obviously, none of these global data actually verify the hypothesis that United States antitrust and patent policies promote innovation. The postwar R and D take-off might be explained on other grounds. Government-financed R and D outlays, which presumably operate outside the prod of antitrust and the profits incentives provided by the patent system, have increased at a more rapid rate than those of private industry; World War II might simply have ushered in the "research revolution." And the business environment of the United States can be distinguished from that of other Western economies on grounds other than public policies toward monopolistic practices and patents. Moreover, a comparison between industrial R and D in the United

States and European economies is not entirely a matter of relatively more or less, but also one of perceptible difference in kind [13]. Nevertheless, the data are consistent with the hypothesis.

The essential issue, however, may not be whether the two policies complement each other in promoting innovation, but whether they perform this function in ways consistent with the optimum allocation of resources. If their joint effect is to stimulate more innovational activity, they presumably do so by stimulating the creation of more patentable inventions and, in turn, more legal 17-year monopolies. In terms of the familiar "ideal" output analysis of welfare economics, the effect of successful innovational activity is to create the technical possibilities of producing greater outputs from the given available stock of resources; it moves the transformation curve $T_1 - T_1'$ outward from the origin to $T_2 - T_2'$ (see accompanying figure). However, if the innovations rest on patented inventions, the resulting output will not be priced at marginal cost, thereby injecting an element
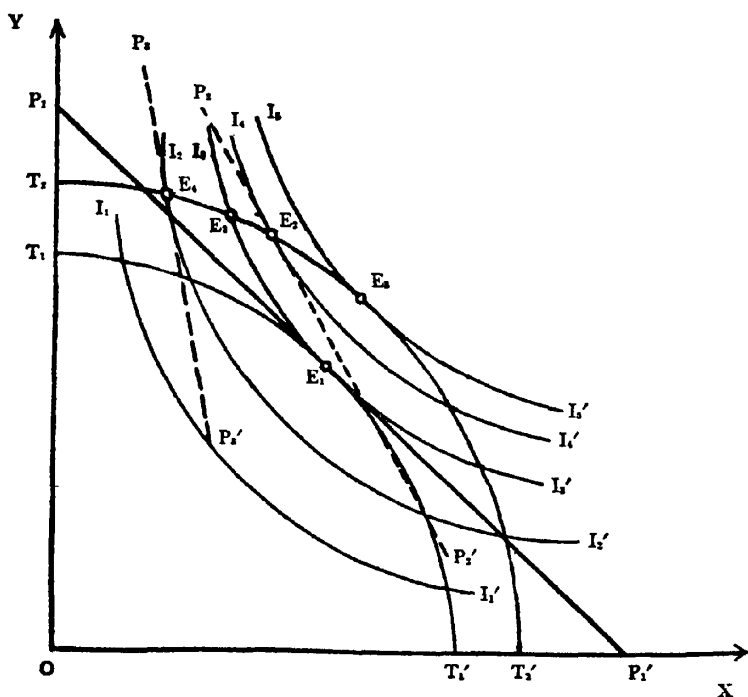
FIGURE 1

of economic inefficiency into the system; i.e., an "ideal" output E_1 satisfying the condition

$$\frac{MU_x}{MU_y} = \frac{P_x}{P_y} = \frac{MC_x}{MC_y}$$

(respectively the marginal utilities, prices and marginal costs of x and y) will not be produced. If, after the innovations occurred, the resulting price ratios were as shown graphically by $P_2 — P_2'$ the equilibrium output would be E_2, not an "ideal" output but a "better" output than E_1, because society has been elevated to a higher indifference curve (from $I_3 — I_3'$ to $I_4 — I_4'$). However, if the price-distorting effect were to lead to an equilibrium output anywhere on $T_2 — T_2'$ to the left of E_3, say E_4, the price-distorting effects of the monopoly the innovations created would, in the short run, more than offset the contributions they made to efficiency. In time the monopoly protection afforded by the patent would erode, the price (of x in this case) decline, and the equilibrium output move toward E_2 along the path $T_2 — T_2'$.

The foregoing analysis is of course more straightforwardly applicable to cost-reducing (process) innovations than to product-improving and new product innovations. Improvements in the quality of existing products can be viewed as fundamentally no different from increases in quantity, and accordingly can also be represented by an outward shift in the transformation function. However, in the absence of unambiguous quality indexes [4], the magnitude of the shift is indeterminate. Innovations leading to entirely new products present even greater difficulties, and in the case of both improved and new products the contours of the community's utility surface are altered, making the results based on comparisons with the pre-innovation situation highly questionable.

The overall inadequacies of such analytical tools of comparative statistics to deal effectively with such dynamic phenomena as innovations and alterations in the community's preferences are all too evident. However, they do help identify activities according to whether they presumably contribute to public welfare. It is difficult to visualize many cost-reducing innovations that inject significant elements of monopoly into the economy's price structure. There is a strong presumption that such innovations result in a welfare gain. The analytical tests also illustrate clearly the theory of the "second best." If the price of X contains some element of monopoly, it does not follow that society is necessarily better off by having the price of Y perfectly competitive.

This suggests that new product innovations on a wide front throughout the economy may be more likely to approach the ideal output solution than when these innovations are heavily concentrated in a few industries; forces tending to drive price-ratios away from competitive norms and to shift community preference schedules may partly neutralize each other. To illustrate, a simultaneous development of a new product X and a new product Y (see figure), both monopolized and sold at prices having the same ratio as their previous competitive prices, may lead to the ideal output E_6.

The welfare model is a way of defining how antitrust and patent policies affect innovation; it provides very little in the way of insights as to what these effects have in fact been. While the noticeable lack of conclusive factual evidence on precisely how two such time-honored and important domestic economic policies have affected the course of industrial technology may be regrettable, this factual void explains in large measure why the historical courses of the two policies have been governed by the a priori case for them. Once the legislation establishing them was enacted, no persuasive factual grounds for eliminating or substantially altering them materialized.

It seems to be generally agreed that in the hundred years that separated the first patent statute and the enactment of the Sherman Act the original safeguards that surrounded the patent grant were substantially weakened through legislative revision, judicial interpretation, and unchecked abuses of the system by private parties: Congress extended the period of exclusive use from fourteen to seventeen years in 1861; other statutory revisions over the years and actions by the courts and the Patent Office enlarged the area of patent coverage. Business firms took advantage of the liberal legal boundaries, often overstepped them [14, p. 450]. In the absence of a national policy constraining monopoly generally, these developments were not particularly anomalous; unprincipled use of a patent right to fix prices was probably no more nor no less inimical to economic welfare than simple unprincipled price fixing.

As antitrust policy slowly developed to its present strength, conflicts arose between the liberal patent system of the past and the relatively vigorous antitrust policy of the present. Increasingly, these conflicts appear to have been resolved in favor of antitrust [6]. The courts have proscribed price-fixing clauses in patent license agreements [10] and they have begun to set higher standards of patentability [2]. It was argued earlier that, a priori, a strong patent policy and a strong antitrust policy should stimulate innovation. The resolution of apparent conflicts in them in favor of antitrust, while at the same time reserving temporary monopoly for genuine improvement in the prior art,

is consistent with this argument. The courts have also tended increasingly to deny the patentee exclusive use where it results in significant monopoly power. According to one patent law and antitrust authority, between 1941 and 1957 over 100 judgments, involving more than 300 antitrust defendants, provided for compulsory licensing or outright dedication of as many as 35,000 patents [9]. The welfare effects of this development, however, are not so evident. As Professor Frederic Scherer and his associates discovered, once corporations have been subjected to patent dedication or compulsory licensing decrees they tend to patent less [10]. This result could be attributable to a reduction in their inventive and innovational effort or to their placing greater reliance on secrecy and less reliance on the patent system for protection. Since there is no evidence that the large corporations operating under the decrees have, as a group, decreased their outlays on R and D relative to all corporations, the presumption is that they have substituted some secrecy for patenting. One consequence of this is a reduction in the total stock of knowledge available to the scientific community generally and, ultimately, a reduction in the rate of technological progress. Again, it cannot be asserted with any degree of assurance whether the resulting loss to society is greater or less than the gain in the form of less monopoly.

All this leads to the rather shaky but comforting conclusion that present public policy as expressed in our antitrust and patent statutes may be characterized by a higher order of rationality than is frequently accorded it. In essence, it encourages the pursuit of monopoly reward through innovation by denying with increasing vigor pursuit of such rewards by other means. Whether present policy consists of the optimum combination of private incentives and public constraints obviously cannot be asserted with even this degree of confidence. As that wise and in recent years much too neglected teacher Alfred Marshall observed in his famous *Principles*, "The theory of Monopoly starts rather than solves practical issues such as these. . . ."

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PATENTS, POTENTIAL COMPETITION, AND TECHNICAL PROGRESS

By ALMARIN PHILLIPS
University of Pennsylvania

Introduction

Some years ago, in *Potts* v. *Coe*,[1] it was held that "where a corporation . . . seeks a patent on a discovery made in the course of its organized technical research it must assume a different burden of proof from that imposed where the discovery is the product of independent inventive genius." It was reasoned that unless patenting by corporations were restricted to discoveries "above the level of the art current in its own corporate laboratory and other corporate laboratories with which it has connections and affiliations" the patent law might "become a cloak under which a corporate group may prevent the independent use of modern technical information by obtaining patents on the step-by-step progress of scientific knowledge."

The doctrine of *Potts* v. *Coe* has not found its way into patent policy. The Patent Code of 1952 stipulates that "patentability shall not be negatived by the manner in which the invention was made."[2] It apparently precludes discrimination in the granting of patents based on whether the discovery is the predictable result of organized, commercial research, or the fruit of independent research, or a "flash of genius."[3] Neither is there basis for discrimination predicated on the extent to which one or a group of patents "prevent the independent use of modern technical information" by other firms. That is, until the patent privilege is abused by its use in price fixing, market sharing, or other monopolistic practices, its actual or potential effects on technical progress and on competition are immaterial to its validity.

I shall not suggest a specific, alternative policy in this paper. It will be argued, however, that it is impossible fully to reconcile existing patent policy with the objectives of the antitrust laws and, indeed, with the constitutionally stated purposes of patents themselves. Without assessing the effects of patents on both technical progress and the degree and type of competition in particular market circumstances and discriminating in patent issuance on the basis of such an assessment, neither competition nor technical progress will necessarily be fostered.

[1] 145 F. 2d 27(1944).
[2] 35 U.S.C.A. § 103.
[3] *Cuno Engineering Corp.* v. *Automatic Devices Corp.*, 314 U.S. 84, 91 (1941).

It is my view that a weakening of the patent rights of large corporations, especially as the patents relate to essentially trivial applications of a well-established branch of technology to their historic products, processes, and markets, would do little to hinder the "Progress of Science and useful Arts"[4] and, in some market situations, would be instrumental in promoting these ends. Further, to the extent that a weakening of the patent rights of established firms would effectively reduce barriers to entry, price and output as well as innovative behavior might result which is more consistent with efficient resource allocation.

As Professor Markham points out in his accompanying paper, there is no obvious empirical method for testing such hypotheses. The procedure here will be first to question the popular notion that technical progress in industry is dependent on the prior existence of large firms operating in oligopolistic markets. It is suggested that the more appropriate explanation is that technical progress is one among many factors which tends to bring about such a market structure. Following this, the likely effects of patents on innovations by established and by entering firms are briefly assessed.

Market Structure and Technological Change

Revolutions in basic scientific concepts, while perhaps not completely disassociated with contemporary economic circumstances, have not been closely related to narrow, supply and demand, market phenomena.[5] As the revolutions have occurred, however, and as new configurations of scientific paradigms have emerged, the continuing, "mopping-up" operations within the discipline, confined to methods and problems consistent with "the preformed and relatively inflexible box that the paradigm supplies,"[6] have created a chain of possibilities for the introduction of new products and processes.[7]

The Schumpeter of the *Theory of Economic Development* ascribed to his entrepreneur the role of perceiving of and attempting these possibilities. That Schumpeter disavowed a "great man" interpretation of his theory and distinguished between the functions of the inventor and those of the innovator is consistent with the idea that developments in

[4] Constitution of the United States, Article I, Section 8, Clause 8.

[5] "Scientific revolution" is used here in the same sense as in T. S. Kuhn, *The Structure of Scientific Revolutions* (Chicago, 1962). Kuhn restricts the term to "tradition-shattering complements to the tradition-bound activity of normal science" (p. 6) or "non-cumulative development episodes in which an older paradigm is replaced in whole or in part by an incompatible new one" (p. 91).

[6] Kuhn, *op. cit.*, p. 24.

[7] "One of the reasons why normal science seems to progress so rapidly is that its practitioners seem to concentrate on problems that only their own lack of ingenuity should keep them from solving." *Ibid.*, p. 37.

science, which are themselves largely exogenous to the market process, inevitably lead to these consequences in a capitalist society.[8] Schumpeter, however, was at this stage working within the context of a circular flow equilibrium and, implicitly, in a historical context akin to that which existed up to the late nineteenth century. As a consequence, he conceived of the new industries which arise from innovations as stemming from new firms and generating towards competitive structures and levels of performance through the "swarming" of secondary innovators and the evolution of a new, but nonetheless static, technology.[9]

When, as occurred for some industries towards the close of the nineteenth century, a changing science becomes allied to an industry's technology and remains so over a period of time, the market consequences are more similar to those described by the Schumpeter of *Capitalism, Socialism and Democracy.* The tendency towards an easy entry, deconcentrated industry is offset by other factors favoring increased concentration, barriers to entry, and long-term monopoly rewards.[10] Whether the initially successful innovation is made by an existing or by a new firm, the fact of initial success in an environment of continuously changing technical possibilities tends to make further success by those firms less difficult than is the achievement of similar success by firms which wish to swarm into the new area. Existing firms which are adversely affected by the innovation have at once fewer resources and a more pressing need to change than do the initially successful. These circumstances make it more difficult to bear the time and resource costs of learning and searching in a continuously developing area of science. They also create a "crisis" atmosphere which may not be conducive to effective applied research by scientific personnel.

A significant element in the "success breeds success" tendency arises from the contributions of the initially successful firms to the related scientific disciplines. The availability of resources internal to these firms permits, and the experience of successful innovation encourages, the

[8] J. A. Schumpeter, *The Theory of Economic Development* (Cambridge, Mass., 1934), pp. 81–94.

[9] It is impossible and unnecessary to pick a precise date after which the fusion of science and industrial technologies altered this pattern. See T. S. Kuhn, "Comment," *The Rate and Direction of Inventive Activity* (Princeton, 1962), pp. 454–55, and J. B. Conant, *Science and Common Sense* (New Haven, 1951), pp. 304–05, for a discussion of the historic change.

[10] I wish to acknowledge—without in any way implicating him in the treatment here—that it was my RAND colleague, Richard R. Nelson, who drew my attention to relationships between organized scientific disciplines and technical progress in industries. In this regard, see his "Statement" in *Economic Concentration,* Hearings before the Subcommittee on Antitrust and Monopoly of the Committee on the Judiciary, United States Senate, 89th Cong., Part 3, *Concentration, Invention and Innovation* (Washington, 1965), especially pp. 1142–1146, and *Technological Advance, Economic Growth, and Public Policy,* by Richard R. Nelson, Merton J. Peck and Edward D. Kalacheck (forthcoming), Chaps. 2 and 5. See also F. M. Scherer, "Firm Size, Market Structure, Opportunity, and the Output of Patented Inventions," *A.E.R.,* Dec., 1965.

engagement of scientific personnel in the professional activities of their own disciplines.[11] Here they may not only create opportunities for additional change but also be in a position to perceive of additional applications for scientific developments arising outside of their own research to the accomplishment of goals by their own firms. The crucial point is simply that, where knowledge of a science and its economic applications is continuously changing, with each change dependent to some degree on previous ones, there are substantial "learning costs" for any firm—new or old—which attempts to enter the field. Knowledge cannot be costlessly and timelessly transferred among firms. There is no static, generally known technology position towards which firms can easily swarm.

It is suggested, then, that a progressing science which is related to the products and processes of particular industries operates on markets in ways such that some firms tend to become larger, more profitable, and more technologically progressive while others experience increasing difficulties in remaining viable. The latter tend either to remain small, in "corners" of the market protected by various forms of product differentiation, or to disappear through mergers and failures. These results are not because "the modern industry of a few large firms" is "an almost perfect instrument for inducing technical change."[12] The results are instead because continuity in technical change made possible by a changing scientific environment is an almost perfect instrument for inducing a modern industry of a few large firms which, in turn, contribute to technology.[13]

Two related points require emphasis. First, not all oligopolistic markets with large firms are necessarily technologically progressive. Oligopoly may have its base in scale economies, resource preemption, monopolistic practices, or mergers in the context of static technologies. Further, even if the concentrated structure is the result of past technical changes, the industry need not continue to be progressive.

Second, while technical change does tend to produce concentrated industries, it is not contended that all atomistically structured industries are necessarily unprogressive. If technical advances do not create scale economies or limit entry and encourage exits because of the size of capital expenditures or the amount of learning involved in their use, an atomistic structure may persist with technological change over long periods of time. The same conditions discourage technical change origi-

[11] T. S. Kuhn, in "Comment," *loc. cit.*, refers to science and industrial technology as being "intimately entangled as they have never been before."

[12] J. K. Galbraith, *American Capitalism* (Boston, 1952), p. 91.

[13] This is purposely overstated since it represents a substantial change from the view I expressed in "Concentration, Scale and Technological Change in Selected Manufacturing Industries, 1899–1939," *J. of Ind. Econ.*, June, 1956. The present position is weakly suggested in "Market Structure, Innovation and Investment," in *Patents and Progress*, W. Alderson, V. Terpstra and S. Shapiro, eds. (Homewood, Ill., 1965).

nating internal to the industry, however, since the benefits of search undertaken by one firm tend to be spread over all firms. Hence, as Galbraith argues, whatever changes occur are likely to originate in sources external to industries of this sort.

A Crude Empirical Test

As has been noted, there is no obvious way to test empirically the validity of hypotheses of this kind. Those just advanced involve an operationally ambiguous notion of relations between the progressiveness of scientific disciplines and the products and processes of industries. In addition, they cover behavior of firms over time as well as comparisons of firm behavior at a given time for which no adequate data exist.

Despite these important limitations, the data presented in Table 1 were analyzed. The time problem can only be handled by the assumption that the structure of particular industries, at the observed point in time, incorporates the effects of recently past technological progress and of other effects on structure.

The numbers in the next to last column are indices purporting to measure the strength of the association between organized sciences and the technologies of the respective industries. They were obtained from studying the descriptions of the products primary to each 4-digit SIC industry included in the 2-digit groups. Each product was assigned a code of 1, 2, or 3, depending on an obviously subjective evaluation of the extent to which current science permits functional (as contrasted with stylistic) product changes and product differentiation among firms. The figure shown for each of the groups is the modal number for the products of the group. The results can certainly be criticized as arbitrary, but a substitute measure was not discovered.

Company financed R and D funds per hundred dollars of value added are used as the measure of the propensity of firms in each industry towards technological change. This can be viewed as the cost value of the scientific knowledge produced from private funds within the industry or as the resource expenditure involved in searching among the alternatives provided by science. Size of company is measured by value added per firm and concentration is represented by the weighted average of the ratio of the value of shipments of the four largest firms to total shipments of the 4-digit industries in each industry group.[14]

The following nomenclature is used:

R_j = company financed R and D funds per hundred dollars of value added in the jth industry ($j = 1, 2, \cdots 11$).
S_j = value added per firm in the jth industry.
C_j = concentration ratio.

[14] I am indebted to Michael Evans for the computation of these averages.

TABLE 1

COMPANY FINANCED R AND D FUNDS, FIRM SIZE, MARKET CONCENTRATION, AND PRODUCT CHANGEABILITY, 1958

Industry	Size Class of Firms*	Company Financed R and D Funds per Hundred Dollars of Value Added	Value Added per Firm (millions of dollars)	Concentration Ratio	Index of Product Changeability	Estimated Company-Financed R and D Funds per Hundred Dollar of Value Added
1. Food and kindred products..	1	0.06	0.24	—	—	.20
	2	0.47	21.98	—	—	.82
	3	1.03	125.24	—	—	.88
	Total	0.45	0.49	.33	1	.53
2. Lumber, wood products, and furniture.......	1	0.16	0.09	—	—	.10
	2	0.34	11.64	—	—	.36
	3	0.82	48.60	—	—	.44
	Total	0.21	0.11	.14	1	.22
3. Paper and allied products.......	1	0.48	0.50	—	—	.17
	2	0.86	21.01	—	—	.65
	3	1.36	396.20	—	—	1.03
	Total	0.92	1.36	.26	1	.58
4. Chemicals......	1	1.77	0.30	—	—	.63
	2	5.62	30.03	—	—	6.03
	3	6.71	217.17	—	—	8.34
	Total	5.49	1.42	.46	3	4.82
5. Petroleum refining and extraction...........	1	0.00	0.22	—	—	.41
	2	1.66	34.10	—	—	2.56
	3	3.09	334.85	—	—	3.56
	Total	2.02	0.72	.34	2	1.79
6. Primary metals.	1	0.00	0.41	—	—	.19
	2	1.09	19.02	—	—	.85
	3	1.00	269.06	—	—	1.29
	Total	0.84	2.39	.48	1	.95
7. Fabricated metal products.	1	0.22	0.23	—	—	.51
	2	1.50	17.24	—	—	1.94
	3	2.23	125.80	—	—	2.62
	Total	0.86	0.39	.27	2	1.35
8. Machinery, except electrical	1	1.25	0.19	—	—	2.36
	2	3.13	16.37	—	—	4.47
	3	7.38	116.79	—	—	6.05
	Total	3.71	0.42	.36	3	3.15
9. Electrical equipment.....	1	0.61	0.37	—	—	.77
	2	4.45	16.61	—	—	5.47
	3	7.81	221.41	—	—	8.69
	Total	5.66	1.65	.48	3	5.11
10. Aircraft and missiles	1	2.82	0.45	—	—	.00
	2	2.71	16.55	—	—	5.30
	3	5.11	230.76	—	—	9.63
	Total	4.80	5.50	.50	3	6.88
11. Instruments....	1	1.40	0.27	—	—	.95
	2	5.80	18.92	—	—	5.58
	3	7.70	144.33	—	—	7.79
	Total	5.35	.93	.48	3	4.50

* Class 1 = employment less than 1,000 per firm; class 2 = employment between 1,000 and 4,999; class 3 = employment of 5,000 or more.
SOURCE: National Science Foundation, Census of Manufactures:

P_j = index of product changeability and differentiability.
R_{ij} = company financed R and D funds per hundred dollars of value added by firms in the ith size category of the jth industry ($i = 1, 2, 3$, with categories based respectively on employment of less than 1,000, 1,000 through 4,999, and 5,000 or more).
S_{ij} = value added per firm in the ith size category in the jth industry.

The analysis was done in two steps. The first is intended to explain differences in R and D behavior among industries; the second, to explain difference within the industries. The estimating equation for the first step is:

$$(1) \qquad R_j = aS_j^b C_j^c P_j^d u,$$

where b and c are the elasticities of R and D funds with respect to interindustry differences in firm size and concentration. Because the values for P_j lack cardinal meaning, 'only the sign of d has economic significance.

Equation (1) was converted to logarithmic form and the resulting least squares estimate is:

$$(2) \qquad \log R_j = \underset{(.676)}{.201} + \underset{(.174)}{.226} \log S_j + \underset{(.554)}{.611} \log C_j + \underset{(.254)}{1.608} \log P_j$$

$$R^2 = .93$$

There are statistical problems in interpreting these results. The correlation between S_j and C_j, in their log forms, is +.81; that between S_j and P_j, +.32; between C_j and P_j, +.59. This collinearity, however, is precisely that to be expected in near-equilibrium conditions for markets operating as hypothesized. Where science permits easy product change and differentiation, the market process tends over time to cause the scale of surviving firms to be large and concentration to be high. Hence, the set of relatively high positive coefficients in the correlation matrix is entirely consistent with the theory despite the purely statistical problems it presents. Since it is not true that markets in which product change is difficult necessarily have small firms and low concentration, the correlations between S_j and P_j and between C_j and P_j are not perfect. Further, the simple correlation between P_j and R_j, again in the log form, is +.91, while that between C_j and R_j is +.80. Adding the effect of C_j to that of P_j in the explanation of R_j produces only small reductions in the proportion of unexplained variance.

Since this is at best a pilot venture, equation (2) was accepted as a good estimator of differences in industry levels of R and D performance. From it, $\hat{R}_j$ values were obtained and the second step was to fit the equation:

$$\text{(3)} \qquad \frac{R_{ij} - \hat{R}_j}{\hat{R}_j} = \alpha + \beta \log \left(\frac{S_{ij}}{S_j}\right) + \gamma \left(\frac{C_j}{P_j}\right) \log \left(\frac{S_{ij}}{S_j}\right) + v.$$

Here the attempt is to explain differences in R and D performance between the estimated industry performance and the three size groups of firms within each industry. It is anticipated that larger firms do relatively more R and D than do the smaller ones but that, as firm size increases in conjunction with increases in concentration and decreases in the ease with which products can be changed and differentiated, the larger firms will tend to perform relatively less R and D.[15] Thus, it is expected that:

$$\text{(4)} \qquad \beta > 0, \quad \gamma < 0$$

and the possibility is admitted that for high values of C_j and low values for P_j:

$$\text{(5)} \qquad -\gamma \frac{C_j}{P_j} > \beta.$$

If (5) holds, the interaction between high concentration and low product changeability "swamps" the simple size of firm effect.

Regression yielded:

$$\text{(6)} \qquad \frac{\hat{R}_{ij} - \hat{R}_j}{\hat{R}_j} = \underset{(.11)}{.49} + \underset{(.05)}{.27} \log \left(\frac{S_{ij}}{S_j}\right) - \underset{(.23)}{.20} \left(\frac{C_j}{P_j}\right) \log \left(\frac{S_{ij}}{S_j}\right).$$

$$R^2 = \ 67$$

The signs are those anticipated, but the coefficient for the interaction variable is not significantly different from zero by the conventional tests. Further, the estimated value for γ is such that, with the arbitrary P_j values adopted, C_j would have nonsensically to exceed unity before swamping could occur.

Table 1 provides the final $\hat{R}_j$ and $\hat{R}_{ij}$ values in the last column. The correlation between R_{ij} and $\hat{R}_{ij}$ is +.91. It can hardly be said, however, that the hypothesis is confirmed that it is primarily the relationship between organized scientific disciplines and the technologies of industries which explains industrial technological advance. Nonetheless, given the crudeness of the methods and data employed, it is perhaps surprising that the results come as close as they do to those hypothesized.

[15] Space limitations prohibit an explanation of the underlying theory. See O. E. Williamson, "Innovation and Market Structure," *J.P.E.*, Feb., 1965, and "Market Structure, Innovation and Investment," *loc. cit.*

Patents and Technical Progress

It is often contended that without patent protection large firms would be less inclined to research and development, less willing to innovate and make fewer contributions to science.[16] Such propositions seem incorrect when a progressing science is closely allied to the technology of an industry and largely irrelevant to technical progress—though not to competition—when no science is closely related.

If science affords continuing opportunities for firms to improve existing products, to introduce new products as substitutes or complements to existing products, or to lower costs of production, the firms can hardly fail to search the horizons of science for these opportunities except in an environment which is otherwise noncompetitive. While there are risks associated with investment in research and innovation, failure on the part of an individual firm to undertake these risks entails the more substantial risk that another will succeed. That is, in a rapidly changing technical environment, the choice not to search for new products and processes almost certainly entails declining profits and market shares if the other firms are not known to be making the same choice. Even a collective decision by all firms not to search has such an expected payoff if there are no substantial barriers to entry.

The previous arguments have indicated that a competitive market is not likely to result in the context of a rapidly changing technology. This is true with or without patents in the hands of leading firms. But the social results appear better if the patent positions of these firms are not allowed to accumulate to a point that so much of the past developments are encompassed that entry is effectively barred. Without a strong patent position for existing firms—individually or collectively—and without other, nontechnical barriers to entry, the changing scientific base will encourage the entry of new firms except as the existing firms themselves prevent it by adopting the economically preferable alternatives which science affords. The maintenance of free entry, that is, will in this context tend to induce entry-preventative technological change in a manner analogous to the inducing of entry-preventative pricing with given technologies.[17]

If it is recognized that leading firms are likely to be able to impede entry by means other than patents and technological progressiveness, a

[16] See the treatment of this point in Corwin Edwards, *Maintaining Competition* (New York, 1949), pp. 216–18. See, also, G. E. Frost, "Patent Rights and the Stimulation of Technical Change," W. Alderson, "A Marketing View of the Patent System," D. Smith, "Technological Innovation and Patents," all in *Patents and Progress*., *loc. cit.*, and F. Machlup, *An Economic Review of the Patent System*, Staff Study, Subcommittee on Patents, Trademarks and Copyright of the Committee on the Judiciary, United States Senate, 85th Cong. (Washington, 1958).

[17] The best recent illustration of this is in the pharmaceutical industry. See W. S. Comanor, "Research and Competitive Product Differentiation in the Pharmaceutical Industry in the United States," *Economica*, Nov., 1964.

corollary argument emerges for strengthening the patent position of potential entrants. The pressure which the latter can put on existing firms depends on their being able to enter if technically possible and economically preferable products and processes are not reasonably promptly adopted. A preferred patent status will not by itself remove other entry barriers, but it would at least eliminate one historically important means by which established firms have been able to control the pace of innovations for their own benefit.[18]

It follows, too, that as the change in technology relating to an industry tends to slow, the possibility of firms preventing entry by technical progressiveness tends to disappear. The technology and its applications become more generally known and the learning costs associated with entry become lower. As this occurs, the possibility of the market operating in a competitive fashion increases if the established firms have not so foreclosed the technology by a web of interrelated and successive "improvement" patents that, despite the common knowledge of technology, entry is impossible. Here, the argument that patents foster technological change is hardly applicable; their purpose and their effect are no more than to sustain established positions of market power.

Conclusions

The recasting of the relationship between technological change and the structure of markets offers a somewhat different foundation for many previously advanced arguments for questioning the social value of permitting the indiscriminant control over technology by established firms through the patent privilege. It is perfectly clear, however, that the legislative changes necessary to inaugurate a discriminatory policy would be difficult to draft. The position is defensible that until enough is known of the particular circumstances in which patents retard rather than encourage technical progress no policy changes should be made. The same argument, however, could be used to stall virtually all legislation affecting the operation of markets. For my own part, I would be willing to see some legislative experimentation reducing the patent protection afforded to large and established businesses. I suspect that such a policy would be consistent with the purposes of the antitrust laws as well as with the avowed purpose of the patent laws.

[18] See Edwards, *op. cit.*, pp. 218–35, W. Hamilton, *Patents and Free Enterprise*, T.N.E.C. Monograph No. 31; C. Wilcox, *Competition and Monopoly in American Industry*, T.N.E.C. Monograph No. 21.

DISCUSSION

CORWIN D. EDWARDS: Professor Phillips has added a provocative new hypothesis to our thinking about patents: that initial successes in a technologically changing industry become, via patents, means to further success, and thus foster oligopoly. To me the logic with which he expounds his point is more persuasive than the correlations of subjectively selected data that he offers in further support of it.

I agree with Mark Massel that we need to reexamine our patent laws in relation to the objectives of our antitrust policy. In what I shall say here, I hope to suggest certain aspects of that reexamination.

However, I shall focus my remarks chiefly upon Professor Markham's paper. By failing to distinguish sharply between scientific discovery, invention, and innovation, his paper seems to me to obscure some points that are highly relevant to his cautious approval of the interaction of patent and antitrust laws.

In discussing the patent laws, four concepts should be sharply differentiated: first, research that results in new scientific knowledge; second, invention, by which knowledge, new or old, is given a new industrial application; third, development, by which use of an invention is made sufficiently cheap and reliable to justify its commercial use; fourth, innovation, by which a new commercial activity is actually undertaken.

Inventions are patentable. Scientific discoveries and developmental activities are not. Innovations are not patentable as such; and though many of them are based upon patented inventions, others consist in unpatented and unpatentable changes in the characteristics of commodities or in business methods. Moreover, invention does not necessarily lead to innovation. Some inventions are kept out of use by their owners; some are used to block innovations by the rest of the community. A firm that has patented what it considers the best means to a desired result often undertakes further research in order to patent and keep out of use alternative means that might otherwise be developed and used by others. The desire to block others and avoid being blocked fosters invention. It may also retard innovation.

With these distinctions in mind, one can readily see that our public policy has paradoxical aspects. We tolerate no property rights in scientific discoveries, however expensively these discoveries may be attained; for we believe that scientific progress requires an unimpeded right for anyone to use whatever scientific knowledge he needs. We presume that without the incentive of property rights, scientific research will be done and scientific discoveries will be announced. Simultaneously, we grant monopoly rights to inventions in the belief that the prospect of such rights is a necessary incentive to induce people to invent and to disclose. Apparently we think that the need for this incentive ceases when the invention has been made; for the monopoly rights enjoyed under a patent that requires expensive development are no more extensive

than those enjoyed under a patent that needs no development expenditure at all. As to innovation, our patent laws ignore it entirely. Although the progress of the useful arts is the constitutional basis for patent law, we, unlike many other countries, permit a patentee to refrain both from using his patent and from allowing others to use it.

Even in a world of individual inventors and individual business proprietorships, such a policy would be anomalous enough to cast some doubt upon the theory of the patent system. Without exploring this doubt, however, I want to focus on the way the anomalies are magnified in a world of large corporations and institutionalized research. In such a world, the patent system comes to include much functionless restriction, yet fails to provide effectively for some of the obvious needs of corporate patentees.

The chief changes that have appeared seem to me to be these.

1. The government now bears a large part of the cost of corporate research and development—partly through foregone income taxes, partly through direct payments to research contractors. (To qualify for government benefits, various large firms classify as research and development, and thus get subsidy upon, expenditures that were formerly treated as operating expenses. Research probably has increased, but the distortion of the figures makes measurement of the increase impossible.)

2. Concentrated research results in great accumulations of patents in a few corporations. The accumulations become still greater because many of the patents obtained by independent inventors cannot be used by them, for lack of access to related patents and for lack of capital. Hence independent patents are often sold to some large corporation.

3. Continuously acquiring new patents, the great patent holders enjoy patent protection for periods of time much longer than the duration of a patent. Particular patents expire, but control endures through successor patents.

4. Patents are obtained and used where there was no invention. Mass processing of patent applications becomes slipshod processing; patents are improvidently granted; and by correcting these derelictions the courts invalidate a large proportion of the patents that come before them. But most of the legally questionable patents possessed by large corporations continue to afford their owners a considerable measure of control. Their validity is acknowledged without a legal contest by small firms too weak to litigate and by large firms that do not desire to subject their own weak patents to a reciprocal legal test. Thus patents reflect not only an underlying process of technological invention but also an equally ingenious process of legal invention, by which dubious patents are obtained and made effective by contract.

5. As patents proliferate, even the large patent holders are likely to need to use technology that is controlled by somebody else. Singly they possess enough patents to veto effective operation by others but face the possibility of a similar veto. If each such firm insisted upon exercising its legal right to use its own patents exclusively, the patent system would often be a source of reciprocal frustration. In such cases, which are increasingly common, pooling of patent rights becomes indispensible.

6. But though under the patent laws an individual patentee monopolizes his invention and can impose significant restrictions upon those whom he licenses to use it, patentees acting collectively have no such immunity for restriction collectively or reciprocally imposed. The pooling of patent rights that is necessary involves great risks under the antitrust laws if the pooling arrangements include commercial restrictions. Much of the incentive provided by patent monopoly is nullified by the barriers to restrictive patent exchanges.

Conditions such as these would be, presumably, the subject of the reexamination of patent policy desired by Mr. Massel. I agree that they need examination; and I also agree that this examination should include consideration not only of the incentives derived from desire for patent monopoly but also of the incentives for innovation derived from competition. I agree with Professor Phillips that in any such reexamination reduction of the power of powerful patentees should be one of the focal points of attention. But I do not think, with Professor Phillips, that every patent grant to an oligopolist should be preceded by a market study that would turn the issue of the patent into a big economico-legal case. We need to reconsider our criteria, not to make a series of elaborately studied *ad hoc* decisions.

The diversity of the possibilities for action cannot be readily perceived if one assumes, as Professor Markham appears to do, that the problem of public policy is merely to assess the pros and cons of authorizing by patent a monopoly that has characteristics similar to the monopoly of economic theory. Patent monopolies have today peculiarities of statutory origin that differentiate them from other monopolies. For example, they include unique rights to interfere with others: because he made an invention first, a patentee could invoke his patent against me even if I, never having heard of his patent nor made use of his ideas, had independently achieved the same invention by my own efforts one week later than he. Other property rights convey no similar power to prevent people from using the results of their own independent efforts.

Patents tomorrow, like patents today, will give their possessors whatever rights public policy may be willing to endorse. This means that the incentives provided by patent law could take some other form than monopoly or could provide any degree or kind of monopoly that imagination can devise and think suitable. The law could seek to foster invention or could reach further to foster all research and development expenditure. It could rely upon pecuniary grants instead of grants of monopoly rights. If it granted monopoly rights, it could curtail them in numerous ways. The following four questions indicate a few of the more obvious possibilities.

1. Should a patentee be forbidden to hold his patent out of use, or should he be required either to use it himself or to license it for use by others?

2. Should the restrictions that a patentee can impose upon others by license be limited to those that protect the patentee from competition by his licensee, or should they (as now) also include restrictions by which he protects one licensee from competition by another?

3. Should the patentee's right to exclusive use of his patent be converted into a mere right to exact reasonable compensation from other users?

4. Where all or most of the cost of an invention has been defrayed by the government, should the rights conveyed to a private patentee be correspondingly curtailed?

ALFRED E. KAHN: There is no respect in which I flatly disagree with these papers. I will confine my remarks to those respects in which I found one or another of them unconvincing.

Professor Markham attributes the "inconclusiveness of the received opinions" about the effect of our antitrust and patent laws on innovation to "our failure to have distinguished between the operative mechanics of static welfare models and dynamic processes. . . ." I cannot imagine what he can have been reading on the subject. Certainly the central question in all the modern literature is precisely whether the positive dynamic contributions of the patent system do or do not outweigh the costs involved in its departure from the static welfare ideal. (See, for example, Fritz Machlup's "The Optimum Lag of Imitation Behind Innovation," in *Festskrift til Frederick Zeuthen,* Copenhagen, 1958; and my own, "The Role of Patents," in John P. Miller, *Competition, Cartels and Their Regulation,* Amsterdam, 1962.)

His own conclusion is that our policy "may be characterized by a higher order of rationality than is frequently accorded it." This is not much of a conclusion. The reason for the modesty, he clearly points out, is that the supporting argument is essentially speculative and the evidence he cites falls far short of objective proof.

The first support he adduces is the a priori argument that if the antitrust laws close certain doors to monopoly, this should increase the flow of effort through the door that remains open: patent-protected innovation. This supposition sounds like an adaptation of the old wages-fund theory, only now it is a kind of "monopoly-effort fund." One might equally posit a tendency of firms to try at all times to protect or strengthen their market positions, by all available means, wherever they anticipate benefits will exceed costs. In that event closing one door would not increase the traffic through another. Moreover, this simple contention ignores the really important problem, which arises from the fact that an effective antitrust policy must partially close the patent-innovation door, too, by setting limits on the accumulation and use of patents.

Second, Markham's factual evidence seems to me trivial. The immense expansion of research expenditures since World War II offers no independent support for the notion that the increased activity and successes of our antitrust agencies—including their successes in striking down patents and patent practices—have made a positive contribution to innovation. It is even more difficult to take seriously his international comparisons of R and D spending as supporting the hypothesis. Differences in national antitrust or patent policies have nothing to do with the high ranking of the U.S.S.R.; with the varying portions of the total national efforts financed by governments (over one-half in the U.S.); and surely they have little or nothing to do with the markedly more intense efforts of Japan than New Zealand, Sweden than Norway and Australia, or the U.S.S.R. than Poland or Yugoslavia. Compared

with levels of GNP, education and industrialization, the influence of antitrust and patent policies could only have been slight; whether it was positive or negative these figures give not even a hint.

Finally, I find his welfare economics argument largely beside the point. I do not see by what process the potential welfare gains from a patented, cost-reducing process innovation could in fact be more than offset by the price-distorting effect of the patent monopoly, the result that his graph purports to demonstrate. Markham himself says he finds such an outcome "difficult to visualize."

To expose the circumstances in which the patent may be too high a price to pay, one must examine the system's original justification: patents, it was held, involve no net social cost because the monopoly they confer is over something society would not otherwise have had. So long as this assumption is valid, society cannot be moved to a lower indifference curve by monopolization of an innovation. The critical questions, then, concern the validity of that assumption: whether or how much later any particular innovation might have been forthcoming without the patent inducement, and whether the patent monopoly power is equivalently limited in duration and scope. It is the function of an ideal patent system and antitrust policy to assure that the innovator's remuneration (involving a static social cost) does not exceed the value of his contribution (the dynamic social benefit).

In these circumstances, the two systems of law can never be unequivocally compatible—or incompatible—or even consistent. Their interrelationships are complex; their effect, singly and in combination, and their optimum blend will vary from one market context to another—as Mark Massel implies with his historical recital, and Almarin Phillips and Markham himself explicitly point out. Antitrust reduces the value of the patent incentive; but it also helps free innovational competition from possible blockages, patent-originating and others. The patent system both contributes to the erosion of preexisting, static monopoly positions, by fostering innovation, and constructs roadblocks to competition, both static and dynamic. An ideal policy, I have argued elsewhere, would have to practice perfect monopsonistic discrimination in deciding how long and strong and broad a patent to offer for each separate innovation, and what kinds of antitrust proscriptions and remedies to apply in each situation.

Professor Phillips' proposed reform attempts just such a discrimination, and I am inclined to endorse it. But I do have some reservations about his supporting reasoning and statistical evidence.

First, against the reasons he cites for expecting a rapidly progressing technology to create progressive tendencies to market concentration and impeded entry must be weighed the greater attractions and opportunities for entry afforded by rapid growth and change. William Shepherd, Ralph Nelson, Michael Gort, and Edwin Mansfield have all found that rapid industry growth seems to be associated with declining concentration and with instability in the market positions of leading firms. The scientific knowledge acquired by the pioneers in petrochemicals, electronics, in catalytic cracking or the oxygen

conversion process did not prevent rapid entry and the erosion of dominant initial positions.

These experiences suggest the most formidable obstacle to putting Phillips' proposed reform into effect: how would one define the industries, the leading firms in which would be denied the right to patent? Would a dominant position in "chemicals" suffice to deny a firm a patent on nylon, titanium, magnesium, Delrin, Saran, or ethylene glycol antifreeze; or in petroleum refining a patent on synthetic glycol or on the Houdry catalytic cracking process; in soaps on a new synthetic detergent? In the context of a rapidly changing technology, how does one distinguish the insiders, who are to be denied patents, from potential entrants, who are still to receive them?

The only parts of Phillips' statistical evidence, as I interpret it, that bear directly on his general hypothesis are the high and moderate respective correlations he finds between concentration on the one hand and research intensity and product changeability on the other. One could hardly doubt that economies of scale in research, accumulations of patents and other advantages of a technological head start do help explain these positive relationships, as his thesis implies. On the other hand, it is noteworthy that advantages of this kind are of minor importance among the reasons for the substantial to very high entry barriers Bain found in the industries he studied. Moreover, it is my impression that in those industries in which a continuously progressing application of science does seem to be associated with comparatively high levels of concentration, it is not typically associated with a progressively widening gap between the competitive abilities of large, inside firms and potential entrants. On the contrary, a rapidly and diversely developing technology offers unusual incentives and opportunities for entry, as I have already suggested.

Finally, I am very skeptical of the hypothesis that the higher the level of concentration the less the probable difference between the research efforts of large and small firms within an industry. I find no such relationship in Phillips' data, and I do not see why it should have been expected. But those data are well worth studying; they suggest all sorts of reasons other than concentration for the great interindustry differences in the uniformity of research efforts by firms of different size.

RICHARD B. HEFLEBOWER: On the anniversaries of such major economic policies as those that provide the occasion for this program, appraising the past is useful only as a prologue to the future. I have shared Markham's "rather shaky but comforting conclusion" about the consistency of the two policies. Where they could conflict most obviously is the use of a patent as the lodestar of a collusive arrangement. But court decisions have now circumvented such behavior quite satisfactorily except for failure to void patents used in this fashion. There are plaints that large corporations' threats of patent suit deter invention and innovation by individuals and small enterprises, but clear evidence is sparse. Of more serious import is the amplification of the monopoly afforded by a basic patent, which Markham terms "incremental product and process patents" and which Phillips refers to as granting patents to corpo-

rations for "trivial new applications of a well-established branch of science to their historic products. . . ." Much of this must stem from court decisions that have weakened the law, as pointed out by Massel. Reversing these trends by legislation or by court law is called for, as has happened recently with respect to mergers and to practices under Section 3 of the Clayton Law. Little improvement will occur, however, without a marked strengthening of the staff and procedures for passing on patent applications.

Phillips presents a basic challenge to the Schumpeterian doctrine of the competitive effect of large firms' leadership in "creative destruction" from which follows, supposedly, a higher rate of technological advance than would occur in more competitively organized markets. By that process Schumpeter saw competition resulting from a "change of a state" rather than from operation of a given state of market organization. The Phillips' contention is that firms patenting major breakthroughs are able to build "patent positions" that "accumulate to a point that so much of the past developments are encompassed that entry is effectively barred" and oligopolistic markets result. In his "crude empirical test" of this hypothesis Phillips avoids the error of assuming that advances in basic science provide equal opportunities for innovation in all industrial areas. His "subjective evaluations" of this opportunity are far better than assuming equality of opportunity. To continue, his hypothesis calls for estimating the net effect of past, hardly just "recently past," technological progress. But he uses only a proxy for current intention to invent; namely, current R and D expenditures. Furthermore, this independent variable is explained in large part ($R^2 = .93$) by the current size of firms measured by value added and by the current concentration in the industry. It seems, therefore, that his finding could equally well support what Schumpeter asserted; namely, that large size of a firm and concentration encourage technological change.

In order to test Phillips' casual assertion, he must demonstrate that in the past, quite far back, the influence of major technological breakthroughs, augmented by the accumulated patents over later years, explains the current market structure to a significant degree. I doubt whether this can be done by regression analysis, for the evaluation and dating of breakthroughs would have to be done for individual situations. He could find cases in the raw material processing industries. But from what I have found for a number of consumer goods industries, patents have not been important. Where they blockaded rivals, nearly always the patent proved to be invalid. Instead, the gain of volume stemmed from the firm's capacity to perfect the product and its head start in merchandising.

Of more general importance is the analytical framework implicit in Phillips' hypothesis and his assertion that a major reduction in what can be patented (by large firms only?) would not inhibit technological advance. His emphasis is on individual firm's skill or luck in past invention rather than on initial economies of scale, the source that fits more clearly into a static framework. Furthermore, initial success provides the internal funds and types of personnel for cumulative growth. (Contrast this description with the usual assumption

that failure of prompt imitation by rivals or even by entrants not barred by patents reflects imperfections of the factor markets!) Nor would the cumulative growth of the innovator be inhibited by being barred from patenting "substitutes or complements to existing products, or to lower costs of production, the firms can hardly fail to search the horizons of science for these opportunities except in an environment which is otherwise noncompetitive." One might ask with respect to this caveat, as would Schumpeter, whether an oligopolist can count on rivals foregoing innovational opportunities and, therefore, would avoid engaging in creative destruction in an era of a "rapidly changing technical environment." Whether one agrees with Phillips or with Schumpeter, note the dynamic and historical, as distinct from static, framework within which both deal with innovation and its relations to competition.

In this session, built around the anniversaries of major economic policies, ought not we take a broader and longer historical view of our past and of what is in prospect? These institutions, in the sense of habits of thinking and their formalization into policies and organizations, are not universal but reflect underlying conditions. Clearly the degree to which we can take a comfortable view of the past organization and performance of our market system reflects the size of our domestic economy, the individualism that the earlier immigrants brought and built into our governments and their policies, the variety of alternatives in earlier decades before those who failed which enabled them often to regain income and even wealth, the lack of continuous concern about national security and external affairs, the almost continual upward trend of per capita incomes, and the favor of nature that endowed us with the richest and most varied natural resources of any land area. Rather than comment on the degree to which each of these basic conditions has been or prospectively will be altered I select three for brief consideration.

On the domestic scene, we are concerned increasingly about those who are victims of or are inept in fitting into the working of the market economy. There is an antipoverty program designed to alleviate by direct income supplements—for which the alternative of a "negative income tax" has been suggested—and assistance to individuals to acquire the education, skills, and attitudes to gain a place in the market system. This is constructive and seems to fit modern welfare theory and its compensation principle. But almost everywhere one observes group organizations designed (with governmental lack of concern or even aid) to avert adverse effects of disequilibria or to exploit in the absence thereof, all of which amplifies the defects in the working of the market system.

Of potential major importance is the adoption of a positive fiscal program to supplement or offset private action so as to provide stability around a rising trend. This promises to lessen the need for governmental direct action to mitigate the effect of defects in the working of the market system and also the drive for group protection of status. This new development in private decision making lessens uncertainty for many adjustments will not require absolute reduction of resources committed to a particular use, but will permit adjustment primarily by the use to which increments of resources are directed.

Our postwar international political and economic position in the world gives rise to major threat to our historic view of a competitive market economy. First, there is the role of our corporations as investors and traders abroad where patent and competitive policies differ from ours but can react on ours. These are the questions raised by Massel and about which we know little. The second is the development of massive government purchases but not so much for the run-of-the-mill commodities for which purchase by bid is feasible. The serious problem stems from the procedures for acquiring advanced weapons and space exploration equipment. Purchases are made, it seems necessarily, under quite different and hardly openly competitive arrangements.[1] Beyond the government as a buyer there is its role in restraining or stimulating particular areas of research, of production, and of international trade; e.g., an embargo against a particular country which minimizes individual economic decision making. All of this means that in some regards we are being "Europeanized" in the pre-World War II sense, just as the countries of that area are now being made more like us in the size of markets brought by free trade areas and, also like we were before 1940 in their distinctly lesser international political and economic roles. Of course, if the world develops as we would like, a not probable event, these concerns will lessen, but not before they have had long enduring effects on our economic institutions.

[1] See M. J. Peck and F. M. Scherer, *The Weapons Acquisition: Process: Economic Analysis*, 1962 and Scherer, *The Weapons Acquisition: Process: Economic Incentives*, 1964, both published by Harvard Graduate School of Business, Boston.

PUBLIC REGULATION: THE IMPACT OF CHANGING TECHNOLOGY

COMMUNITY ANTENNA TELEVISION SYSTEMS AND THE REGULATION OF TELEVISION BROADCASTING*

By FRANKLIN M. FISHER
Massachusetts Institute of Technology

Under existing institutional arrangements in the United States, the broadcast signal of a television station has many of the aspects of a pure public good. If a signal is broadcast over the air to a certain community, all members of that community can receive and enjoy it, provided they possess receiving sets. Further, television sets are sold under supply conditions that presumably involve the costs of their construction, but surely have nothing to do with the cost of production of the signals they receive. In general, no attempt is made to charge the latter cost directly to television viewers.

This is not to say that such costs could not be so charged in principle. Experiments with pay-TV have been made with varying degrees of success; however, there is no reason to believe that, in the absence of free television competition, a system of exclusively pay-TV would not be viable. (And, if it would not, ought the service to be provided at all?) It is technically feasible to charge viewers according to use of the service. Alternatively, a less economically efficient arrangement might be used whereby any owner of a receiving set is taxed a flat amount every year and the proceeds used to finance television broadcasting. Such a system is used in most other countries. It has the merit of financing the service by charging the users, but the defect of not discriminating between heavy and light users. Further, it imposes a single flat charge covering all programs broadcast during the year rather than different charges for different programs which are, in fact, different products. In such a system, programming decisions are made without regard to the state of demand for individual programs when individually priced. Rather they are made on the basis of the state of

* My limited knowledge of the matters here discussed was largely acquired during my preparation of testimony on behalf of the National Association of Broadcasters before the Federal Communications Commission. That testimony concerned the impact of community antenna television systems on local television broadcast stations' audience and revenue. The research is reported in detail in F. M. Fisher and V. E. Ferrall, Jr., in association with D. Belsley and B. Mitchell, "Community Antenna Television Systems and Local Television Station Audience," *Q.J.E.*, May, 1966. I am indebted to V. E. Naftalin for discussions of many of the issues here considered. The responsibility for the views here expressed is solely mine, however.

demand when all individual programs are priced at zero once the fixed charge has been paid, or on the basis of what the producing authority for other reasons thinks ought to be broadcast.[1]

The system used in the United States has all the defects of such an arrangement without the merit of charging television viewers as such for the cost of providing the programs. Television viewing is mainly financed through advertising, so that viewers are subsidized by the consumers of advertised products. In these circumstances, the choice among alternative programs is only a very indirect market choice and one which certainly fails to reflect the state of demand which would obtain if programs were priced at cost rather than at zero. In this system, the long-run costs of placing and keeping a television station on the air are not defrayed by those who use its services in their capacity as television viewers.

Even given the inefficiency of such an arrangement for reflecting consumers' tastes and production costs, it has not been the case that one could safely rely on the competition of stations for advertisers to optimize resource allocation subject to the limitations of that arrangement. Nor does that competition obviously produce those programs which might in some sense be said to be in the best public interest. This is the case in part because of the limitations on entry into local television markets which have until fairly recently been inherent in the technology and history of the industry.

The Very High Frequency broadcast band (VHF) happens to be that in which television broadcasting first became feasible. Unfortunately, that band contains only twelve channels (2-13). Even after Ultra High Frequency (UHF) broadcasting opened the technical possibility of an additional seventy channels (14-83), in most communities, the fact that most existing receivers were able to receive only VHF limited entry to the 12 channels of the VHF band. While this situation is slowly changing due to the All-Channel Law (which went into effect in 1964) requiring all new sets to be capable of UHF reception, VHF broadcasting has been and still is the most profitable if not the only economically viable means of television broadcasting in most major television markets.

In addition, television signals cannot be received off the air without special relaying apparatus, unless the receiver and the transmitting tower are in a direct line of sight. In particular, signals cannot be directly received beyond the horizon or through mountainous terrain.

[1] It should be noted that the argument of this paper does not turn on the question of whether consumer tastes would be "uplifted" if programs were differently priced. Nor does it depend on knowing how to price programs in an industry in which the marginal cost of an additional viewer is zero. It merely involves the obvious fact that the present institutional arrangements for TV financing are not those of a Pareto optimum so far as recovering costs from users is concerned.

This circumstance means that while programs may be nationally produced, the sources of broadcast signals in each local area have to be local sources. This has led (at least in principle) to the possibility of a system of local control over programming and of diversity from market to market, a possibility greatly fostered by the deliberate policy of the Federal Communications Commission (FCC). The wish to promote such diversity as a matter of public policy, the considerations of equity between neighboring communities, and the fact that communities are sufficiently close that the "natural television reception area" of each generally overlaps that of others, has led the FCC naturally to restrict licensing of television stations in a given locality not merely to the twelve VHF channels but to those channels not assigned to neighboring communities. The result has been that only the largest cities have as many as seven VHF channels available (and one of these is generally reserved for educational television) while most communities have three at most.

In this situation, it is not reasonable to rely on atomistic competition to produce optimal results even given the limitations of the arrangements for recouping costs already discussed. Indeed, we do not rely on such competition. The FCC has generally strongly encouraged local television stations to show programs of a public service nature and has been particularly interested in the promotion of locally originating live programs.

This is a reasonable interest. The effect of the line-of-sight limitation in promoting a diversity of broadcast sources and local control over local program choice may be largely lost if local stations find it most profitable to rely on network or other national sources for programs. While it is possible that such profitability may reflect an overall similarity in tastes among television viewers everywhere, the nonexistence of a mechanism for charging the viewers the long-run marginal production costs of the programs they watch makes it impossible to know this. If all programs were properly priced, it is at least not obvious that local programs would be less profitable for local stations than national ones. In insisting that local broadcast stations originate local programming (which frequently operates at a loss under present arrangements), the FCC may be partly redressing the inefficiencies of the current institutional arrangements for financing television production. Moreover, such a policy may act to inform and change the tastes of the viewing public and (as the FCC doubtless believes) may well be justified on grounds other than that of economic efficiency. Indeed, the policy of diversity of control over mass communication media is obviously founded in public purposes outside of economics.

In any case, the effect of such a policy is the subsidization of local

programming by the profits from network affiliation and broadcasting. Those profits are available for this purpose in part because of the technologically imposed limitations on entry into local television markets. Thus, any innovation which partly removes such limitations is likely also to have an upsetting effect on the FCC's policy of favoring local autonomy.

Such innovations have, in fact, been made, and their growth has been rapid. One might expect this given the existence of entry-inducing profits in local television markets, particularly if such innovations permit relatively low-cost extension of existing services into markets which could not fully support an additional broadcast station. There are now two principal means of overcoming the line-of-sight limitation on the reception of the signal of a given station. These are boosters and satellite stations, on the one hand, and community antenna television systems (CATV's), on the other.[2]

A booster is a device to pick up a given television signal and instantaneously rebroadcast it with increased power. It is particularly useful in mountainous terrain where it enables a television station to reach communities and areas which are separated from it by mountains.

For our purposes, a satellite station may be considered a large booster. It is a television station which originates no (or almost no) programming but rebroadcasts the programs of its parent station. In so doing it gives the parent station coverage of a much wider area than that station could reach by itself. Typically, advertising time on parent and satellite stations is sold as a single unit.

There are three features of the booster or satellite station arrangement which are important for our purposes. First, such arrangements typically involve only the signal of a single station; second, the rebroadcast signal can be received (as can any ordinary television signal) by any receiver within range. This leads to the third point. Boosters and satellites are generally not financed directly by viewers as such in the communities to which they bring the given signal. All these points distinguish boosters and satellite stations from CATV's and all of them help to explain why CATV's have had a far higher rate of growth and are believed to present a much more pressing regulatory problem than boosters and satellite stations.

The influence of the first feature is evident. Since boosters or satellite stations typically involve but a single signal, they are not so attractive to viewers in a local television market as is a device which brings in many signals at once. Next, since off-the-air signals can be received

[2] The use of communications satellites of course provides another means of transmitting television signals beyond the horizon, but it is not likely that this will be used to effect entry of existing television signals into local television markets in the United States in the foreseeable future.

by all viewers, a television station (or other entrepreneur) contemplating the installation of a booster to serve a given community cannot hope to profit from the investment by charges on the viewers served (unless, in true public-goods-analysis fashion, the community can be persuaded to use its taxing power and build the facility itself).[3] As is the case (under present institutional arrangements) with the construction and operation of a television station itself, the profits from the construction of a booster or a satellite station must therefore come from the additional demand for advertising time on the parent station generated by the existence of the additional audience which the new facility will allow advertisers to reach when their commercials appear on the parent station. While such a return has frequently been (and still is) present in sufficient measure to justify the construction and operation of a booster, particularly in communities with little or no preexisting television service, it clearly depends *ex ante* on forecasts of the size of the extra audience which will actually view the station's rebroadcast signal.[4] It is further dependent on the desire of advertisers to reach the entered market via the given station rather than in some other way (which may already be in use).[5]

Thus, especially as a means of entry into markets with some existing television service, boosters and satellite stations may not always seem so attractive a proposition as some device which can gain revenues directly from the users without nonuser benefits and which is attractive to users because it brings in several different television signals rather than one. Such a device is the CATV.

The original CATV's were simply giant television antennas. Such an antenna set up on a mountain can receive signals that are blocked by the terrain from reception in the valley below. The signals are then fed by cable to the television sets of subscribers to the system rather than being rebroadcast and picked up off the air as is the case with signals passed through boosters. While most CATV's still operate in this way, a growing number of large systems use microwave relay to beam the desired signals from a point near their origin to the CATV master antenna in the community served. In this way, CATV's are able to bring in the signals of television stations over very considerable distances.

[3] Incidentally, even this may leave rural viewers subsidized. Many boosters are indeed financed by local cooperatives or local television set retailers and repairmen rather than by broadcast stations.

[4] It is at least possible that such forecasts may be misleading if based on surveys of the potential audience. The public-good nature of a booster signal makes it in the interest of anyone who would like to have such a signal available to overstate the amount of viewing which he will do, as he does not pay for the signal in his capacity as viewer.

[5] In the long run, the addition of one television home to the average audience estimated as actually viewing a station's programs in prime evening time (and the associated addition to nonprime time viewing) is worth about $26-29 in yearly revenue to the station (in 1963 dollars). See Fisher *et al., op. cit.*, for the estimation of this figure.

From their original purpose of providing television service to communities with little or no television service they have grown very rapidly and are now extensively used even in communities with two or more signals available off the air. They serve to bring in (sometimes with better reception than is available off the air) a full line of network programs as well as the signals of commercial and educational independents.[6]

CATV's charge their users directly (although not in proportion to the use made). They also carry several signals. Thus, as indicated above, they are a less risky and more profitable means of entry into local television markets than are boosters and satellite stations, which presumably accounts in large measure for their relative growth. Originating in 1952, CATV's now service about 1,785,500 television homes and their subscriber growth during 1964 was more than 30 percent.[7]

While CATV's have grown and prospered, their growth has naturally had its effects on existing television stations with signals receivable off the air in the communities served by CATV's. Such effects have become a matter of concern to the FCC. To a brief description of those effects and a discussion of whether that concern is properly placed we now turn.

A CATV locating in the market area of an existing television station affects that station's revenues by reducing its viewing audience. This it does primarily through offering viewers a substantial range of alternative signals, so that its effect (so far as subscribers are concerned) is rather like that of the entry of several new broadcast stations into the given area. There are, however, three reasons which (where applicable) make a CATV's impact on the audience and revenues of an existing local station more pronounced than is suggested by the view that it simply involves the provision of alternative signals to a given number of viewers.

In the first place, a CATV subscriber generally does not retain his ability to view programs off the air after his set is connected to the CATV cable. Even in the minority of cases in which this is not so, he must activate a special switching device to change from CATV to off-the-air reception. This means that a CATV which does not carry the signal of a given station and whose subscribers could otherwise receive that signal off the air preempts part of that station's audience or at

[6] Large CATV systems have even been proposed for New York City—a return to their original purpose as a means of providing better signal quality, with tall buildings replacing mountains as the signal barrier.

[7] Based on April, 1964, estimates of the National Community Television Association (the CATV trade association) and November, 1964, estimates of the American Research Bureau. An alternative estimate puts subscribers at the end of 1964 at 1.4 million and growth at about 15 percent per year. See D. M. Blank, "The Quest for Quantity and Diversity in Television Programming," *A.E.R.*, May, 1966.

least places that station at a substantial disadvantage in competing for the subscribers' viewing time.

Second, CATV's in relatively small markets typically bring in all the signals from a large city's stations. Further, most stations are affiliated with one or more of the three major networks and thus show principally network programs in prime evening time. Thus, a CATV which locates in the market area of an existing station and which carries that station's signal is quite likely also to carry the prime time programs of that station from another source, either simultaneously or on a somewhat different schedule depending on the scheduling of network programs by different stations. Such a CATV does not merely provide its subscribers with programs competing with those shown by the local station; it provides the identical product except for the dial position or, possibly, the time the programs are scheduled.

Finally, there is evidence that the average CATV subscriber in one- and two-station markets watches the television considerably more than the average off-the-air viewer able to receive a similar set of signals.[8] In part this may be due to the better average signal quality which may be afforded the average CATV viewer as opposed to the signal quality of off-the-air reception available to the average viewer in a small market able to receive a similarly large number of signals from overlapping distant sources. In part at least it is probably due to the self-selection of subscribers. It is the avid television fan who is most likely to pay for the services of a CATV system. To the extent that this is the case, a CATV does not merely affect a station in whose area it locates by affecting the viewing habits of a part of that station's market chosen, as it were, at random; it affects the habits of a part of that station's market which accounts for a disproportionately large share of that station's audience measured in terms of actual viewing.

All these things mean that CATV's, while aiding the stations whose signals they bring to viewers otherwise unable to receive them, have a substantial long-run impact on the revenues and profits of stations in whose market areas they locate.[9] The FCC has viewed this as a matter of public concern and has adopted for CATV's using microwave relays and proposed to adopt for all CATV's[10] rules essentially requiring CATV's to carry the signal of local stations and not to duplicate the programs of those stations from fifteen days preceding to fifteen days following the time they are shown (news and special events programs excepted).[11]

[8] This is discussed in Fisher *et al., op. cit.*

[9] The size of that impact is estimated in Fisher *et al., op. cit.*

[10] The difference stems from the question of the Commission's jurisdiction over non-microwave CATV's.

[11] While this paper was in press, the FCC issued revised rules. Those rules reduced the duplication protection to the same day on which a program is shown and essentially

The rule that a CATV shall not simply preempt part of a local station's market by not carrying its signals to viewers who could otherwise receive it is patently in the public interest since it prevents the foreclosure of part of the market. The nonduplication rule is less clearly so, especially when simultaneous duplication is not involved. The FCC clearly wishes to protect local stations, but the protection of competition does not generally mean the protection of particular competitors. We must therefore consider whether the FCC's favoring of local stations is a well-grounded policy. We shall do this by considering the general arguments as to whether CATV entry into local markets should be unimpeded by regulatory action rather than by discussing the wisdom of the nonduplication rule in particular.

The argument in favor of unregulated CATV expansion is simple and apparently strong. CATV's provide a service the cost of which the users are apparently willing to pay. To argue that regulation is desirable in such a situation, one must argue that the general presumption that such a service is worth providing is invalid in the instant case for reasons special to the CATV broadcasting industry.

There are indeed such reasons. We saw above that FCC policy, the line-of-sight limitation, and barriers to entry into local television markets have led to a situation in which local programming is subsidized by the profits earned by broadcast stations on other types of programs. We argued that for reasons of public interest such local programming may be desirable or that it may be that viewers, if charged directly, would support such programming. Yet CATV competition drawn by those very profits tends to lessen or eliminate them. It follows that such competition may very well lead to a substantial alteration in local programming—to an end to such local diversity as the FCC has been able to foster under the line-of-sight limitation.

Yet this is not a result which can reasonably be said to be presented to the individual CATV subscriber when he subscribes. If local programs are valued by him, then the amount which he is willing to pay for CATV services is greater than would be the case if he knew that local programs would be changed by his action. Even if this were explained to him in detail, it would make no difference, since the public good character of television financing makes it not in the interest of any individual subscriber to take into account the effect of CATV on local stations even if it would be in the collective interest of all such individuals to do so. In the absence of a system in which viewers bear directly the full costs of the programs they watch, there can at the least be no presumption that because CATV's are profitable the

prohibit CATV's locating close to stations in the 100 largest markets from bringing in signals not already receivable off the air. This change does not affect the analysis of the paper, which does not run in terms of particular rules in any case.

particular marginal costs of providing CATV services are wortl
curring.

Moreover, even if CATV subscribers would in any case subscril
profitable rates knowing with certainty that they were buying a p
age which included the resulting effects on local programming, it w
still not necessarily be the case that CATV expansion shoul
unregulated. By their nature, CATV's are largely restricted to t
and cities where a relatively short cable can serve many viewers.
local stations whose programming may be affected, however, dc
merely serve the CATV subscribers. Rather, they broadcast ov
considerably greater area. If local programming is changed as a r
of CATV competition, therefore, there is an external effect on the
vision services available to nonsubscribers. The resulting possible
utility to such viewers is obviously not included in the CATV costs
by subscribers.[12]

Finally, aside from these arguments, the technological situati
changing in a way that may mean that CATV's if temporarily in
ited would not be profitable in a few years in many of the ma
which they have entered and are entering. The effect of the All-C
nel Law is clearly to remove one of the limitations to local-market e
by new broadcast stations as the existing stock of television sets
preciates. A market which currently has not a full network lineu
VHF stations because it has not been assigned three VHF char
(for reasons of signal overlap) may very well support additi
UHF stations, both network-affiliated and independent, when
sets can receive them. Were such stations in existence, CATV
vices would be far less attractive to subscribers. With unregul
CATV's in the principal towns of a given area, however, entry of
UHF stations is likely to be substantially inhibited. Moreover,
UHF entry will serve the surrounding area and local diversity of
gramming is obviously less likely to be inhibited by local compet
than by competition from large-city stations brought in on CATV's

In short, given the economically highly inefficient institutiona
rangements in local television broadcasting and given the chan
technological situation, the FCC's policy of mild regulation of CA'
may be a reasonable one. Indeed, it may clearly be the case that
carriage and duplication protection of local stations does not gc
enough. Yet it is well to be cautious. The effects of the All-Cha
Law are as yet in the uncertain future and the diversity of prog
ming provided by big city independent stations which are carrie

[12] It should be noted that the argument does not depend for the most part on
programs being the ones which are sacrificed when revenues fall (or fail to rise).
change in the programming of the local station induced by the effects of CATV compe
is an externality in the context of the present institutional arrangements.

CATV's may be more important than the local diversity provided by profit-subsidized local programming. In not inhibiting CATV growth beyond the mild restrictions involved in the carriage and nonduplication rules—restrictions which reduce only mildly the alternatives open to CATV subscribers—the FCC may be giving proper weight to these considerations and to the fact that CATV subscribers are willing to pay for the services they are getting, even if the full costs of those services are not recovered thereby. Whether such a restrained policy is in fact sufficient to allow the growth of UHF stations and to offset the effects of CATV's on the local programming available to subscribers and also to nonsubscribers is difficult to say at the present time.

One further point. It is clear that the encouragement of local diversity in television is a legitimate aim of public policy. This is particularly evident if we recall the noneconomic benefits of such a policy in a free society. Regulation of CATV may be necessary to preserve that diversity in the presence of the economically inefficient methods now used to sustain it. Yet one cannot but feel that such regulation is but another patch on a set of arrangements not particularly well suited to public ends. If local programming and local control are to be encouraged, it might be more efficient to subsidize local stations directly than to restrict their competitors.

REGULATION AND TECHNOLOGICAL DESTINY: THE NATIONAL POWER SURVEY

By William R. Hughes
Wesleyan University

The image of regulatory institutions as tardy, reluctant, and imperfect adjusters to changing technology finds considerable historical justification. Yet regulation should lead wherever the technological contours of the future can be foreseen. The FPC's recent National Power Survey is an attempt to exercise such leadership. Through long-range planning, the Commission has tried to anticipate technology, to understand its implications for policy, and to influence entrepreneurial choices governing its development and use. My purpose is to give a general interpretation of the survey as a policy instrument from the viewpoint of a sympathetic participant. I shall abstract from the specific contents of the survey in order to focus on the logic and defining characteristics of its strategy, the technological and institutional context in which the survey has been applied, and what the survey might be expected to accomplish.

Technological Destiny in Electric Power

If any mode of production can be said to have a foreseeable technological destiny, it is central station electric power service. The essence of this destiny is the interdependent nature of generating and transmission networks, which reduces the individual autonomy of separately managed "systems" in making efficient planning and operating decisions. This increasing interdependence stems from growth in demand and from the dynamics of fundamental economic-technological relationships which underlie local natural monopoly for central station service. The more important factors for network development are (1) "built-in" scale economies in such basic pieces of electric system hardware as generating units, lines, and transformers, so powerful that a scale-increasing bias is imparted to technological progress, (2) simultaneous production and consumption and the consequent importance of interconnections which, by pooling reserve capacity and combining demands which occur at different times, reduce the capacity needed to meet peak loads reliably, and (3) land-use economies afforded by a single optimized network. These latter economies increase in importance as the industry expands and competes for other land uses in an urbanizing, growing economy. As these and other factors operate over

time, networks tend to grow not only in geographic extent but, more importantly, in the degree of their "internal" interdependence.

Why Should Regulators Plan?

Basically there are two reasons why the Commission can add something useful to the industry's own planning. The first is self-education. The resources at stake are so vast—perhaps $100 billion in new capital outlays alone over the survey's twenty-year planning period, not to mention operating costs and externalities—and our dependence on a reliable energy supply so great that the FPC would be remiss in the exercise of its responsibilities if it did not seek a much more systematic understanding of future patterns of network development than the usual "keeping up with the current industry activities" approach affords. This in itself argues for FPC involvement in planning no matter how good a job the industry might appear to be doing and even if no other public action were needed. The blackout of November 9 is a case in point. The FPC's ability to move expertly and constructively in leading the post-blackout investigation has been greatly enhanced by the learning experience and working relationships with industry experts gained by both Commissioners and staff as a result of their immersion in the survey. Second, there is good reason to believe that a knowledgeable FPC will also have a geographically broader, temporally longer, and more public-interested perspective than the typical industry planner. Given the great size of the industry, its institutional variety, its fragmented and somewhat haphazard ownership pattern, the complex, sometimes inconsistent legal framework within which it must function and the lack of strong pressures for major institutional change, it should be no surprise that the initiatives of utility system managers—public, investor-owned, or cooperative—have often failed to produce the highly coordinated planning and operating decisions needed to approach the ideal of efficient resource use very closely.

Presurvey Policy

It is ironic that the institution of local monopoly and its attendant public controls have experienced recurrent adjustment strains with respect to the dynamic manifestations of the very economic-technological factors that led to their creation. While the inevitability of large interstate networks was apparent before World War I, public policy has been slow to recognize the implications of this development. In the reform legislation of 1935, efficient network integration was at last recognized as a federal policy goal. However, until the survey was initiated in 1962 under Joseph Swidler's leadership, this goal was largely overshadowed in the execution of policy—initially by more urgent corrective tasks and more recently by the FPC's postwar retreat from

regulatory activism in power. The World War II mobilization involved joint government-industry planning that embraced many elements later used in the survey and has had lasting effects on coordination. But it necessarily lacked the more flexible, long-term orientation appropriate to peacetime circumstances.

Recent Network Developments and Institutional Response

Meanwhile, the development of power networks and industry institutions has made it increasingly difficult to ignore the implications of growing interdependence. If we use the physical definition of a network as a set of facilities normally operated on a continuously interconnected basis, then we are already close to being a virtual two-network country. Nearly all of the U.S. east of the Rockies (over 70 percent of U.S. generating capacity) has been operating in parallel since 1962, and completion of lines already committed west of the Rockies will tie together nearly all of the capacity in that area. The Texas Interconnection, with only about 6 percent of the nation's generating capacity, is the lone significant holdout, for the purely institutional reason of avoiding federal regulation. Spanning the Rockies is largely a question of when and how. The near-term economics of large-scale ties are unfavorable, but growth in the absolute scale of potential time-zone diversity transfers, possible reductions in reserve capacity west of the Rockies, and falling costs of long-distance transmission are likely to tip the balance eventually, perhaps in the early 1980's. This event is physically impressive and raises important questions of ownership, control, and cost-benefit sharing with respect to the long-distance lines that would have to be built. Perhaps for these reasons, the future geographical extension of networks tends to attract attention out of proportion to the very small percentage by which it should eventually reduce the total cost of power.

Less visible but economically much more significant are the implications of technological destiny for the "internal" development of the network: the increasingly complex, multi-system nature of efficient choice of location, scale, and design of new generating facilities, lines, control equipment and other network components and of their efficient operation. Underlying technological and economic trends have been somewhat uneven in their historical pace, and we are currently in the midst of an acceleration in these trends which began to appear in new capacity installations during the 1950's. The simplest illustration I can give is the current explosive increase in the capacity of the largest steam-electric generating units available. From 1930 to 1950, maximum prevailing unit size grew from roughly 75 MW to around 175 MW, a somewhat slower rate of growth than that of system peak

loads. Consequently, growth in unit size per se did not apply increased pressure on individual systems to coordinate with their neighbors. Today, a 1,000 MW unit has already been installed, larger units are on order, and orders for new units in the 500-1,000 range are no longer unusual. This represents a rate of growth in unit scale since 1950 of two to three times that of system peaks. Today only the four or five largest of the industry's 3,600 systems can, even with strong interconnections, justify installing most economical large units now available if new capacity were to be used only to meet their own loads. Other parallel developments in transmission voltages and in possibilities for long-distance transmission can also be cited. Add to this such developments as the increasing economic attractiveness of nuclear and pumped storage plants, both of which are most advantageous when employed on a very large scale, and it is easy to see that optimizing decisions in the electric power industry, as presently structured, will be increasingly multi-system to the extent that they are efficiently made.

Moreover, the industry's ownership pattern has been comparatively stable since the early 1950's, both within the investor-owned segment as a result of the post-Holding Company Act slowdown in consolidations and between the segments as a result of the comparative maturity of the public and cooperative segments which has accompanied such developments as the saturation of rural electric service, the post-Dixon Yates stabilization of TVA service territory and financing, and the growing scarcity of attractive new hydro sites for federal development. Dramatic changes in this pattern are unlikely.

This stability of the ownership pattern implies a shift away from the consolidation route to efficient network integration and toward greater reliance on intersystem coordination. While such primitive forms of coordination as interconnections for emergency assistance are at least sixty years old, and most of the basic practices we associate with the term "power pooling" had lost their novelty before 1930, the scale and complexity of their application have undergone such greatly accelerated development since World War II that the difference in degree is almost one of kind. In a few of the most tightly organized coordinating groups, the pool has begun to eclipse the system as a basic decision-making unit with respect to network planning and operation. There is as yet great variation in the level of coordination achieved, and among the 100 or so largest systems, with some 90 percent of industry generating capacity, there is a great potential for improvement simply by extending well-established methods of pooling. For most of the 1,200 smaller utility systems with generating capacity, a group which includes over 850 nonfederal public systems and some 75 cooperatives, the efficiency of the very large systems is approachable only by fairly

radical departures from existing "best practice" if they continue in the generating business. My own opinion is that most of these systems will have to become distributors if they are to survive as part of an efficient power supply arrangement. Because of their minute share in industry generating capacity, these small systems are relatively unimportant from the standpoint of industry-wide resource allocation. But the sharp cleavages of interest between the "public" and private camps make the small system problem very important to policy-makers.

In this context of intensified technological and economic pressures for network integration, the National Power Survey was begun to encourage increased coordination within the confines of a relatively stable industry structure. In pursuit of this goal the Commission employed a distinctive strategy, the defining characteristics of which are heuristic planning, industry involvement, and a deliberate exclusion of basic institutional questions from explicit consideration.

Heuristic Planning

First, the FPC's planning and recommendations had to be heuristic rather than operational. This is not only, or even primarily, because of the absence of legal authority to compel adoption of its recommendations. Far more important are (1) the fact that the FPC's comparative advantage lies not in its technical planning capabilities but in its perspective and (2) the fact that operational planning is a continuous process within which any given comprehensive "plan" has a very short life and frequently looks very different on a map or a drawing board from the specific investment alternatives which are finally chosen.

With respect to comparative advantage it is clear that the FPC cannot come close to matching the professional skills, specific experience, and other resources of the industry's hundreds of full-time system planners. But although raising the quality of regulatory planning certainly increases its worth, it appears that planning of modest complexity and quality can still have great heuristic value—an important point in view of the severe staff limitations under which FPC and other agencies must operate. By raising pertinent questions and by suggesting alternatives neglected by the industry, the regulators can stretch the horizons of industry planners. Some Power Survey "planning" does no more than state systematically what any knowledgeable observer of the industry knows to be an improvement over the imperfectly coordinated choices presently made—as in the case of increasing the scale of generating units where constraints on unit size to date have been clearly institutional. In other cases, stating obvious implications of future trends has surprised and stimulated decision-makers whose vivid operational focus on near-term problems has increased the difficulty of thinking clearly and thoroughly about the more distant future. Other

FPC planning exercises (e.g., its studies of load diversity) have led to industry efforts to assemble detailed information previously not available.

The industry's comparative advantage in operational planning is reinforced by the necessity for continuous adjustments in plans, the need for very specific cost and design information in choosing between alternatives, and the importance of information feedbacks from operating experience and from such tentative attempts to implement decisions as bargaining with suppliers. The idea of looking as far ahead as twenty years on a system or network basis is to gain an improved understanding of the long-term consequences of specific near-term choices. The 1980 patterns published by the FPC are only as good as the information available before the report went to press; moreover, since they cover the entire country they must rely on short-cut planning procedures and a very approximate, thin information base. Hence the frequent caveats in the report.

Inevitably, heuristic planning can be expected to have some perverse effects. Overliteral or premature adoption of recommendations is a real danger. If, for instance, the climate of persuasion creates fears of imminent increases in federal control or expansion of federally-owned networks, investor-owned utilities may try to forestall these developments with massive, highly visible commitments of their own. The politics of electric power are such that any FPC recommendation has side effects on intra-industry disputes and industry expectation of FPC policy changes. Furthermore, the FPC would lose much of its persuasive muscle and hamper its independence of action in recommending future legislative changes if it tried to give unequivocal assurances that it would oppose extension of federal controls and federal ownership. Equally serious is the possibility that Power Survey recommendations might be used as spurious support for actions which conflict with their intent, as in the case of giving a "coordination" image to excessive transmission capacity intended to help build up the owner's rate base in a period of rising rates of return. Another problem is that recommendations which make their best sense when considered as part of an interrelated package may be bought piecemeal. Interconnections and large units may be added without adequate provision for dealing with such side effects on the larger network as the need for better protective equipment, faster communications, emergency procedures, and delineation of operating responsibilities. All of these problems set severe limits to how far persuasion can reasonably be pushed. They emphasize the essentially incremental, trend-modifying nature of the Power Survey and suggest the importance of injecting its end-goals as important considerations in every area of regulatory contact between the Commission and the industry.

Industry Participation

The second major feature of the Power Survey approach—industry participation—follows directly from the considerations underlying the first. To the extent that the FPC can tap the industry's resources without impairing integrity and perspective, it can formulate more solidly based plans. Further, periodic exposure to a variety of expert critical reaction during the planning process weeds out mistakes and opens up new lines of inquiry. Equally important is the opportunity for communicating and refining the survey's persuasive messages. Public communication, valuable as it is, lacks the flexibility, intensity, and frankness of informal give and take in an atmosphere of mutual respect. A decision-maker will take recommendations most seriously if he knows the Commission has made an honest effort both to understand his suggestions and reactions and to evaluate them on their merits.

Wherever possible, the FPC used planning criteria and technical assumptions endorsed by advisory committees. Consequently the survey's technical assumptions approximate expert consensus where it exists, and the FPC has demonstrated its good faith in resolving ambiguities on such disputable assumptions as service lives of new equipment. Similarly, the FPC, before making up its own broad network plans, organized Regional Advisory Committees which submitted region-wide plans of their own. Frequently, these were little more than patched-together versions of plans already developed, valuable to FPC primarily as an information source and as a vehicle for asking questions. But in at least one region—the midcontinent area—a single organic plan was submitted for a hitherto loosely coordinated area, and the positive influence of the FPC request was acknowledged by the planners involved.

When the survey was announced in January, 1962, industry reaction was variously hostile, suspicious, and fearful. The subtly changing tone of the trade press as the survey progressed testifies to Swidler's success in creating greatly increased respect for the survey's integrity and usefulness. This change reflects such factors as judicious selection of advisory committee members, a generally open-minded and respectful response to industry advice, and Swidler's remarkable sense of the appropriate in his personal industry contacts. Furthermore, industry participation in the survey was far more substantial and constructive than the routine advisory committee performance that might have developed. Surprisingly, some of the survey's most detailed and complicated planning exercises were suggested by industry advisory committee members. The outstanding example is the Commission's extensive computer study of reserve capacity savings from increased interconnection.

Of course, once it was clear that FPC could not be dissuaded, industry cooperation was a matter of intelligent self-interest, not only to avoid a foot-dragging image or because constructive participation might give the survey a softer critical tone, but also because—for any given degree of regulatory toughness—a competent, consistent, realistic line of persuasion is more in harmony with industry self-interest than one that is erratic and poorly conceived. Moreover, the gains from coordination among investor-owned systems are in part translatable into profit, and the survey has provided some coordination-minded executives with leverage in overcoming the reluctance of their neighbors. Beyond all this are the easily underrated elements of professional pride, integrity, and desire for service. For such men as Philip Sporn, of American Electric Power, and G. O. Wessenauer, of TVA, technical excellence is a way of life and efficient power supply a major end in itself.

Nonalignment on Institutional Questions

The third main ingredient of the survey approach, nonalignment with respect to major institutional issues, is well summarized in the following excerpt from the report:

> The National Power Survey does not attempt to resolve or even examine all the existing or anticipated institutional problems of the electric utility industry. Rather, we are here concerned with demonstrating the opportunities of full utilization of our technology and our fuel, capital and management resources, and how they can improve the lives of all Americans in the years ahead.
>
> We are aware that many controversial areas of public policy are related in one way or another to the industry's success in lowering power costs. These policy areas include the territorial integrity of the retail marketing areas of competing systems, the usefulness of public power programs as a yardstick to supplement regulation of privately owned systems, the differential revenue requirements because of variations in the tax, financing and earnings requirements of individual systems, the obligations of the large systems in their relationships with the small in power supply arrangements and in competitive situations, the proper role of statutory preferences to public and cooperative power distributors for the sale of power from Federal systems, and many others. We believe that the National Power Survey Report is not the appropriate medium for attempting to reconcile the conflicting views on these issues of public policy, if that were feasible, or to formulate or express our own views on these issues. The question of the proper scope of government in regulating or conducting economic affairs is one of the root problems in a democracy. We do not attempt to resolve it in this report.[1]

Just as institutional problems are acknowledged but generally played down throughout the survey, there is also a sustained emphasis on the fact that coordination at its best has shown great adaptability in coping with institutionally and legally messy situations. There is continuous stress on the idea that the major ownership segments are here to stay and that their relative positions are not likely to change rapidly. Moreover, the virtues of pluralism and variety are alluded to without being specified. The primary objectives of this tack are (1) to get the various "public" and private power factions to recognize that

[1] *National Power Survey* (Fed. Power Comm., Washington, D.C., 1964), p. 5.

campaigns of extermination are most likely to end in a general stalemate in which everyone loses and (2) to concentrate the attention of these groups on the one thing they have in common: an interdependent technological destiny and the need to cooperate in order to benefit from it fully. At a minimum this approach has succeeded in limiting the use of the survey by narrow interests as a device for aggravating intra-industry squabbles. Given existing hostile attitudes and genuine differences in interest, the survey's progress beyond this minimum must be measured with a very small scale and with an eye toward the possible long-term effects of continued peacemaking efforts by the FPC. The cost of trying is low, the payoff could be very high, and the FPC has been trying hard for only a few years.

A third reason for keeping basic policy issues out of the survey is the Commission's desire for freedom of action in formulating and evaluating specific policies and legislative proposals in directly operational contexts.[2] But deemphasis of problems connected with the institutional *status quo* tends to create an impression of direct endorsement despite the FPC's carefully phrased disclaimers. There is a risk—hopefully small and of short duration—that reform might therefore be made more difficult.

What Next?

Perhaps because the survey excluded basic institutional issues, they may now be more amenable to solution. By encouraging peaceful coexistence between industry factions, the survey may help create a more favorable political atmosphere for resolving the legal issues which aggravate intra-industry disputes. Furthermore, the implications of technological destiny for overall industry performance may receive greater weight in the formulation of policy. But the initiatives of government and industry are likely to bring only piecemeal change to a policy framework that has not been comprehensively examined since the New Deal. In this respect, we, as professional students of economic policy, have no excuse for lagging in the formulation of policy suggestions appropriate to the future. *Ex post* criticism and warmed-over utopian proposals may sound convincing to the isolated academic faithful, but they have little impact on the men who make practical decisions. We need to improve our understanding of the interactions between electric power institutions, technological destiny, and the goals of public policy, if changes in the policy framework are to benefit from the broad, independent-minded perspective that we should provide.

[2] Where FPC policy is already well defined and requires no new legislation, the survey is used as a vehicle for promoting the policy. For instance, the close link between the quality of wholesale rate regulation and the willingness of small systems to specialize in distribution is heavily stressed in all survey contexts.

NEW TECHNOLOGY AND THE OLD REGULATION IN RADIO SPECTRUM MANAGEMENT

By HARVEY J. LEVIN
Hofstra University

New communications technology has required the reexamination of many policies and assumptions in recent years. So much so that the spectrum manager at the Federal Communications Commission and the President's Director of Telecommunications Management are starting to look like a harrassed out-of-towner, racing down an up escalator, for that crowded subway about to leave Times Square on Christmas Eve. They may get there on time, but there must be an easier way.

Not that all responses to the challenge of new technology have by any means been this awkward or inappropriate. However, it is well to recall that the regulatory policies formulated to cope with such new developments as community antenna TV, communications satellites, or even high-speed data processing are really only half the story. Equally important is the more general problem of how to accommodate new radio users and the new frequency demands of old users in a seriously congested spectrum. And it is in this very area that we have probably had some of our more frustrating experiences.

The relative ease with which radio users are accommodated will often affect the level of their capital costs, the rates they must therefore be permitted to charge (or would in fact come to charge under competition), and the spectrum manager's ability to impose public service standards. Yet there is now no ready way to take account of all such effects in the allocation of frequency space. Accordingly, the accommodation of new radio users and the new demands of old users will occupy my full attention here after a brief preliminary word on the spectrum resource itself.

I. *The Radio Spectrum Resource*

The radio spectrum refers to the full range of radio waves that may theoretically be used to transmit information by electromagnetic energy. By way of very crude analogy it can be likened to the highways, the airways, or the national airspace. In these cases, the spectrum "user" bears the same relation to the information which he transmits (the "service" he provides) as the trucking company or airline does to its vehicles. And it is well to recall that the broadcaster is only one of a multitude of radio users which include government as well as nongovernment users in the so-called "fixed, mobile, and broadcast" bands.

The three military services alone, incidentally, account for some 80 percent of all government occupancy, and some 40 percent of total U.S. occupancy (government plus nongovernment). Nongovernment users, on the other hand, in addition to radio-TV broadcasters, include the communications common carriers, the nonbroadcast public user (which includes local governments, fire, forestry, police, and other public safety services), and the burgeoning nonbroadcast private radio user (in business, industry, transportation, and utilities).

Like the national airspace, the spectrum exists even when it is not used. Moreover, the spectrum user, like the user of highways, airways, or national airspace, must be constrained in his use to assure safe, reliable, effective occupancy. Without rules of the road, that is, use in each case may be abortive, inefficient, or foreclosed with the diversion of potential users to other modes. Perhaps even more than the nation's highways and airspace, moreover, radiofrequency space has long been so heavily clothed with a public interest as virtually to hide its drabber economic potentialities. To be sure, radio users can be classified and cross-indexed very roughly, at least in terms of their relative contributions to national and internal security, public information and education, private communication, recreation and entertainment, scientific exploration and research. But radio figures also along with other less romantic factor inputs in the production functions of many business firms today—in business, industry, transportation, and utilities. It therefore comes as no surprise to find at least a glimmer of interest among regulators in measuring the spectrum's annual contributions to the GNP, and the opportunity costs of some of its loftier uses.

It is well known, moreover, that technological advance may help us accommodate more radio users—and thus reduce the opportunity costs of particular uses—notwithstanding perennial cries of "spectrum scarcity." It does so by extending the intensive and extensive margins of frequency space as well as by developing viable new substitutes for radio.

Extensive frequency development refers to improved capabilities to communicate at higher and higher frequencies, capabilities that may result from some new technique like wartime radar, which helped us subsequently to generate a whole new microwave technology, and thereby to open up for intensive commercial use broad expanses of theretofore unusable frequency space. Other times the extensive development may come through some spectacular noncommunications breakthrough, as, e.g., the advent of booster-tracking-satellite technology. For this breakthrough means that we are no longer limited in our transoceanic communication to the cruder radio systems that operate in the congested high frequency band, or even to submarine cable tech-

nology. Rather may we use also the more abundant microwave frequencies, higher in the spectrum, for international communications of a striking variety, volume, and reliability. Frequencies hitherto unusable, for certain purposes at least, have now become technically and economically usable for these purposes.

Intensive spectrum development, on the other hand, refers simply to the ability to transmit the same volume and quality of information in less frequency space than before or at a higher rate in the same band width. One illustrative advance here is the breakthrough in improved directionality of communication through new directional antennas and line-of-sight beaming. Another illustration is the dramatic new spectrum-saving techniques which have virtually doubled potentially usable space in the high frequency band (through the move to single sideband modulation); and multiplied the space for mobile radio users manyfold since 1940—some 2,500 percent, I believe—through a sustained reduction in channel spacing.

Intensive like extensive development may result from government or from private R and D and innovation. Extensions of the intensive margin have in fact tended to reflect private initiatives, whereas the more crucial advances at the extensive margin are government precipitated. The private advances also result from deliberate spectrum-developing research programs of such companies as Motorola, A.T.&T., G.T.&E., RCA Communication and the Hughes Aircraft Corporation. Whereas some of the more spectacular government contributions have been inadvertent—unexpected by-products of "crash" programs geared to quite different ends—to weaponry, rocketry, and radar development.

Among the best-known substitutes for radiocommunication are open wire, landlines, coaxial cable, and submarine cable. However, transportation is also a substitute and so, conceptually, are the other factor inputs which the use of radio displaces in transport, public utility, and manufacturing enterprises (viz., labor, storage facilities, vehicles, etc.). Even in the broadcast field, some say that a nationwide grid of wired CATV systems, interconnected by microwave frequencies and financed by subscriber fees, may one day offer a viable economic alternative to the advertiser-supported broadcast service. If this were so, of course, at least some of the frequencies now allocated to television might conceivably be reallocated to private or common carrier users in the fixed or mobile service, or even to some of the needier, nonmilitary government users.

In the spectrum, then, as elsewhere, if one is willing to pay the price, there are more substitutes than the regulator may think. And this is worth remembering whenever we encounter that ingenious web

of administrative regulation based upon the asserted scarcity of radiofrequency space. Indeed the greatest onslaught on this age-old premise may well be mounted with the eventual introduction of the circular waveguide tube (cousin of the overland cable), and the communications laser (key to communication through guided lightwaves, no less). The capabilities of both these techniques incidentally are so fantastic as virtually to dwarf all communications capacity used globally today.

How ought the frequency manager respond to these facts of new technology?

II. *The Task of Radio Spectrum Management*

The traditional task of spectrum management has long been to distribute use rights to competing claimants. For this the manager has sometimes employed an impressive array of social priorities, although more often than not he has simply acquiesced to the brute standard of first come, first served. And yet, however the occupancy rights are distributed—whether by priority-of-application, by high-flown social priority, by auction, or by lot—technological advances that extend the range of usable frequency space should obviously ease the accommodation of competing claimants. Indeed, frequency managers—especially at the ODTM—have become increasingly aware of this fact. So much so that a new-fangled "frequency development" goal may one day share equally on the regulatory agenda with the old-fashioned "frequency allocation" goal.

To see why this is so one has only to recall two hard facts of telecommunications life today:

1. New spectrum users, or the new frequency demands of old users, can ordinarily be accommodated only through dislodging or preempting space already occupied by others; or by sharing occupied space; or by borrowing unused space; or by using their own space more intensively; or by developing new capabilities for utilizing unused space higher in the spectrum.

2. Each of these five accommodating processes involves new hardware or technical improvements of varying magnitudes. The incumbent will normally not cooperate and move unless he has somewhere to go, and this may require new R and D and capabilities to use higher frequencies. Frequency sharing and more intensive use of present assignments both by incumbents and newcomers alike may also require improved equipment, or new techniques. Even the borrowing of unused space may require the development of new devices to vacate on short notice.

By making the spectrum pie bigger in some sense, then, the accumulation of even minor technical improvements can potentially ease

the allocator's problems in choosing among competing claimants. But the accommodation of radio users who introduce the bolder new technologies still raises serious problems of its own, and it is to two such problems that I shall now turn. First, the problem of newcomers, incumbents, and innovative change; and, second, the problems posed by frequency stockpiling.

Newcomers, Incumbents, and Innovative Change. In Table 1 below are several illustrative episodes where new spectrum users (or old users with new frequency demands) sought accommodation from incumbents either through sharing, borrowing, or outright preemption; and where these preferred options were presumably more attractive to the newcomers than such alternatives as developing new systems, turning to a wireline substitute or to transportation, etc. Now in each case examined investment in new improved hardware was an issue either because the newcomer himself sought to introduce a new technique (or even some major new invention) or because the incumbent's accommodating behavior would require such investment. Presumably, newcomers would secure costless accommodation only where they sought to borrow idle frequency space. Although here, too, there could conceivably be dislodgement costs later when the original incumbents sought to reenter, and we have already noted the need for special equipment to vacate on short notice.

Careful study of such episodes as these suggests at least five preliminary and tentative conclusions: (1) Newcomers would presumably prefer to buy rights to share, borrow, or preempt frequency space directly, so long as these cost less than it would to develop a new system or to use some substitute for radio. (2) There is, however, no way at present for them to do so. (3) Incumbents normally have little economic incentive to accommodate newcomers now unless regulators compel or otherwise cajole them to do so. (4) *De facto* squatters rights often strengthen the incumbents' hands immeasurably in exacting a monopoly rent before accommodating any newcomer. (5) The question is therefore how to articulate the opportunity costs of spectrum occupancy lest incumbent's equities in his frequency space and his political leverage, plus the regulator's inertia, combine to make *de facto* squatters rights determinative whatever the economic or social merit of a new technique.

In short, can radio spectrum managers devise techniques to help them do what market incentives would otherwise do? Of course, one approach would be simply to auction off renewable rights—leaving them freely transferable and subject to periodic reauctioning. This is one way to create at least limited property rights in the U.S. portion of the spectrum. But whatever the intriguing theoretical case for trying

TABLE 1*

ILLUSTRATIVE EPISODES INVOLVING THE ACCOMMODATION OF NEW DEMANDS FOR RADIO FREQUENCIES

Source of New Demand	Proposed Radio Use	Present Incumbent	Option Sought	Alternative	Outcome
American Telephone & Telegraph Co. (1957)	International Telephone & Television Service	Broadcast Television (UHF)	Temporary use of idle channels 74–75, 81–82, on noninterfering basis, for multi-channel telephone and TV service to Cuba, using new tropospheric scatter technology developed for military needs and desired here partly to diversify modes of service.	Develop new system at higher frequencies and higher cost; or lay additional cable with higher message cost per channel mile.	FCC lends requested UHF channels for service at 840 and 880 mc/s, contingent on (a) noninterference with Florida TV stations; (b) use of highly directional antenna; (c) normal renewable 2-year license in International Fixed Public Service. License granted 1957, renewed to date, though new microwave system now available at 2100–2200 Mc/.
Federal Aviation Agency (1959)	Air Traffic Control	Military	Preempt or share rarely used military space with military bearing adjustment costs.	Develop new system elsewhere at far greater cost	President resolves contest in favor FAA as recommended by OCDM; but military guaranteed reentry in emergency into 132–135 Mc/band. FCC further requires costly supporting adjustments by related aeronautical licensees (Aeronautical Radio, Inc.).
Federal Communications Commission (1959)	Broadcast TV	Military	Exchange of some UHF channels (512–890 Mc/) for as much as practicable of military VHF (225–450 Mc/), thereby to implement social priorities of TV Allocation Plan.	Continued use of UHF with steps to activate it; reduced geographic spacing of VHF stations; CATV systems; pay-TV.	Military and OCDM reject FCC proposal because of (a) $5 billion cost to develop new military systems elsewhere; (b) interim loss of U.S. and Allied defense capabilities; (c) uncertainty as to whether future military capabilities would be comparable to those lost. Subsequent passage of all-channel receiver law, and other steps to activate idle UHF as yet unsuccessfully.
Intergovernmental Oceanographic Commission UNESCO (1964)	Weather and oceanographic buoys	International Fixed, Maritime, Aeronautical, Amateur and Broadcast Service	IOC asks International Telecommunication Union to create new Ocean Data Service through sharing of HF band with low power until new satellite system developed.	Await development of new meteorological satellite with no interim use of HF band.	In process. U.S. favors sharing of maritime frequencies; Scandinavia favors sharing of aeronautical frequencies.
Midwest Program for Airborne TV Instruction (1963)	Airborne Instructional TV	Broadcast TV (UHF)	Preempt UHF chs. 72, 74, 76, 78, 80, 82. Or secure (a) 10-year lease on all six channels; or (b) full-time use of two, part-time use of four channels. Each option requires sharing with ground-based commercial or educational TV stations.	Develop new system in International Fixed TV Service band (2500–2690 Mc/) at 25 percent higher costs, with delay and loss of educational service. Or install wire system at far higher costs.	FCC rejects petition July, 1965, to preserve UHF space for ground-based broadcast TV. But temporary use of chs. 72 & 76 to enable MPATI and local school systems to amortize TV investments. Invites application for six channels for new microwave system at 2500 Mc/s.

Source of New Demand	Proposed Radio Use	Present Incumbent	Option Sought	Alternative	Outcome
Military (1954)	Tacan-DME (distance finding service)	Civil Aeronautics Agency	Preempt space then occupied by newly instituted CAA system, to institute military system developed secretly and requiring identical frequencies.	Scrap system just developed and (*a*) develop new system elsewhere; (*b*) use CAA system; (*c*) do without service.	Military prevails. CAA undertook costly conversion of hardware to maintain common navigational system (VORTAC).
University of Illinois; Office Naval Research (1963)	Radioastronomy	Broadcast TV (UHF)	Borrow or preempt idle UHF channel 37.	Relocate radiotelescope elsewhere in U.S., or curtail program.	FCC acknowledges de facto borrowing and systems development investments and formally authorizes continued borrowing for ten years starting 1/1/64.
National Academy of Science (1964)	Radioastronomy	Weather Bureau; Military (radiosondes)	Share space Weather Bureau with latter investing in new weather balloons. Also preempt military space since sharing not feasible.	Because radioastonomy in some of its functions can operate at certain points in spectrum only, sole alternative to sharing or preemption is to curtail program.	IRAC and FCC house radioastronomy in 1660–1670 Mc/band; radiosondes in 1670–1700 Mc/band. Military excluded from 1660–1670 Mc/ except in wartime emergency, pending development of new hardware.
National Aeronautics and Space Administration (1959)	Tiros weather satellite	Military Telemetry	Continued borrowing of military space (235, 237.8 Mc/s).	Develop new system higher in spectrum (at 1435–1525 Mc/s, or 2200–2900 Mc/s), at far greater cost, with uncertainty as to its capabilities.	Continued borrowing negotiated within Interdepartment Radio Advisory Committee, pending removal, by Jan. 1, 1970, of all telemetry in 225–400 Mc/band.
National Association of Manufacturers (1955)	Manufacturers Radio Service	Broadcast FM	Reallocate FM band to permit greater geographic flexibility in frequency assignments; share selected FM frequencies to facilitate creation of new Manufacturers Radio Service.		FCC rejects FM petition but creates separate manufacturers radio service in 1958 in Land mobile bands (150, 450 Mc/). However, problems of congestion persist.
NAM (1962)	Manufacturers Radio Service	Broadcast TV (VHF)	Share VHF channels 4 and 5 (66–72 and 76–82 Mc/s) to accommodate "flea power" mobile equipment in manufacturing plants for personal safety. Superior here than in 150 or 450 Mc/bands.	Growing access time (delays) in using all mobile radio equipment at 150 Mc/; increased use of 460 Mc/ at higher costs; borrow unused FM or unused UHF channels; borrow or share adjacent VHF channels in major metropolitan areas (viz., chs. 3, 6, 8, 10, 12); or withdraw completely from radio, and use instead more vehicles, labor, storage facilities, etc.	FCC permits sharing on noninterference basis.
NAM; Electronics Industries Association (1964)	Land Mobile	Broadcast TV (UHF)	Reallocate unused UHF chs. 14 and 15 for land mobile users in Los Angeles area.		Decision pending. Meanwhile congestion grows, longer access time, greater use of systems at 450 Mc/, with exploration of other alternatives cited.

* Abbreviations—

Mc/ = megacycle (1,000 kilocycles)
Kc/ = kilocycle (1,000 cycles)
UHF = Ultra High Frequency
VHF = Very High Frequency

IRAC = Interdepartment Radio Advisory Committee
OCDM = Office of Civil and Defense Mobilization
CATV = Community Antenna Television

SOURCE: Information provided by Federal Communications Commission, Office of Telecommunications Management, and other government agencies or private organizations involved in above proceedings.

to create an organized market for frequency space at home this way, such an approach does pose some very serious international problems in view of the reluctance of nations to relinquish sovereignty over the spectrum shares they now claim. In addition, the deep-rooted political resistance to any such scheme at home, the radical institutional changes needed to institute it domestically, and surely internationally, all point up the need to consider still other alternatives. One possibility that may merit more careful scrutiny than it has so far received would be to impose rental charges on spectrum occupancy.

A Modest Proposal. At present, newcomers are free to choose between developing a new system, using a wide array of substitutes for radio, and risking the hazards of administrative proceedings to win rights to share, borrow, or preempt the incumbents' frequency space. Incumbents, on the other hand, now oppose accommodating a newcomer wherever the cost of this opposition falls short of the costs they would otherwise incur. This is true both for government and nongovernment incumbents.

But let us now introduce some such two-part rule as this: Newcomers must reimburse incumbents for any costs incurred in vacating, sharing, or lending space to accommodate them. However, should incumbents prefer not to accommodate, then they in turn must reimburse the newcomers for any extra costs imposed on them through exclusion. Had the FCC or the DTM been empowered to authorize such reimbursements over the years, one wonders how many more incumbents might have agreed to make adjustments which they in fact opposed as unwieldy or uneconomic. Even within the government family of users itself (the Interdepartment Radio Advisory Committee), technical "facts" on the feasibility of sharing, vacating, or lending frequency space are probably as much the subject of bargaining and negotiation as are the more elusive economic or social facts. In other words, the economic incentives of all parties are intimately related to the determination of technical as well as to the nontechnical facts needed for effective spectrum management.

Some such rule as that proposed here then would have forced the incumbents to choose between accepting full-cost reimbursement to accommodate a newcomer and refusing to accommodate him but then paying "rent" instead—equal to the extra costs imposed by forcing the newcomer to his next-best alternative—whereas newcomers could now choose between the direct purchase of sharing, lending, or preemption rights and the development of a new system or the use of some substitute. While surely not ideal, such an arrangement could help articulate opportunity cost for managers and users alike, both in the government and nongovernment spectrum.

Finally, the imposition of such charges on incumbents might induce

them to "clean up" their old equipment in a more socially efficient manner. For in the absence of alternative users, lax, sloppy spectrum usage is economic: the incumbent merely economizes on his high-cost inputs by substituting low-cost frequency space. Rising opportunity costs of spectrum occupancy, on the other hand, when translated into rental charges equal to the costs imposed on newcomers, will raise the need for reallocation or for technical improvements which did not exist before. For it is economically rational to invest in new equipment so long as the last dollar invested reduces one's bill (for imposing extra costs on newcomers) by an amount greater than this investment.

As envisioned here, then, incumbents would find it worth while to extend the intensive or even the extensive margin of frequency space whenever the extra costs imposed on newcomers through exclusion became sufficiently large, whereas newcomers would choose to develop new systems only where these cost less than sharing, borrowing, or preempting occupied frequencies; viz., where the extra costs imposed on them through exclusion just exceeded the extra costs which incumbents would otherwise incur to accommodate them.

Towards a Proper Role for Frequency Stockpiling. So much then for the accommodation of new users. A second, corollary problem is to determine the proper role for frequency stockpiling. For just as the borrowing of idle space may be a cost-saving option for newcomers faced with harsher alternatives, so the national policy of interim nonuse will inadvertently act to distort the spectrum users' incentives to innovative effort. At least radio grantees favored with lush frequency assignments will, on that account, probably feel less compelled to develop their frequency space or its substitutes than will radio grantees who are relatively more deprived. Indeed the varying frequency shares allocated to different user groups would seem to provide differential incentives for frequency development comparable to those provided by relative resource scarcity and differential prices in other natural resource fields. Moreover, insofar as the spectrum managers' preferences and not the consumers' will presumably govern the distribution of these frequency-developing incentives, any distortion due to stockpiling must be justified explicitly as essential for policy objectives that could not otherwise be attained or attained as efficiently.

There are in any case at least two major ways in which large tracts of frequency space have come to lie idle—notwithstanding the clamor of excluded claimants who would gladly snap them up.

First, old users may be allowed to introduce new techniques without relinquishing space where their older techniques could in fact be activated in an emergency. A good example is the continued retention of underused high frequency circuits by the international common carriers over the North Atlantic route, where the traffic is now largely car-

ried by submarine cable or satellites, notwithstanding the alternative uses to which that space could be put, say, by the aeronautical-mobile services or by certain developing nations. There is clearly some value in buying insurance this way against natural calamities or military disaster. Extra unused capacity may in fact be a small price to pay to meet unknown contingencies, especially when extensive disruption of service raises even a small risk of infinite losses in a nuclear age. However, internal subsidies which ultimately enter the rate base are quite different for this purpose than the overt external subsidies used elsewhere. The upshot is that the balance which the carriers strike between their investment in HF radio circuits, submarine cables, satellites, tropospheric scatter, and microwave links might well differ if the common carriers and their regulators had a clearer picture of opportunity costs. Right now, neither they nor the spectrum manager really knows the full cost of hedging against contingencies this way, and it will therefore be very hard to justify any distorting effects on frequency-development as between different user groups—even in noneconomic terms.

Second, the spectrum manager may deliberately reserve idle space to safeguard the entry or growth of favored classes of users. However, in a field with so many public and private users, changing use patterns, and institutional variations, small wonder that the predictability of future frequency needs will vary considerably from case to case, or that inadvertent long-run stockpiling often results. One good example is the continued stockpiling of unused UHF space for possible future commercial broadcast use notwithstanding the hungry cries of manufacturers, transport companies, and other mobile radio licensees. Another example is the reservation of valuable FM and TV channels for noncommercial, educational service. And a third is the channels reserved exclusively for public safety and military purposes.

Now however lofty the social priorities which frequency stockpiling purports to safeguard, the policy clearly imposes sometimes substantial costs on the excluded parties. The question is surely moot, therefore, as to whether our political decisions would have been quite the same if these costs and the discounted foregone benefits of nonuse had been fully articulated. Or if any distorting effects of stockpiling on the pattern of frequency development had been considered.

For example, educational FM or TV is obviously not the only way to invest in the nation's education. Nor are they necessarily more efficient for this purpose than alternative investments in teachers' salaries and school buildings. To include as the national investment in ETV or EFM only the cost of the equipment used and the program resources, with no further consideration of the value of the reserved frequency space, may be good political strategy. But it could also pro-

duce faulty public investment decisions in view of known alternative uses for the idle frequencies and the relative costs and productivity of alternative educational inputs. To say that radio has a vital contribution to make to public education, public safety, or national defense by no means implies that the composition of relevant federal, state, or local budgets would be quite the same if the full cost of radiocommunication inputs were represented more accurately.

The excess frequency capacity that builds greater flexibility and certainty into our military communications system and the unused frequencies that stand ready for future educational and public safety users, then, both have indisputable social or political appeal. But it is hardly candid to ignore the vital distinction between economically-rational stockpiling by frequency managers and manipulative, strategic hoarding which, like any entry control arrangement, confers privileged market positions on favored grantees, depriving others of comparable benefits. The question at issue, then, is simply whether the radio user or the spectrum manager would continue to stockpile as much space as he now does, for as long as he does, if he had to pay rent equal to its full opportunity costs; or whether the new spectrum user or the old user with new demands would have to invest so heavily in his systems if he could somehow pay incumbents directly to accommodate him under the conditions just set forth.

Such questions yield no easy answers. But even rough and ready answers could mean marked improvement in a field where valuable rights are normally redistributed, if at all, in the frustrating arena of administrative proceedings devoid of serious economic measurement or the murky underworld of behind-the-scenes trade-offs.

DISCUSSION

BEN W. LEWIS: Since I have seen only the two papers that deal with airwaves, I shall confine my direct observations to them, and since I yield to no one in my lack of knowledge about airwaves and my confusion in the presence of more than one knob on a TV set, I shall further limit my observations to some general propositions about technological innovation and regulation. I shall arrange my propositions around the unchanging nature and task of regulation—whether regulation by the market or by formal or informal public governmental measures, or by any combination of these devices and processes. To regulate is to economize: to direct resources where we want them to go, and to distribute claims upon society's goods as we want them distributed. As regulation struggles with or is overrun by technological advance manifesting itself in an ever changing array of new problems and issues, I am constantly impressed by the essential sameness of the problems and issues—by the unchanging nature, the agelessness of the regulatory task. This is worth keeping in mind. It does not insure an instant solution to the variegated assortment of troublesome issues which foist themselves ceaselessly upon the machinery of regulation, but it can help to pinpoint our search for solutions; at least, it can help us not to panic.

What problems do the airwaves pose for regulation? When you work your way through the antennae, the frequencies, the hot and cold running relays, the lines-of-sight, and the very high, the ultra high and the medium-to-well-done broadcasting bands, who is really trying to do what and to whom, and for whose benefit? It appears that society has a naturally limited and very valuable resource at its disposal, whose value has only recently been disclosed; that it would like to have this resource well and fully used; that the resource has a variety of conflicting and hence competing uses; that many, many people equipped with funds and finesse are contending for these uses; that society (on this occasion organized through its government) is also armed with a panoply of ideas, judgments, institutional arrangements, skilled manpower, and experience; that conclusions will be reached and decisions made from time to time and that these will change over time; that necessarily some deeply imbedded interests will be forced to give way; that benefits and satisfactions will be gained and enjoyed at the cost of other benefits and satisfactions foregone; that not everyone will be happy (or equally happy) with society's disposition of these matters; and that society in the large will never be quite sure that it has done as well as it might have done. For the life of me, this sounds like the business of economizing, and I am reminded that the human race has been in this business, facing up to an endless parade of innovations ever since it was, itself, an innovation.

The special features presented by airwaves are, of course, new, but the need to make economizing decisions about naturally limited resources—forest lands, mineral deposits, hydro sites, to suggest a few—is not new. We would

all concede that society's economizing approaches and decisions in these ear lier cases have been less than optimal—that we could have done better in mos instances if we had acted earlier and more wisely, and if we were not eternall picking up the pieces and patching the cracks. But, I would insist that despit the novelty of airwaves, we are old hands at this business, and that we ca and ought to take airwaves in our airborne stride. And, if we keep in mind th essentially simple nature of our task and the qualities (including the limita tions) of our economizing-regulatory instruments and processes, we will prob ably sort out and use our airwaves in an acceptable manner and with accept able results—keeping in mind that we can always unplug the whole business i we cannot locate the right knobs.

We are not exactly novices at this business of public, governmental econo mizing. We may not be too good, but we have been over the falls and througl the rapids. In fact, our record has a few A's as well as E's—together with a lo of C's for effort. How many in this room remember the long night of battl over "public callings," culminating in *Nebbia* v. *New York* when we finall won from ourselves (in the person of the Supreme Court) the right to regu late? Or the skirmishes in the courts and the Congress which finally closed th regulatory gap that existed for so long between the respective reaches of th state police power and the federal power over commerce? Or the legislative judicial, and administrative maneuvering that finally cut through the financia web that encased the power industry in the 1920's and made effective the as tonishing conclusion that the function of power companies is to furnis power? Or the silly to-do that resulted in the establishment of regulation ove public utility activities which managements insisted were managerial in char acter and hence insulated from regulation? Or the interminable and some times very discouraging siege which finally wore down the constitutional prop from under "going value," "spot reproduction cost," "fair value," and "ob served depreciation," and opened the door to a functional approach to rat regulation to those regulatory agencies and authorities sufficiently recovere from the siege to use the door? Every one of these was a regulatory-econo mizing hurdle of very substantial proportions—a crisis. Today, they are regu latory history. For most of the past century, government regulation ha seemed to lumber doggedly on its own trail, sniffing its own scent, baying fo the benefit of its own ears. But, one by one, the little foxes have been overtak en. It would be too much to claim that regulation always gets its man. Som of the men it gets are pretty well washed up by the time they are apprehend ed. Nonetheless, across the board, regulation as an economizing institution ha won its way. Frequently outmaneuvered at the outset, it has made some sur prising recoveries. We have established dominion over ourselves in the calcu lated sorting out and use of resources. Government regulation is in business we are in business.

When, as now—and as always—technological innovation thrusts new mat ters before us for our regulatory-economizing ministrations—airwaves, com petitive transport (a real stinker!), giant oligopolies in basic industries—it i comforting to remember that once upon a time inns and wharves were innova

tions, and, somehow, the common law, unheralded and unadorned, responded to the regulatory issues which they raised.

Regulation, whether dealing with old or new facets of old or new industries, is economizing, and economizing is of one piece. It is the process by which society disposes of matters whose disposition it cannot avoid. Society has no escape from the task of economizing; it can choose its tools; it cannot run from the task and the responsibilities which the task entails. When alternatives are present, failure to choose is to accept, and to accept is to decide. Inaction is, perforce, action. Many of our most important determinations fall into this category. I suggest that this fact holds a lesson for our treatment of airwaves.

I am impressed in these papers by the way in which, and the extent to which, valuable interests which have become vested either by public action (wise or unwise in retrospect) or by public inaction, keep crowding to the fore, to be overtaken by newly developing, tremendously important interests, seeking recognition and acceptance. Regulation is forever being asked to undo packages which it has just tied up.

It should be said again and again that regulation economizing is not only an unavoidable, never-ending business; its practice is unavoidably studded with cost and pain. Economizing decisions have their impact not on cold forms and cold statistics but on warm bodies—sometimes pretty hot bodies. In their very nature, these decisions give joy to some and unhappiness to others. Vested interests present a special problem in the dispensing of joy and sorrow. Vested interests which reflect honest, reasonable expectations born of considered public action are not casually to be overridden. But, neither, in a moving society, can the dead hand of the past be permitted forever to push the buttions on society's transmitters and receivers. An interest becomes vested because society wills it so. In the business of economizing, society must inevitably decide, in its own overriding interest, between conflicting current claims; equally, it must choose, with purpose and clear vision, between vested claims to which, earlier, it has given rise, and present claims which it cannot rationally and decently ignore.

Dealing as it does with imponderables, immeasurables, and uncertainties, regulation—economizing—can rarely be precise and never certain. It should seek precision and certainty, both in its decisions and in the criteria on which its decisions are based. It should be eager in its pursuit of realistic evidence of opportunity costs and of benefits in relation to costs. It should seek quantification, but it must not fall prey to the illusions of certain truth which quantification is all too prone to generate. Its search for certainty and precision should be tempered by the one certainty that it will not be rewarded by any certainty of which it can be cheerfully confident. Our world knows no restful overall economizing equilibrium. In the face of uncertainty, it makes sense for regulation to approach the newly developing manifestations of its old problems slowly and with deliberation; but it must move rapidly enough to avoid being controlled by the events which it should control. Regulation must take some chances.

Our papers talk of the management of the radio and television spectrum. My remarks have ranged more widely to encompass the spectrum of all economic resources. I would match this with a spectrum of all of the economizing arrangements and devices presently ready to our touch and within our capacity to innovate. This is a big job we have set for ourselves by being born. I suggest that a major objective of regulation is technological innovation, and that technological innovation be met by regulatory innovation. I suggest that the confrontation will be easier and more fruitful if we understand that our task is as old—and as new—as mankind.

GEORGE W. WILSON: The lot of the regulator of an industry is indeed a sorry one. He is invariably stuck with an industry constantly in the public eye, the functioning of which affects many interests in diverse ways. The industry is typically dynamic in the sense of rapidly changing, or brand new, closely substitutable, technology, which results in different cost levels, new production functions, and different demand-influencing elements. Finally, the industry is normally composed of multiproduct enterprises with vast gobs of costs that are indivisible which makes pricing on any cost-occasioned basis highly subjective and arbitrary. It is almost a perverse law of nature that requires economic regulation of those segments of the economy that virtually defy effective and efficient control. We seem mainly to try regulating the unregulatable. But since this is not generally recognized, we take out our frustrations on the regulators and accuse them of messing things up even though it is impossible to say whether the inherent messiness has been made worse or better by the overt acts of regulation. So a regulator's lot is an unhappy one. Yet no one has written "The Regulator's Lament." I would recommend that some economists turn to such balladeering rather than, for example, bemoan the fact of regulatory failure to equate prices with long-run marginal costs; it would at least be more fun and certainly less pompous.

The present papers illustrate some of these problems and propensities. Fisher acknowledges that different TV programs are different products; yet laments the failure to establish a "mechanism for charging viewers the long-run marginal production costs of the programs they watch." With technologies changing so rapidly, as both papers effectively show, one wonders what pragmatic meaning is to be attached to the term "long run." With so many cost indivisibilities attached to a "program" the term "marginal" may be equally elusive even if we could agree on a definition of the homogeneous units in which marginal is to be expressed. Levin also appears to be upset by the failure to represent "the full cost of radiocommunication inputs . . . more accurately," although he does not imply allocation to particular programs or other microscopic unit. Levin is also more realistic in implying that rough and ready measures are about all that can be expected. Let me add, however, that I favor more serious attempts at realistic cost finding and more effective experimentation with pricing. Above all, with such rapidly changing industries as radio and television, rate structures and regulation in general must never be

allowed to become as rigid and institutionalized as is the case in transporta tion. Let us have no "Rail Form A," for example.

Both Fisher and Levin suggest the necessity for regulation, because of th inherent limitation of supply as far as channels and spectrums are concerned although this will be less relevant in the future. The task of assignment, th terms on which assignments are made, and the prices charged to users thus tak on crucial importance. Let me say here that the authors, besides describin highly technical operations in a fashion that even this layman could under stand (a real achievement in a short paper), have correctly identified and fo cused upon the issue of internal subsidization. Although this is an area loade with value judgments (i.e., the desirability of robbing A to help B depend largely upon the observer), it is good to have the issue squarely faced. Fishe argues persuasively that restrictions on CATV are essential to preserve loca stations and programming under present arrangements, since these involve in ternal subsidization from network affiliation and broadcasting which in turn i financed by an income transfer from consumers of advertised products to view ers. Ultimately the present arrangements transfer income from consumers o products advertised on TV to local stations. This raises two fundamental an related issues: (1) the desirability of subsidizing local programming and (2 if desirable, how best to finance it. Levin makes a similar kind of point in dis cussing frequency stockpiling, although he seems to be saying that since w know so little about opportunity costs and benefits, the amount of inefficienc involved in, say, reserving frequencies for education or military use cannot b ascertained, a point with which I am sure Fisher would agree. Putting asid our ignorance of these matters and focusing on local programming, what ca be said about it? In the wasteland that is TV, there is clearly something to b said for encouraging local diversity. Yet having observed some of the loca programs, I am not sure whether my utility surface is such that at zero cos more or less disutility is involved in watching professionally produced pap o amateur, but diverse, mediocrity. Too often the option left open to loca programs, like TV itself, is misused. However, it is better to have failed wit opportunities for success available rather than not to have had the options a all; and there is always hope for the future so long as options are not fore closed. In this sense Fisher is more clearly correct.

But if even good local programming and local control would not be eco nomic under a technically efficient pricing system, assuming such a thing to b possible, how should it be encouraged and to what extent? Fisher is not clea whether he would so subsidize, protect, or encourage local programming unde such circumstances. As I read his last paragraph, he argues that it deserve special assistance only because of the present, nonoptimal pricing arrange ments. Earlier he has questioned whether pay-TV should be produced at all i it were not economically viable. I am not sure whether he would apply th same criterion to local programming. In any event, we are here in very murk waters. Is it not a kind of poetic justice that the part of advertising deemed t be economically wasteful contributes to a less obviously wasteful portion of

vast wasteland? Indeed, it is one of the aims of TV advertising to persua the viewer to buy the stuff. Thus, if successful, purchasers of such advertis goods can be made to coalesce with viewers. The existence of a gap implies t lack of success of TV advertising. More fundamentally, is it an inherent fa ure of a market system that although many nonuser beneficiaries would willing to pay something simply to maintain a wider range of options, the sy tem does not provide for such a choice? Or is the situation simply a failure give equal weight to externalities or to measure them adequately?

In any case, I am not convinced that direct subsidization financed from t sources would be much better than from advertising. Certainly one wou need to know more about the income transfers involved than we do at prese to decide which is preferable. Fisher was wise to terminate his discussion terms of "if" such and such, then so and so "might" be better. Indeed, bo papers reveal a refreshing sense of our substantial ignorance of costs a benefits which makes economically rational policy recommendations at bes tenuous business. Levin is a bit more frank in raising questions to which have as yet few answers, and he shows more awareness of the problems asso ated with obtaining reasonable estimates. Both papers stress the need for more effective and flexible pricing scheme and thus, explicitly or implicit cry out for more accurate measures of costs and benefits. I would not only co cur but would urge them to get on with it. The time has long since pass when economists could simply recommend that prices be based on costs in r ulated industries and then blithely depart from the committee room leav the grubby details to semiliterate statisticians. There are problems of c finding and cost concepts that, unless squarely faced by competent economis will frustrate any such recommendations. The role of the economist in affluent, welfare state is not ended, as some have alleged. In many respects has barely begun. The papers in this session clearly indicate not only one the paths of future contributions but also how much remains to be done.

WILLIAM IULO: It should be made clear at the outset that I ha no particular quarrel with the major points made by Professor Hughes abo the National Power Survey nor with his summary of the conjuncture of e nomic, technological, and institutional factors that led up to and envelop that endeavor by the FPC. Indeed, if I were presenting a paper on this subj rather than discussing one I would make many of the same points.

That is to say, given the present organization and technology of the elect power industry: (1) there are large, apparent, and underutilized economies scale in the generation and transmission of electric power; (2) there justification and even necessity for a geographically broader, tempora longer, and more public-interested perspective than that produced by the ty cal industry planner; (3) that industry participation was an invaluable, if a vital, part of the Power Survey approach; (4) that the side-stepping major institutional issues contributed to the conduct and the acceptance of results of the Power Survey. Such a large area of agreement would seem

leave little room for further discussion, but there are two aspects of the paper that raise questions in my mind.

First, I question whether Professor Hughes believes as strongly in the technological determinism of the electric power industry as his paper indicates. The assertion that regulation should lead wherever the technological contours of the future can be foreseen bothers me because of its seeming overfull acceptance of technological direction. Technology should be the servant, not the master of economic and social affairs; its course, in fact, is more determined by man than the reverse. In this same vein, I question whether the technological destiny of the electric power industry is either so immutable or so clear that we can accept as ordained the local natural monopoly organization associated with central station service. The success (albeit limited to date) of the "total power" concept of the natural gas industry indicates that even currently, under some conditions, the self-supplying of electric power in connection with other energy needs is privately advantageous vis-à-vis central station service. In the future, given appropriate improvements in the technology of the fuel cell and/or the battery, it is possible to envision electric power self-supply and central station service competing for a large share of the industrial and residential markets with all the agony and woe that characterize railroad-trucker competition.

The second area of questioning relates to the exclusion of major institutional and public policy issues not only from the survey itself but also from Professor Hughes's paper (except to the extent that he suggests this be done). If the technological destiny of electric power is so clearly laid out as it would appear from his paper, then certain broad changes of policy are implied. Examples of such changes would include: (1) the discouragement of generation by all but a relatively few regional producers with heavy interconnections among these few; (2) the active encouragement of merger and/or other organizational concentrations in this industry, including pooling arrangements; (3) perhaps the creation of a Federal Reserve Power System, organized regionally, owned by its member systems and having sole responsibility for electric power generation and transmission in its region with overall public policy goals established by a central Board of Governors. However, if there are some doubts as to the permanence and nature of the relationships among the technology, the economics, and the public policy of electric power, it may be wiser to move only marginally in these directions, pending replications of the Power Survey at more or less regular intervals.

I would like to make one comment with respect to Professor Fisher's paper or perhaps it is more accurate to say that the comment relates to the essential validity of the presumption by the FCC that locally originating, live, public service programming is a desirable and fairly important end.

It may be assumed that the supply of talent required to produce public service programming that will attract a substantial segment of the viewing public is a relatively scarce good. If this is true, then FCC pressure for more local programs may not be producing real diversity in programming but merely a

ind of "eye-service" programming to satisfy the FCC examiner by enabling he broadcaster to point to the hours of broadcast time he has devoted to the ocal evangelist or other easily programmed event. Pressure for more local rograms may be less effective in producing diversity in public service programming than pressure for better network provided public service programs nd fewer but better local ones.

It seems to me that if the goal of more local live public service programming is invalid, then so is a good part of the argument for stringent CATV egulation. In any event I agree with Professor Fisher that direct subsidy is kely to be a more efficient incentive to either local or network public service rogramming than the indirect approach of restricting CATV competition.

THE ECONOMICS OF EDUCATION

EDUCATION AND THE DISTRIBUTION OF EARNINGS

By Gary S. Becker *and* Barry R. Chiswick
Columbia University and National Bureau of Economic Research

I. *Introduction*

The history of interest among economists in the distribution of income is as long as the history of modern economics itself. Smith, Mill, Marshall, and others recognized that many areas of considerable economic importance were affected by it. Although poverty, for example, was partly defined in absolute terms, they recognized that each generation's "poor" are mainly those significantly below the average income level. In addition to poverty, the degree of opportunity, aggregate savings and investment, the distribution of family size, and the concentration of private economic power were thought to be affected.

How does one explain then that in spite of the rapid accumulation of empirical information and the persisting and even increasing interest in some of these question, such as poverty, economists have somewhat neglected personal income distribution during the last generation?[1] In our judgment the fundamental reason is the absence, notwithstanding some ingenious and valiant efforts, of a theory of income distribution that both articulates well with general economic theory and is useful in explaining differences among regions, countries and time periods. Some earlier work[2] by one of us led to the belief that an analysis of investment in human capital provides a theory of income distribution that satisfies both desiderata.

This is a report on a National Bureau of Economic Research study in progress[3] that is developing such a theory and applying it to a variety of evidence. Not only is the report preliminary, but brevity of space requires that many details and proofs be skipped and the discussion concentrated on a few highlights. We expect to publish the full study before too long, which would permit our methods and conclusions to be examined more closely.

[1] As one test of this statement, try to find many textbooks on economic principles that pay it much attention.

[2] See G. S. Becker, *Human Capital* (Columbia Univ. Press for N.B.E.R., 1964), especially pp. 61–66.

[3] Financed by the Carnegie Corp. of New York. We are greatly indebted to Linda Kee for valuable research assistance, and to members of the Labor Workshop at Columbia University for useful comments.

II. *Theory*

The total earnings of any person after he has finished investing in human capital can be said to equal the sum of the returns on his investments and the earnings from his "original" human capital. If returns could be treated as constants for essentially an indefinitely long period, this relation could be expressed as

$$E_i = X_i + \sum_{j=1}^{m} r_{ij}C_{ij}, \tag{1}$$

where C_{ij} is the amount spent by the ith person on the jth investment, r_{ij} is his rate of return on this investment, and X_i are the effects of the original capital.[4] Note that our analysis directly applies to earnings alone, which is just a part, although the dominant one, of total income. While the framework developed here could also be usefully applied to property income, we have not done any empirical work on such income and ignore it in the rest of the paper.

The point of departure for our approach, which integrates it with economic analysis in other fields, is an assumption that the amount invested in human capital results from optimizing behavior: each person is supposed in effect to invest an amount that maximizes his economic welfare. This assumption permits the investment decision to be analyzed in terms of the following familiar figure. The curve D shows a person's marginal rate of return on an additional dollar of investment, while S shows his marginal "interest" cost. Equilibrium is assumed to occur at p, where the total amount invested would be OC, and the (gross) income on this investment would be $ODpC$, represented in discrete form by the term $\sum r_{ij} C_{ij}$ in equation (1). In this framework, therefore, the distribution of earnings is simply determined by the shape and distribution of the supply and demand functions,[5] and we now dwell a while on these determinants.

Legal and other obstacles to financing investments in human capital have been a significant "institution" in Western societies. As a result, these investments have been financed either by gifts from parents and others, reduced consumption during the investment period, or various kinds of loans. Since financing usually becomes more difficult as the amount invested increases because gifts become less available, reduced consumption more onerous, and risks to lenders greater, the effective supply curve of funds, say S in the figure, would be positively inclined, its elasticity measuring the rate of increase of these difficulties.

[4] See Becker, *op. cit.*, Chap. III.

[5] For the moment we ignore the distribution of X_i; it is incorporated into the analysis in section III.

Supply and Demand for Investment in Human Capital
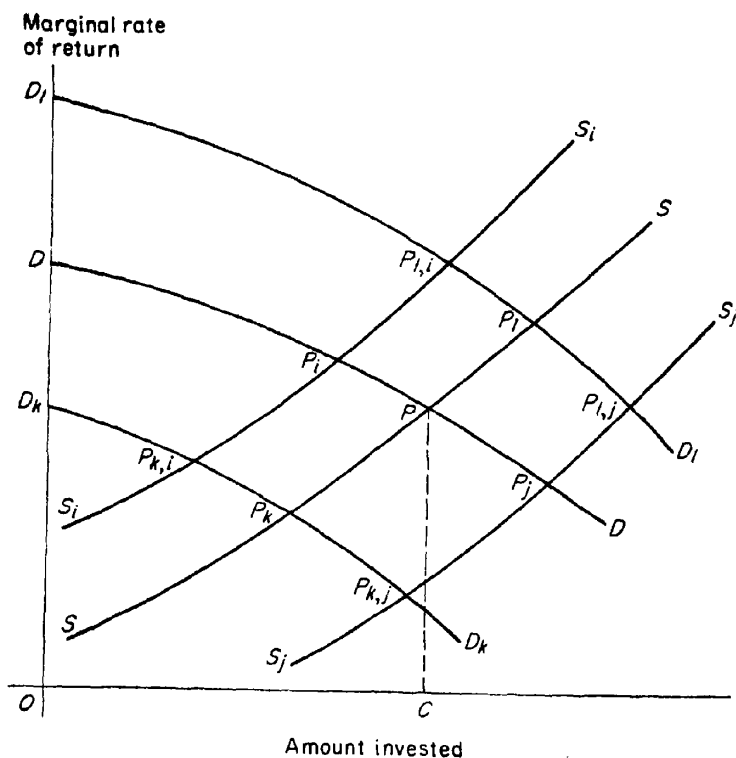
FIGURE 1

One factor decreasing the marginal rate of return, at least eventually, with increases in the amount invested is a presumed diminishing marginal product from adding more capital to a fixed human body. Moreover, increased investment usually requires a longer investment period, and with a finite lifetime the marginal rate of return would tend to be inversely related to the length of this period.[6] The rates of return to any person depend, however, not only on his investments but also on those by others and on the derived demand for persons with different kinds of human capital. For example, a college education might yield a very high payoff if few persons manage to get one and if those who do are in great demand. Consequently, depending on general supply and derived demand conditions, the marginal rate of return might well

[6] See Becker, *op. cit.*, pp. 49–52.

increase over different investment intervals. Although, therefore, the curve D might not always decline,[7] our analysis requires only a decline relative to S, especially around the point of intersection p, and that seems plausible enough.

Let us turn to the distribution of these curves and thus to the distribution of earnings. Since the income and wealth of parents, the willingness to forego consumption, and the availability of scholarships and loans vary from person to person, the supply of funds would also vary, as illustrated by the curves S_i, S and S_j in the figure. If the demand curve D was the same for everyone, the equilibrium positions would lie along D at the points of intersection, given in the figure by p_i, p, and p_j. Knowledge of the various equilibrium positions would permit an "identification" of the demand for funds curve D.[8]

The distribution of earnings would be determined by the distribution of the areas under D, which in turn would be determined by the distribution of supply curves, their shape and the shape of D. For example, if the marginal rate of return was constant so that D was horizontal, earnings would have the same inequality and skewness as the amount invested. If, however, marginal rates decreased, the inequality and positive skewness in earnings would be less than that in investments because large investors would receive lower rates of return; conversely if marginal rates increased.[9]

More usually the demand for funds would also vary, probably significantly, because of differences in "ability," attitudes toward risk, and other personal characteristics. The figure shows three demand curves D_k, D, and D_l, with D_l, reflecting the most "ability," and D_k the least. If the supply curve S was the same for everyone, the equilibrium positions would lie along S at the points of intersection, given in the figure by p_l, p, and p_k, and knowledge of the various positions would permit an "identification" of S. If S was positively inclined as in the figure, the inequality and positive skewness in earnings would exceed that in investments because large investors would receive higher rates of return.[10]

[7] Since the marginal rate of return to any person would depend not only on his investments but also on those by others, the demand curve D would be defined only for given amounts invested by others.

[8] This kind of technique has been used by G. Hanoch in his "Personal Earnings and Investment in Schooling" (unpublished Ph.D. dissertation, Univ. of Chicago, 1965), Chap. II.

[9] If D was a straight line, the average as well as marginal rate of return would be linearly related to the amount invested, and earnings from human capital would be

$$\bar{r}_c C = (a + bC)C = aC + bC^2,$$

where C is the total amount invested, $\bar{r}_c$ its average rate of return, and a and b are constants. If $b=0$, earnings simply equals aC; if $b\neq 0$, the term bC^2 either increases or reduces the inequality and positive skewness in earnings as $b \gtrless 0$.

[10] If S was linear as well as positively inclined, the average rate of return would be linearly and positively related to the total amount invested, and earnings from human capital would be

$$\bar{r}_a C = (e + fC)C = eC + fC^2,$$

where $f>0$.

In the usual case where supply and demand curves both vary, the different equilibrium positions could still be found at the intersections of the relevant curves, but knowledge of these positions would no longer be sufficient to "identify" either set of curves. In addition to the factors already discussed, the distribution of earnings would now also depend on the correlation between supply and demand conditions. These conditions might well be positively correlated, say because of a positive correlation between ability or the psychic earnings received from human capital with either parental wealth or scholarships; in the figure the demand curve D_l could be associated with the supply curve S_j, D_k with S_i, and D with S. The resulting equilibrium positions p_{lj}, p, and p_{ki} indicate sizable dispersions in rates of return and amounts invested, and a strong positive relation between them. If S_i was associated with D_l and S_j with D_k, supply and demand conditions would now be negatively correlated, and the resulting equilibrium positions would be p_{li}, p, and p_{kj}. The inequality and skewness in earnings would be reduced because the negative correlation between these conditions would reduce the investment and earnings of persons with favorable demand conditions, say D_l, and increase that of persons with unfavorable conditions, say D_k.

Before passing on to a quantitative implementation of this model, we might dwell a little on one interesting implication. The often discussed but seldom defined concept of "equality of opportunity" can be rigorously defined in our framework as a situation in which low parental wealth and other supply disadvantages were sufficiently offset so that the effective supply curve of funds was the same to everyone.[11] One way to achieve this would be to make investment in human capital a free good through subsidies from public or private agencies; all supply curves, in effect, would then lie along the horizontal axis.[12] Our definition of equality of opportunity would imply not equal investment but equal opportunity to invest, the actual amount depending on ability and other personal characteristics (see the points p_k, p, and p_l in the figure). The elimination of unequal supply conditions would reduce the inequality in investments unless supply and demand conditions had been sufficiently negatively related. On the other hand, it would increase the positive correlation between the amount invested and its rate of return unless supply and demand conditions had been sufficiently positively related.

[11] One might also want to offset some of the forces, such as discrimination or nepotism, making for differences in demand curves.

[12] Free public schools are not a perfect example, since foregone earnings, often an important cost of schooling, are not subsidized. The GI Bill, on the other hand, does cover at least some foregone earnings by providing living allowances as well as tuition. Legislation making schooling compulsory is still another and quite different example, for some persons may be forced to continue in school longer than they would like (or even than is good for them). Incidentally, because of space limitations our discussion in this paper must abstract from forces, such as compulsory school legislation or the rationing of school places, that make the actual amount invested differ from the desired amount.

III. *A Statistical Formulation*

The contribution of human capital to the distribution of earnings could be easily calculated empirically if the rates of return and investments in equation (1) were known. Although information on investment in human capital has grown significantly during the last few years, it is still limited to aggregate relations for a small number of countries. Much more is known about one component of these investments; namely, the period of time spent investing, as given, for example, by years of schooling.

To utilize this information we have reformulated the analysis to bring out explicitly the relation between earnings and the investment period. The principal device used is to write the cost of the jth "year" of investment to the ith person as the fraction k_{ij} of the earnings that would be received if no investment was made during that year. If for convenience r_{ij} in equation (1) is replaced by $\bar{r}_j + r^*_{ij}$, where $\bar{r}_j$ is the average rate of return on the jth investment and r^*_{ij} is the (positive or negative) permium to the ith person resulting from his (superior or inferior) personal characteristics, then it can be shown that equation (1) could be rewritten as

$$(2)\ E_i = X_i[1 + k_{i1}(\bar{r}_1 + \overset{*}{r}_{i1})][1 + k_{i2}(\bar{r}_2 + \overset{*}{r}_{i2})] \cdots [1 + k_{in_i}(\bar{r}_{n_i} + \overset{*}{r}_{in_i})]$$

where n_i is the total investment period of the ith person.[13] If the effect of luck and other such factors on earnings is now incorporated within a multiplicative term e^{u_i}, the log transform of equation (2) is

$$(3)\qquad \log E_i = \log X_i + \sum_{j=1}^{n_i} \log [1 + k_{ij}(\bar{r}_j + \overset{*}{r}_{ij})] + u_i.$$

By defining $X_i = \overline{X}\ (1+\alpha_i)$, where α_i measures the "unskilled" personal characteristics of the ith person, and $k_{ij} = \bar{k}_j + t_{ij}$, where $\bar{k}_j$ is the average fraction for the jth investment, and by using the relation

$$(4)\qquad \log [1 + k_{ij}(\bar{r}_j + \overset{*}{r}_{ij})] \approx k_{ij}(\bar{r}_j + \overset{*}{r}_{ij}),$$

equation (3) could be written as

$$(5)\qquad \log E_i \approx a + \sum_{j=1}^{n_i} \bar{r}'_j + v_i,$$

where $a = \log \overline{X}$, $\bar{r}'_j = \bar{k}_j \bar{r}_j$, and

$$(6)\qquad v_i = \log (1 + \alpha_i) + \sum_j k_{ij}\overset{*}{r}_{ij} + \sum_j t_{ij}\bar{r}_j + u_i.$$

[13] We cannot take space to give a proof here; the interested reader can find a proof for a somewhat special case in Becker, *op. cit.*, p. 64.

The term v_i largely shows the combined effect on earnings of luck and ability. If the $\bar{r}'_j$ was the same for each period of investment, the equation for earnings is simply

$$\log E_i \approx a + \bar{r}'n_i + v_i \tag{7}$$

If $\bar{r}'$, the average rate of return adjusted for the average fraction of earnings foregone, and the investment period n_i were known, equation (7) could be used to compute their contribution to the distribution of earnings. For example, they would jointly "explain" the fraction

$$R^2 = (\bar{r}')^2 \frac{\sigma^2(n)}{\sigma^2(\log E)} \tag{8}$$

of the total inequality in earnings, where $\sigma^2(n)$ is the variance of investment periods, and $\sigma^2(\log E)$ is the variance of the log of earnings, the measure of inequality in earnings.[14] Ability and luck together would "explain" the fraction $\sigma^2(v)/\sigma^2(\log E)$, and the (perhaps negative) remainder of the inequality in earnings would be "explained" by the covariance between ability, luck and the investment period.

Even equations (5) and (7), simplified versions of (2), make excessive demands on the available data. For one thing, although the period of formal schooling is now known with tolerable accuracy for the populations of many countries, only bits and pieces are known about the periods of formal and informal on-the-job training, and still less about other kinds of human capital. Unfortunately, the only recourse at present is to simplify further: by separating formal schooling from other human capital, equation (5) becomes

$$\log E_i = a + \sum_{j=1}^{q_i} \bar{r}'_j S_j + v'_i, \tag{9}$$

where $\bar{r}'_1$ is the adjusted average rate of return on each of the first S_1 years of formal schooling, $\bar{r}'_2$ is a similar rate on each of the succeeding S_2 years of formal schooling, etc.;

$S_i = \sum_1^{q_i} S_j$ is then the total formal schooling years of the ith person, and

$$v'_i = v_i + \sum \bar{r}'_k T_k \tag{10}$$

includes the effect of other human capital.

A second difficulty is that although an almost bewildering array of

[14] Note that this measure, one of the most commonly used measures of income inequality, is not just arbitrarily introduced but is derived from the theory itself.

rates of return have been estimated in recent years, our empirical analysis requires many more. Additional estimates could be developed by making equation (9) do double duty: first it could be used to estimate the adjusted rates and only then to show the contribution of schooling, including these rates, to the distribution of earnings. If the S_j and v' were uncorrelated, an ordinary least squares regression of log E on the S_j would give unbiased estimates of these rates, and, therefore, of the contribution of schooling. If, however, the S_j and v' were positively or negatively correlated, the estimated rates would be biased upward or downward, and so would the estimated direct contribution of schooling

Some components of v' are probably positively and others are negatively correlated with years of schooling, and the net bias, therefore, is not clear a priori. It is not unreasonable to assume that α_i and u_i in equation (6) are only slightly correlated with the S_j. The r_{ij}^* term in (6), on the other hand, would be positively correlated with the S_j[15] since the theory developed earlier suggests that persons of superior ability and other personal characteristics would invest more in themselves. Some empirical evidence indicates a positive correlation between years of schooling and the amount invested in other human capital.[16] The term v' depends, however, on the correlation between years of schooling and years invested in other human capital, a correlation which might well be negative. Certainly persons leaving school early begin their on-the-job learning early, and possibly continue for a relatively long time period (see fn. 17). Finally, one should note that random errors in measuring the period of schooling would produce a negative correlation between the measured period and v'. Although the correlations betweeen the S_j and these components of v' go in both directions and thus to some extent must offset each other, a sizable error probably remains in estimating the adjusted rates and the contribution of schooling to the distribution of earnings.

IV. *Empirical Analysis*

The sharpest regional difference in the United States in opportunities and other characteristics is between the South and non-South, and the following table presents some results of regressing the log of earnings on years of schooling separately in each region for white males at least age 25. Adjusted average rates of return have been estimated by these regressions separately for low, medium, and high education levels. As columns 1, 2, 6, 7 and 8 indicate, the adjusted rates at each of these school levels and the variances in the log of earnings and in years of schooling are all a fair amount larger in the South. Moreover, these

[15] That is, unless a negative correlation between k_{ij} and r_{ij}^* was sufficiently strong.
[16] See Becker, *op. cit.*, p. 89.

RESULTS OF REGRESSING NATURAL LOG OF EARNINGS ON EDUCATION FOR 1959 EARNINGS OF WHITE MALES AGED 25–64 IN THE SOUTH AND NON-SOUTH

	Variance of Natural Log of Earnings (1)	Variance of Education (2)	Average Natural Log of Earnings§ (3)	Average Education (4)	Inter-cepts§ (5)	Adjusted Rates of Return*			Adjusted Coefficient of Determination (R^2)	Residual Variance in Natural Log of Earnings (10)
						Low Education (6)	Medium Education (7)	High Education (8)	Unadjusted R^2 (9)	
						(standard errors)†				
Non-South	.42	11.28	1.66	10.78	1.09	.05	.06	.08	.07	.39
					(.67)	(.09)	(.06)	(.06)	.11	
South	.55	15.23	1.43	9.96	.66	.07	.09	.09	.16	.46
					(.50)	(.08)	(.07)	(.06)	.20	

* "Low" education is defined as 0–8 years of school completed, "medium" as 8–12 years, and "high" as more than 12 years.

† In calculating the standard errors and the the adjusted coefficients of determination, the number of degrees of freedom was assumed to equal the number of cells minus the number of parameters estimated. The true number is clearly somewhat greater than this.

§ Earnings measured in thousands of dollars.

SOURCE: *United States Census of Population: 1960, Subject Reports-Occupation by Earnings and Education* (Bureau of the Census, Washington, 1963), Tables 2 and 3.

differences in schooling and rates exceed the difference in earnings, for as column 9 shows, the coefficient of determination, or the fraction of the variance in the log of earnings "explained" by schooling, is considerably higher in the South. The regional difference in earnings does not entirely result from schooling, however, for column 10 shows that the "residual" inequality in earnings is also larger in the South.

These results can be given an interesting interpretation within the framework of the theory presented in section II. The greater inequality in the distribution of schooling in the South is presumably a consequence of the less equal opportunity even for whites there and would only be strengthened by considering the differences in schooling between whites and nonwhites. The higher adjusted rates of return in the South[17] are probably related to the lower education levels there, shown in column 4, which in turn might be the result of fewer educational opportunities.

The residual v' is heavily influenced by the distributions of luck and ability, which usually do not vary much between large regions. Therefore, greater rates of return and inequality in the distribution of schooling would go hand in hand not only with a greater absolute but also with a greater relative contribution of schooling to the inequality in earnings. The residual is also influenced, however, by investment in other human capital. Since the rates of return to and distribution of these investments would be influenced by the same forces influencing schooling, the absolute, but not relative, contribution of the residual to the inequality in earnings would tend to be greater when the absolute contribution of schooling was greater. Consequently, our theory can explain why both the coefficient of determination and the residual variance in earnings are greater in the South.

In order to determine whether these relations hold not only for the most extreme regional difference in the United States but also for more moderate differences, similar regressions were calculated for all fifty states. To avoid going into details at this time let us simply report that the results strongly confirm those found at the extremes: there is a very sizable positive correlation across states between inequality in adult male incomes, adjusted rates of return, inequality in schooling, coefficients of determination, and residual inequality in incomes, while they

[17] Higher rates of return to whites in the South have been found when estimated by the "present value" method from data giving earnings classified by age, education, and other variables (see Hanoch, *op. cit.*, Chap. IV). Although estimates based on the present value method are also biased upward by a positive correlation between ability and schooling and downward by errors in measuring school years, they are less sensitive to the omission of other human capital (see Becker, *op. cit.*, pp. 88–90). Consequently, the fact that Hanoch's estimates are almost uniformly higher than ours (after adjustment for the k_i) suggests, if anything, a negative correlation between school years and the years invested in other human capital. This could also explain why Hanoch's rates tend to decline with increases in years of schooling while ours tend to rise.

are all negatively related to the average level of schooling and income. Whereas only about 18 percent of the inequality in income within a state is explained, on the average, by schooling, the remaining 82 percent explained by the residual, about one-third of the differences in inequality between states is directly explained by schooling, one-third directly by the residual and the remaining one-third by both together through the positive correlation between them.

Similar calculations have also been made for several countries having readily available data: United States, Canada, Mexico, Israel and Puerto Rico (treated as a country). Again there is a strong tendency for areas with greater income inequality to have higher rates of return, greater schooling inequality, higher coefficients of determination, and greater residual inequality. While there is also a tendency for poorer countries to have lower average years of schooling, greater inequality in income, etc., there are a couple of notable exceptions. For example, Israel, for reasons rather clearly related to the immigration of educated Europeans during the 1920's and 1930's, had unusually high schooling levels and low inequality in earnings until the immigration of uneducated Africans and Asians after 1948 began to lower average education levels and raise the inequality in earnings.

V. *Summary and Conclusions*

This paper has developed and applied to several bodies of evidence a theory of the distribution of earnings. The principal attraction of the theory is that, unlike most other approaches to income distribution, it does not consist mainly of mechanical curve fitting or *ad hoc* probability mechanisms, but rather relies fundamentally on maximizing behavior, the basic assumption of general economic theory. Each person is assumed in effect to maximize his economic welfare by investing an appropriate amount in human capital, and the distribution of earnings is determined by the distribution of investments and their rates of return. These determinants are in turn related to various "institutional" factors which also play an important part in our theory: inheritance of property income, equality of opportunity, distribution of abilities, subsidies to education, and other human capital, etc.

Limitations of the data available have reduced the scope of the empirical analysis to investment in formal education as measured by years of schooling. Evidence from states and regions within the United States and from several countries indicates that schooling usually explains a not negligible part of the inequality in earnings within a geographical area and a much larger part of differences in inequality between areas. These and other findings are generally quite consistent with the implications of the theory.

The body of economic analysis desperately needs a reliable theory of the distribution of income. While this report is preliminary and more work is in progress, our approach seems to offer considerable promise of filling that need. In any case we hope to have demonstrated that a theory of income distribution need not be a patchwork of Pareto distributions, ability vectors, and the toss of a coin, but can wear clothing as neat as that worn by the theory of households and firms.

INVESTMENT IN THE EDUCATION OF THE POOR: A PESSIMISTIC REPORT*

By EUGENE SMOLENSKY
University of Chicago

First I will propose a new approach to the measurement of poverty. This approach focuses attention upon the necessary conditions for declining income inequality, some of which will be discussed, therefore, in sections II and III. Finally, the analysis will be extended to evaluate the prospects for reducing the incidence of poverty by adding further financial incentives for completing high school.

As I view the problem, poverty is not likely to decline as a serious social issue in the foreseeable future,[1] even though the real income of the poor is likely to be rising all the while.

I. *The Poverty Line as a Signal*

Twice during the past thirty years, a poverty line has been drawn to guide federal policy. In constant prices, the recent poverty line is about 53 percent above the earlier one [16]. Since this percentage increase is similar to the independently estimated rise, over the same period, in the cost of a varying minimum-decency market basket,[2] the rise has a substantial rationale: It reflects technical revisions in minimal nutritional requirements and other needs as seen by experts, taste changes, and product substitutions due to relative price changes and the introduction of new products. These market baskets, however, embody a rise in real income, too, and since no criteria for admitting a rise in real income into the definition of poverty has been agreed upon, the resulting change in the line over time poses problems of interpretation. I would like to suggest an alternative approach to setting the poverty line which would be free of this ambiguity, and which would also,

* I am greatly indebted to Millard F. Long. If he believed the results, he would properly be the coauthor. Paul Davidson, James Kindahl, Albert Rees, and Joel Segall contributed significantly to reducing the number of errors embodied herein. Jude Laspa was my very capable research assistant. Some of these views, which are strictly my own, were developed while working on quite a different problem, under contract with the Office of Economic Opportunity, and I am indebted to that Office.

[1] A few comments on what I am not saying seems appropriate. I am not saying that it is an unwise policy to promote investment in human capital. Nor am I asserting that alternative goals are less important than poverty reduction. Therefore I am not asserting that current policy is unwise. I cannot even assert that current policy will positively fail to reduce poverty. I am merely pessimistic and my pessimism extends only to a policy which calls for extending years of school by adding financial incentives to reduce the proportion of families in poverty.

[2] The market basket was for New York City.

I feel, better serve policy. More precisely, I will propose a rational basis for changing the real content of the poverty line over time in a growing economy.

The disutility that arises from the persistence of poverty is not confined to the poor, and the effective demand for federal antipoverty programs does not emanate solely, nor perhaps even primarily, from a concern for equity. The primary objective may very well be efficiency in the Pareto sense. If this be so, a poverty line which considers only the needs of the poor may not be the most pertinent guide to federal policy. Perhaps the most relevant requirement of a poverty line is that it serve as an index of the disutility to the community of the persistence of poverty.

Take as an example the externality in consumption due to slums. Satisfactions flow from two separate aspects of a house. One is the quality of service the house itself provides to its occupants. The other aspect is that the quality of the neighborhood in which the house stands may affect the satisfactions the dwelling unit provides to its occupants. The existence of substandard housing can, therefore, reduce the satisfactions of some who do not live in them. It follows, therefore, that since poverty is generally a precondition for slums, the elimination of poverty, and with it slums, will raise the real incomes of some who are not poor. These externality considerations suggest an approach quite different from the "needs" approach to the measurement of poverty. They suggest that the poverty line take into consideration the external benefits of raising families above the poverty line.

Imagine a community in which none may change address, but in which the quality of each house can be improved (at constant cost), including raising substandard housing to standard quality. As incomes rise, all individuals will want both the quality of their own housing to be improved and the proportion of substandard dwellings occupied by others in the community to decline. Only the former, however, can be directly affected through the market by each family, for the latter depends upon the actions of others. The change in the quantity of substandard housing will therefore depend in large part upon the income elasticity of those who live in substandard housing and upon the change in their income. The welfare of the whole community is, therefore, tied to the ratio of the growth of income of the poor to the growth in median income of the community and to the desires of the community for raising the quality of its neighborhoods relative to the desires of the poor to raise the quality of their own housing. These considerations taken together suggest the alternative approach to measurement. The rise in the poverty line over time should be such that were the incomes of the poor to rise at the same rate, the poor would, by volun-

tary market actions, satisfy the evolving desires of the community due to changes in its income level for those things in which there are externalities in consumption; e.g., housing.

It is feasible to implement this alternative ("consensus") approach,[3] but there are many problems of concept and of estimation.[4] Were these issues resolved and a new poverty line drawn, it would, I suspect, rationalize the recent increase in public concern over poverty.

The needs approach is something of an anachronism, for surely the material needs of the poor are being more adequately met then before. For public concern to increase at a time when a social problem is decreasing in significance appears to be irrational [14]. If poverty is not a declining social problem, then, of course, the increased public concern is understandable. Although the needs poverty line has fallen relative to per capita income over the past three decades [16], it is my hunch that if the consensus poverty line had been estimated, it would have risen relative to average income over the same period, and that consequently, there would not, in recent years, have been a decline in the incidence of poverty.

Let me return to my housing example. There is some evidence to suggest that the income elasticity of demand for better neighborhoods at the median of the personal income distribution is greater than the elasticity of demand for standard housing among the poor.[5] Differences in the relevant income elasticities can be offset by compensating differences in the income change which they modify, however. In my example, therefore, the preferences of the majority for better neighborhoods could be satisfied by voluntary action if the incomes of the poor were to rise more rapidly (by the ratio of the elasticities) than median income.

One caveat: Even if the elasticities are as I suspect, it does not fol-

[3] In one instance, for example, Muth [10] obtained the following estimate:

$$y(10^{-3}) = -.25 \ln s + \dots$$

where: y = the median income of a census tract in South Chicago in 1950

s = the percent of substandard houses in the tract.

Suppose, to take some arbitrary numbers, the median income of the poor to be $2,725 and the median income of the sample to be $3,870. If all incomes rose by 10 percent, the desired decline in substandard housing would be 15.4 percent. For the poor to voluntarily reduce the proportion of substandard houses by 15.4 percent, however, the median income of the poor would have to have advanced by 14 percent. That would imply that the poverty line, whatever its initial and perhaps arbitrary value, be advanced 14 percent.

[4] For example, should the line be based on a single class of goods or on many? If many, what should delimit the set and how are the various groups of commodities to be combined? The current needs line was derived from essentially one class of commodities—a food basket. At this juncture shelter may be a considerably more meaningful commodity on which to base a poverty measure than food, even on the needs criteria.

[5] See footnote 3 above.

low that the most efficient solution to the poverty problem is for income inequality to decline over time, simply because the poverty line rises relative to median income. The policy-makers would have a meaningful signal, however, for, given a consensus definition of poverty, a rise in the proportion of families in poverty would imply that with the same rise in income differently distributed, everyone could have been made better off, without anyone being made worse off and it would then be appropriate to consider community action.[6]

What are the circumstances under which the market economy will generate a fall in the proportion of families in poverty, if the concensus approach were accepted? More specifically, when is a general rise in income distributed in a way that avoids the threat to Pareto optimality at issue here? If my hunch about the magnitudes of the relevant elasticities is correct, a necessary, but not necessarily sufficient, condition is for income inequality to decline. In the next section I turn to a major aspect of declining income inequality: the circumstances in which the wages of the poor rise relative to the average wage.

II. *The Historical Record Points Toward Full Employment*

We know from many sources (e.g., Kuznets [9], Goldsmith [7], and Kravis [8]) that most measures of income inequality in the United States exhibit long and short swings around a trend toward convergence during this century. The trend, in turn, is mainly attributable to a sharp decline in income inequality during the second World War which has survived subsequent cycles. Between 1935 and 1962, to report a somewhat more pertinent fact, the proportion of families in poverty (using the needs definition) fell by 43 percent with two-thirds of that decline occurring between 1935 and 1947. Over the same period, however, the needs poverty line fell relative to per capita income. If the poverty line had risen proportionately with per capita income, only the decline from 1935 to 1947 would have been observed [16] [6].

A large part of the convergence during the second World War is proximately attributable to convergence within wage income [15]. At the same time, occupational wage differentials narrowed relatively, and the theory most often invoked to explain that fact, suitably amended, explains, to a considerable extent, the significant interrelationships between a wartime economy and declining wage differentials. That analysis emphasizes only one aspect of a wartime economy—the historically low levels of unemployment which are achieved—but an important policy role for wage and interest rate controls is also implicit in the explanation.

[6] Since the consensus poverty line has no direct implication for policy, it would probably be best to confine its use to policy discussion within the government.

III. *Full Employment and Declining Wage Differentials: The Trickle-Down Mechanism*[7]

In a series of articles, Reder has developed a model to explain relative convergence in occupational wage differentials [11] [12] [13]. In the following paragraphs his argument is restated and, I believe, strengthened.

Normally employers believe that they can hire all the workers (of each grade) that they wish at prevailing wage rates, although they recognize that to do so will be to raise other costs (e.g., search costs). For each skill level above the unskilled, the labor supply is perceived to be infinitely elastic at the prevailing wage rate (but not prevailing unit labor costs), because increased demand can be responded to most profitably by drawing from among the disguised unemployed, or by upgrading less skilled workers through training, or by the dilution of job specifications, or by extending the time during which job vacancies go unfilled.[8] An increased demand for unskilled workers can normally be met, the real wage unchanged, by drawing upon the involuntarily unemployed and domestic workers. The money-wage rate for unskilled labor does not fall and involuntary unemployment persists because there exists a wage floor below which corporations and governments will not pay. Reder calls this wage floor "the social minimum."[9]

During periods of prolonged expansions of aggregate demand (e.g., the two world wars), involuntary and disguised unemployment among the unskilled is eliminated, despite a substantial rise in the labor force participation rate. Once full employment is reached, further increases in labor demand can be satisfied only if the real wage of the unskilled is raised. Since the real wage of skilled workers will not rise, the rise in the real wage of the unskilled will reduce relative wage skill differentials.[10] This narrowing of wage differentials among skill groups

[7] Locke Anderson first used this term to describe "the relationship between economic growth and the extent of poverty among . . . families" [1].

[8] Allowing job vacancies to go unfilled is an aspect of allowing orders to go unfilled. Zarnowitz has shown that choosing to backlog orders rather than raising product prices is consistant with pure competition [18].

[9] This floor may be viewed as a perfectly elastic segment of the supply function of unskilled labor due to either unions or minimum wage legislation. Alternatively the effective demand curve for labor may be viewed as terminating at the social minimum wage, so that the marginal worker receives a wage above his supply price, and yet, involuntary unemployment persists.

[10] The model has been stated quite rigidly for heuristic reasons, but some cyclical and random fluctuations in the wage structure can and have been admitted into the argument by Reder without damaging the thrust of his argument, which is oriented toward explaining the trend, or more accurately, the once-over structural changes in skill differentials. A drift toward the old skill differentials over the long haul is likely, however, as gains in marginal physical product due to technological advance lead to a sharing between increases in wages and increases in employment. The greater unemployment among the unskilled suggests that they will take a greater proportion of the productivity gains in increased employment than will the skilled. On the other hand, the skilled face

is not reversed when unemployment increases (as will be explained below). With the narrowing of wage differentials, income inequality will also decline, *ceteris paribus.*

Some discussion of the role of inflation in the model must be added to this brief summary of Reder's arguments.[11] An analysis of the incidence of training costs must also be undertaken. First, note that raising the real wage rate of the unskilled above the initial social minimum requires moving from one full employment level to a higher full employment level. That is, the demand curve for unskilled labor must slide up the supply curve of unskilled labor, and the latter schedule is an increasing function of the real wage. The rise in the real wage requires income redistribution via inflation, with the rising consumer price level acting to transfer real income from rentiers and skilled workers to unskilled labor [5]. As the incomes of the unskilled rise, the money equivalent of the higher real wage rate becomes the new social minimum, and since it is rigid downward, the old skill differential cannot be reestablished even in periods of less than full employment. The level of employment must be the variable which brings the real wage and marginal value products into equality.[12]

Since the wage differential begins to narrow after full employment, the incentive to undertake training declines.[13] If the number of trainees declined with the fall in skill differentials, the real wage of the skilled would have to be raised to maintain the increased flow of skilled workers which would, in turn, tend to reestablish the original wage differentials. I do not believe that this, in fact, occurs.

The wage differential between skilled and unskilled workers reflects the return to investment in training [2]. In a purely competitive market, workers would shift from unskilled to trainee occupations, lowering the marginal value product and hence the wage rate of trainees. Eventually, equilibrium would be reached. The wage differential between skilled and trainees would yield a return on the investment in training (foregone income in the form of the differential between the

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greater pressure from the adoption of laborsaving innovations. Finally, it may be noted, that convergence in the face of inflation involves rising money wages of both skill groups, but rising real wages only among the unskilled; technology held constant.

[11] Reder dismissed the role of inflation, believing its effects were confined to money illusion [12]. There is no money illusion in what follows.

[12] If with the newer higher wage floors, there are higher levels of unemployment than before, then there may be more poverty than before. Hopefully, this result is ruled out by the political pressures to sustain aggregate demand. Also, the inflation requires raising money transfers to forestall a rise in poverty among those outside the labor force as the proportions in poverty within the labor force fall.

[13] A general price increase widens absolute skill differentials (the marginal physical products serve as weights), but the relative skill differential is not changed. Since the price level change raises the differential and the foregone income proportionately, the return to training is virtually unaffected.

wage of the unskilled and trainee wages plus any direct training costs) just equal to the interest rate. The wage of trainees and apprentices would be below the wage rate of the unskilled, and the employer would train anyone who chose to be trained since the employee would pay the full cost of his training.

Apprentice and other trainee wages are not lower than the real wage rate of the unskilled, however. They are set, to a very large extent, by unions as part of the effort to restrict entry into the skilled occupations.[14] The wage rates of trainees, like the wage rates of the skilled, are fixed and inflexible downward, so that the adjusting mechanism, here, as in the rest of the system, is from wages to employment and marginal products. With the trainee wage rate fixed at a level higher than the wage of the unskilled and inflexible downward, employers must now restrict the number of trainees (the return to training is virtually infinite) to equilibrate the marginal products of trainees and the trainee wage rate. With a sustained outward shift in aggregate demand, employers can now increase the number of trainees, just as they increase the number of skilled (with unions tolerating the increase) and they choose to do so as a part of the process of lifting the unskilled up the occupational ladder to maintain the skilled wage rate.[15] There is, therefore, considerable scope for the real wage of the unskilled to rise before the flow into training is curtailed.

Embracing trickling-down from growth to poverty reduction as an avowed public policy implies a most unappetizing program politically. Either it must be accompanied by a vigorous "incomes policy" or there will be substantial inflation. Since there is reason to believe that the rate of reduction in poverty for a 1 percent increase in prices will be smaller now than in the past [1], either the inflation will have to be severe or the incomes policy will have to be much more than a melt-the-metals policy for it to have an effect other than moving the unemployed and domestics to the unskilled category. With a consensus definition of poverty, there is likely to be no decline in poverty up to the point where the unskilled wage rises relative to the skilled wage.

Since a vigorous full employment policy has been ruled out because of its inflationary side effects, emphasis has been put on less aggregative policies. The hope is to make use of a detailed description of those socioeconomic characteristics which distinguish the poor from the rest of the population. Programs to encourage students to complete high

[14] Trainees have more formal education than unskilled workers, and, therefore, part of their higher wages is attributable to returns to education. This is not necessarily in conflict with the hypothesis that unions are restricting entry into training, thereby creating a wage differential, since some rationing device into training is required.

[15] This is something of an overstatement. However, only 41 percent of 1964 high school drop-outs were employed in October, 1964. Of those employed in nonagricultural activities, 25 percent were employed only part time [3].

school are a prominent feature of this "detailing" approach. What are the prospects for success?

IV. *Increased Schooling as an Antipoverty Policy*

In the past, the proportion of families in poverty has declined when the incentive to invest in education has been the weakest; i.e., when the opportunity cost of education was high and rising while skill differentials and therefore returns to education were declining. In general, if the prior section is correct, full employment encourages the substitution of on-the-job training and work for formal education. Can a federal program of financial incentives invert the relationship between poverty reduction and years of schooling that is generated by individual profit maximizing behavior? The possibility certainly exists, for the probability of being impoverished is above the national rate at each education level below high school graduation. As years of schooling rise, the proportion of urban male white workers in the prime working years earning less than $3,000 falls toward 20 percent, finally reaching it at high school graduation [17]. While the possibility exists, it has a low probability.

Since the Korean war, unemployment levels have been high, especially for young people, so that the opportunity cost of education for a very large proportion of high school drop-outs has been close to zero.[16] Yet the unemployment rate has not been so high that large numbers of skilled workers are being forced into disguised unemployment. The economic incentive to stay in school is very great, therefore; yet the drop-out rate is high. And if anything, the proportion of impoverished young people has risen and the penalty for failing to complete high school has been increasing even for those who find employment [4].

The two most basic reasons for a federal policy to add financial incentives to education are made largely irrelevant by these considerations. One justification for an education policy could be that the marginal social benefit of schooling exceeds the marginal private benefit. This argument would be pertinent, however, only if the marginal private benefit equaled marginal private cost at present. Yet the large number of drop-outs at this time suggests that the drop-outs are not now staying in school long enough for marginal private benefits to

[16] Several additional points could be made here. First, in trying to maximize a joint product (skilled and apprenticed), the employer could, as part of profit maximizing, choose to run a loss on the training function. Second, it is clear that once full employment is reached, the government must tolerate some further inflation (it already must have faced inflation from diminishing returns) if it is to get increased output, but a severe wage price spiral must be prevented. That is, the unions must be banned, as they were with wage controls, from attempting to raise the real wage of the skilled and the trainees, but the government cannot prevent a rise in the money wage of the skilled and maintain the participation rate. The fall in the real rate of interest (money rates pegged) may also play a small part in contributing to the demand for training.

drop to marginal private costs. Staying in school even longer, so that the marginal social benefit falls to the marginal social cost, is not now an issue, therefore.

A second justification for a federal education policy could be that an imperfect capital market or a credit market lacking the necessary credit instruments exists which leads to underinvestment in education. When alternative earnings are close to zero, however, students cannot be dropping out of school in large numbers because they cannot borrow against the future returns to their training to forestall foregoing current consumption unnecessarily. Nor can it be important that the drop-outs may have a higher propensity to consume present as opposed to future commodities than the marginal borrower.

Assuming the government will use financial incentives (and not coercion) to gain its objectives, what scope is there for actions that have not already been undertaken? Where is the operative market failure in the present situation? Having undertaken to insure that poor children cannot get work and income, what more can be done to keep them in school? What can be achieved through economic incentives, since to be effective they require rational economic responses: but all we think we know suggests that drop-outs are behaving irrationally, at least as economists use that silly word. Under the circumstances, pessimism is the most optimistic position on an education policy for an economist with any compassion for the poor.

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MEASUREMENT OF THE QUALITY OF SCHOOLING*

By Finis Welch
University of Chicago

This paper reports the results of a cross-sectional analysis of the determinants of the return to schooling. In the analysis, quality of schooling is explicitly considered as a principal contributor to productivity. The population considered consists of rural farm males, twenty-five years of age or older, that earned income in 1959. Persons who had attended college are excluded from consideration in this study so that an estimate of the quality of elementary and secondary schools can be obtained. The original observations are for fifty-seven "states"; ten southern states[1] being designated as two "states," one consisting of white persons and the other of nonwhites. Alaska, Hawaii, and Rhode Island are omitted.

The preliminary discussion focuses upon a procedure for the prediction of individual income by years of school completed for each state, holding age, race, and the ownership of land and buildings (of farm operators) constant. The adjusted income-schooling profiles are then compared to profiles "predicted" by a market in which all labor is freely mobile. The conclusion of this comparison is that the returns to education are likely to be affected by differences both in the quality of schooling and in the return per unit of education. Finally, the analysis proceeds to a production function framework in which a search for the factors affecting quality of schooling is made, holding other productive inputs constant. In the production function analysis, twelve additional states are excluded from consideration because of insufficient school systems data.[2] Thus, the data reported in this paper are for the remaining forty-five "states."

The empirical analysis requires several transformations of the data. These transformations result in a set of parametric estimates whose probability distribution is unknown. In addition, the analytical procedure is one of searching rather than testing. For these reasons, it is

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[1] These states are: Alabama, Arkansas, Georgia, Louisiana, Mississippi, North Carolina, South Carolina, Tennessee, Texas, and Virginia.

[2] These states are: Arizona, Connecticut, Delaware, Maine, Maryland, Massachusetts, Nevada, New Hampshire, New Jersey, New York, Vermont, and Wyoming. Pennsylvania, the only remaining northeastern state, is assumed to be a part of the East North Central division.

not possible to make probability statements about the empirical results. However, there is fairly strong evidence that the most important determinants of differences in the quality of schooling are: (1) teacher quality as reflected in salaries and (2) the size of secondary school. Apparently, real economies can be obtained from the consolidation of school districts so that schools can be enlarged and teachers can become more specialized.

The General Framework

In this paper, education is assumed to be a standardized productive input that is comparable between areas. In a competitive market, the return to education is the value of the marginal product of education times the amount of education employed. Accordingly, there are two ways in which an increase in education affects the return to education. First, it increases the number of units of education that are income recipients. Second, other things equal, it changes factor proportions through substitution and, assuming diminishing marginal productivity, reduces the return per unit of education. Both these effects can be important in the measurement of the quality of schooling.

Quality shall be defined in terms of a simple adjustment relating the heterogeneous input, schooling, to the homogeneous factor, education. Thus, the quantity times the quality of schooling is defined as education. This definition mitigates the problem posed by regional differences in the quality of schooling that make the consideration of schooling as a standardized input impossible. According to this definition of education, if the quality of schooling in one area were twice that of another, *ceteris paribus,*[3] the amount of education would be twice as great, and hence its marginal product lower. Conceptually, then, quality differentials are reflected in differential returns, but the differences in returns must be adjusted for differences in marginal products.

The Empirical Framework

In order to derive an estimate of the return to schooling from income data, it was necessary to adjust incomes for differences in variables which may otherwise introduce bias. The adjustment procedure adopted was, first, to estimate an income generating function, and, second, to use that function for predicting income when the age distribution, the average value of land and buildings owned, and the proportion of nonwhites in each state-schooling class are held constant. This estimating procedure is a variant of the multiple regression technique. Estimates of

[3] "Other things" include quantities of factors other than education as well as the distribution of "schooling" in the population.

the product moments are obtained from four separately published samples drawn from the 1960 U. S. Census of Population.[4]

The predicted values of income are the values given by the estimated parameters of the income generating equation and the national averages of the factors in the *ceteris paribus* bundle; i.e., age, race, and capital I was unable to adjust for ability differences by years of school completed. Predicted income is supposedly for a "representative" individual for which the large, apparently random, interindividual differences in earning capacity have been averaged. The incomes predicted by states and by years of school completed are summarized by divisional averages in Table 1. The average increase in income per year of school completed is also reported.

TABLE 1

ADJUSTED AVERAGE INCOME BY GEOGRAPHIC DIVISION AND YEARS OF SCHOOL COMPLETED (Dollars)

Division	Years of School Completed						Average Increase in Income per Year of Schooling
	0	1–4	5–7	8	9–11	12	
North Central							
East North Central.....	$2,150	$2,470	$2,990	$3,530	$4,310	$5,170	$250
West North Central....	1,820	2,350	2,590	3,056	3,530	4,110	180
South (white)							
South Atlantic.........	1,410	1,730	2,240	2,630	3,290	3,890	210
East South Central.....	1,280	1,550	1,960	2,380	3,200	4,360	240
West South Central.....	1,320	1,620	2,260	2,280	3,210	3,350	170
South (nonwhite)							
South Atlantic.........	1,120	1,200	1,380	1,610	1,700	2,073	80
East South Central.....	1,010	1,050	1,214	1,360	1,460	1,647	60
West South Central.....	1,090	1,160	1,300	1,410	1,520	1,737	50
West							
Mountain.............	1,960	2,600	2,920	3,330	3,840	4,354	190
Pacific................	2,160	2,760	3,530	4,260	5,140	6,189	330

[4] These samples are: (1) the state reports of the Census of Population, Series D, (2) the 1 in 1000 sample of the Census of Population, (3) the matched sample for farm operator families of the 1960 Sample Survey of Agriculture and the Census of Population, and (4) a subject report, "Whites with Spanish Surnames," of the Census of Population. For a detailed explanation of the estimation technique, see my "The Determinants of the Return to Schooling in Rural Farm Areas, 1959" (a forthcoming Ph.D. dissertation, Univ. of Chicago, winter 1966). Briefly, the dependent variable was the logarithm of income and the independent variables were: (1) a set of 342 dummy variables consisting of six schooling class variables (0, 1–4, 5–7, 8, 9–11, and 12 years of school completed respectively) in each of the fifty-seven states; (2) a dummy variable for nonwhites and whites with Spanish surnames; (3) a set of seven dummy variables for age groups (25–29, 30–34, 35–44, 45–54, 55–64, 65–74, 75 and over); and (4) a capital variable for the return to farm land and buildings to farm operators (the return to owned capital is included in income). Persons other than farm operators were assumed to own no capital. The capital coefficient was allowed (by covariance technique) to vary between the major agricultural producing regions to adjust for transitory effects upon the return to capital.

If labor were freely mobile, one would expect a tendency toward equality in the wages of similar types of laborers. But, is the variation in average income observed in the estimates of Table 1 consistent with a highly mobile population? Suppose that income is the wage of the purely physical attributes of a person plus the return to his education. Suppose further that the physical attributes of the individual are those of a person who has never attended school. Then average productive properties, adjusted for differences in age, etc., of persons without schooling would most likely be similar in all states. Thus we would expect to observe a tendency toward income equality for persons without schooling. Furthermore, this tendency toward equality would be observed in the return per unit of education. By definition, the return to a unit of schooling is the unit return to education multiplied by the quality of schooling. And, in this world of labor mobility, the return to education would be approximately equal so that the return per unit of schooling would be an hedonic index of quality. On the other hand, if there are interregional differences in the return to education, the return to schooling cannot be so easily interpreted.

For this reason, to examine whether or not mobility has resulted in approximate equality of marginal products, we can rely only upon the estimated incomes of the unschooled since comparisons of returns to schooling are obscured by potential differences in quality of schooling and in the price per unit of education. Inspection of the adjusted incomes of those without schooling in Table 1 indicates a degree of variation which should at least raise doubt about the validity of the mobility hypothesis. Furthermore, it is interesting to note that the income differences indicated in the adjusted incomes of the unschooled have persisted for several decades. For example, if the divisional income rankings in 1959 are compared to a rank index for the average monthly farm wage which existed from 1935 to 1942, the rank correlation is 0.93 and the correlation with the rank index for 1910 is 0.78.[5] Given (1) that incomes of those without schooling differ by region and (2) that these differences have persisted for some time, we must conclude that these income observations were generated in a market in which labor was not freely mobile.[6]

Without mobility, equality of marginal products is not a necessary result, nor can an index of the return to schooling be justified as an

[5] The nonwhite regions are omitted from the comparison. The rank indices for farm wages appear in Robert J. Wolfson's "An Econometric Study of Production and Wage Determination in American Agriculture with Particular Reference to Three Production Regions" (an unpublished Ph.D. dissertation, Univ. of Chicago, Mar., 1956, Table 26, p. 83).

[6] Of course, the income differentials may represent transportation costs and are conceivably equilibrating differences. The origin of the differences is unimportant. It is sufficient that they exist and are not transitory (random).

index of quality. In the absence of freely mobile labor, an analysis of factors affecting quality must be accompanied by an analysis of factors affecting the demand for education. But before considering such a joint determination of the value of a unit of schooling, it is necessary to indicate a method for estimating the number of units of schooling a person possesses. The method employed is discussed in the following section.

Schooling is a productive factor and differences in the amount of schooling possessed by persons create differences in their productivity. An individual is, by assumption, a composite factor consisting of one bundle of physical labor (a warm body—the original but not indestructible properties) and a bundle of education. The properties of the physical bundle are those associated with a person who has not attended school. According to our limited definition of education, persons who have not attended school have no education and the income of such a person is the return to his physical properties. Since persons who have attended school also have a bundle of the same physical properties, their income includes the return to these physical properties. Hence by subtracting the expected income of a person who has not attended school from the expected income of someone who has, an estimate of the return to the second person's education is obtained.

Now, the return to education, as defined previously, is the number of units of schooling multiplied by the product of quality of schooling and the value of the marginal product of education. I assume further that the number of units of schooling a person possesses is determined by the amount of time he has spent in school, and that the transformation function of time in attendance into schooling is the same in all states. The rate of transformation of schooling into education—a rate which is assumed to be constant within each state—is given by the quality of schooling. Thus persons throughout the country who have attended school for the same number of years, say eight, receive the same amount of the productive factor, schooling, but not the same amount of education. The amount of education obtained by a given individual is the amount of his schooling times its quality. Since the scale of measurement is arbitrary, I assume that a person who has attended school for eight years has one unit of schooling. If a person who has not attended school has no schooling and if a person who has attended for eight years has one unit of schooling, then we might ask how much schooling has a person who has completed, say, twelve years of school? This question can be answered only if we assume that the price per unit of education is constant within (but not necessarily between) states. Assuming quality of schooling to be constant within states, the number of units of schooling possessed by a person with

twelve years attendance is given by dividing the return to his schooling by the return to schooling of a person in the same state who has eight years attendance.

In general, the return to an individual's schooling is

$$W_{ij} - W_{oj} = \beta_i C_j, \quad (1)$$

where

$$C_j = Q_j MP(E)_j.$$

W_{ij} is the predicted income of a representative individual in state j who has attended school for i years. W_{oj} is the predicted income of a person in the same state who has no schooling. β_i is the number of units of schooling possessed by a person who has i years in attendance ($\beta_8 = 1$) and C_j is the return to one unit of schooling in state j or the quality of schooling, Q_j, multiplied by the value of the marginal product of education, $MP(E)_j$.

The β's and the C's were jointly estimated by least squares-covariance technique in which the logarithm of the estimated return to schooling within each state-schooling class was regressed upon two sets of dummy variables, one indicating years of school completed and the other, state of residence. The estimate values are provided in Table 2. As we may have anticipated, the time in attendance into schooling relationship is nonlinear. For example, a person with twelve years in attendance has an estimated 2.26 times as much schooling as a person with eight years attendance.

TABLE 2

ESTIMATED VALUE OF THE SCHOOL ATTENDANCE—PRODUCTIVE CAPACITY PROFILE, THE β'S, AND THE AVERAGE RETURN TO A UNIT OF SCHOOLING BY GEOGRAPHIC DIVISION, THE C'S

I. UNITS OF SCHOOLING REPRESENTED BY YEARS OF SCHOOL COMPLETED

Years of School Completed	0	1–4	5–7	8	9–11	12
Units of Schooling $\beta_8=1, \beta_0=0$	0	.25	.65	1.00	1.63	2.26

II. AVERAGE RETURN TO A UNIT OF SCHOOLING BY REGIONS, 1959

Pacific	$1,980	
Mountain	1,430	
East North Central	1,330	
West North Central	1,260	
	White	Nonwhite
South Atlantic	$1,200	$390
East South Central	1,140	260
West South Central	1,120	290

Holding years of attendance constant, the systematic differences in returns to schooling are determined by the multiple of quality of schooling and the value of the marginal product of education. Since the marginal product of education is determined by a production function, it was necessary to make specific assumptions about the determinants of total product. I assume here that production is a function of three variables: the number of persons productively employed, the quantity of education used in production, and an aggregation of nonlabor inputs. Specifically, I assumed Cobb-Douglas functions linear in the logarithms, so that

$$Y = AN^{\alpha_1} (Q \Sigma \beta_i N_i)^{\alpha_2} K^{\alpha_3} \quad (2)$$

and

$$Q = BZ_1^{\gamma_1} Z_2^{\gamma_2} \cdots Z_n^{\gamma_n}$$

where total production is Y. The number of persons is N. Q is quality of schooling and β_i the amount of schooling possessed by a representative individual with i years in attendance and N_i is the number of persons who have attended school for i years. $Q\Sigma\beta_i N_i$ is, therefore, the quantity of education. The Z's refer to specific school system inputs. The return to one unit of schooling is

$$C = Q\alpha_2 AN^{\alpha_1} (Q \Sigma \beta_i N_i)^{\alpha_2 - 1} K^{\alpha_3}. \quad (3)$$

Equation (3) is the basic estimating equation. The predicted values of the C's, estimated from equation (1), are used as dependent variables. The independent variables are: (1) quality of schooling, (2) quantity of schooling, (3) the number of persons employed, and (4) the quantity of nonlabor inputs. Since quality is not directly observable, various combinations of school system inputs were substituted for quality, Q. The observations are for states. Before examining the empirical results, the operational definitions of the inputs should be considered.

The number of persons productively employed is simply the number of persons in the population described earlier with two adjustments: the first for persons between the ages of 14 and 24 and the second for persons who had attended college.

The stock of schooling, $\Sigma\beta_i N_i$, is a weighted summation of persons by years of school completed. Estimated β weights appear in Table 2 for those who had not attended college. The β's are estimated for two groups of college "attenders": those with 1–3 years and those with 4 or more years of college. For these persons, β's were estimated sepa-

rately for each state by dividing an estimate of the return to schooling by the return to one unit of schooling, *C*.

The quantity of nonlabor inputs is computed first for those persons working on farms and then expanded by dividing by the proportion of persons in the rural farm population of each state that work on farms.[7] This expansion is appropriate only under two conditions: (1) if production is subject to constant returns to scale and (2) if the marginal product of education in each state for those who work on farms is equal to the marginal production of education for those members of the rural farm population who do not work on farms. For those who work on farms, the quantity of nonlabor inputs is, in accordance with the assumption of a Cobb-Douglas production function, a weighted geometric mean of nonlabor inputs for an agricultural production function. The weights are proportional to estimated factor shares.[8]

For the ten southern states which are separated into white and nonwhite states, the number of persons and the quantity of schooling in each state is computed by adding to the calculated "white" quantity the calculated "nonwhite" quantity weighted by its relative marginal product. For example, the wage of a person with no schooling is the marginal product of the factor, *N*. If in a given state, the average income of a nonwhite with no schooling is 80 percent of the average income of a white with no schooling, it is assumed that the market functions as though whites and nonwhites were perfect substitutes at a rate of 1.0 to 0.8.[9] In this case, the number of persons would be given by the number of whites plus 80 percent of the nonwhites.

Before explicitly considering the school system inputs, it should be noted that quality of schooling is a measure of schooling's intensity and can be interpreted as the partial product of a production process in which inputs by school systems and time in attendance are transformed into education. In this process, school system inputs determine quality and attendance time determines quantity of schooling.

Since schooling is only a function of time in attendance, the specification of school system inputs should be sufficiently broad to allow for externalities such as the motivation and the ability of the population. Although an attempt was made to isolate some of these external effects, it was not successful.[10] School system inputs, therefore, refer only to ob-

[7] About 70 percent of the rural farm population worked on farms.

[8] The factor share estimate as well as the list of nonlabor inputs is given by Yoav Kislev, "Estimating a Production Function from 1959 U.S. Census of Agriculture Data" (an unpublished Ph.D. dissertation, Univ. of Chicago, June, 1965).

[9] We actually know very little about the manner in which a competitive market allows discrimination to exist. The procedure I have used can be interpreted as underutilization either directly or indirectly by improper factor combinations.

[10] For example, there is typically a relatively large emigration from rural farm areas. If there is nonrandom selection of the migrants, the remaining population's schooling may be in-

servable attributes of the rural school systems. Although the estimates of returns to schooling are derived from incomes of persons who did not attend school at the same time, the school systems data refer only to one academic year, 1955–56. In view of the fact that the distribution of expenditures per pupil between states has been approximately constant through time, the error introduced by using a point estimate of the distribution is not thought to be large.[11] Data for school systems are primarily from the 1954–56 Biennial Survey of Education. Data in the chapter referring to Rural Counties are combined with those rural counties enumerated in the County Unit chapter. To prorate the inputs within southern states between white and nonwhite school systems, it was necessary to compare Negro school systems to other systems in 1945 and then to extrapolate relative differentials to 1955–56. For example, if Negro teachers received one-half as much income as white teachers in 1945, I assumed that the same salary ratio existed in 1955–56.[12] Although several variables were initially used as school system inputs, only a few exhibited any significant independent explanatory power. Most of the expenditure variables were highly collinear with total expenditures. The variables considered in this paper are summarized in Table 3.

The Empirical Results

The objective of this section of the paper is to isolate the more important factors which contribute to quality of schooling. To set the stage for these results, some perceptive admonitions concerning the interpretation of rural school systems data used in this analysis seem relevant. The authors of the Rural School Survey make the following comment:

ferior (or superior) to the original population. Since my analysis is based upon the residual population, the resultant measure of quality of schooling would be possibly biased. In an attempt to control for "quality drain," I used three variables which would affect the propensity of a population to migrate. They were: (1) the ratio of the median income of rural farm people to urban people, (2) the percent of a state's population living in urban areas, and (3) the percent of a state's income derived from value added by manufacturing. No combination of these variables indicated any significant explanation of the returns to schooling. As a further example, I "controlled" for the schooling of the parental population as a proxy for taste for schooling. This variable was highly correlated with a dummy specification of race and could not be analyzed separately. Within each racial group, there was no relationship between the return to schooling and the schooling of the parental population.

[11] The proper school system inputs would be an average, weighted according to the time in which members of the population were in school. The correlation of the state distribution of per pupil expenditures in 1955–56 with the distribution in the years 1953–54, 1951–52, 1949–50, 1945–46, 1939–40, and 1929–30, is .98, .97, .96, .93, .92, and .89, respectively. Office of Education, U.S. Department of Health, Education and Welfare, *The Biennial Survey of Education in the United States—1954–56*, Statistics of State School Systems: 1955–56, Chap. 2 "Organization, Staff, Pupils and Finances," 1959.

[12] This is not quite accurate. The 1945 data refer to state aggregates and the 1955 data refer only to rural areas. I also included an adjustment for differentials between urban and rural areas.

Before the total expenditures per pupil can be understood, it must be borne in mind that the major part of this index is the product of two variables, viz., the salaries paid and the number of pupils per staff member [p. 56]. . . . Generally speaking, rural schools are small schools. They are the natural product of scattered homes, local autonomy, and expediency in educational planning. Large rural schools reflect successful efforts to develop state and local plans of school consolidation, to provide free transportation, and to organize more comprehensive programs of education [p. 46]. . . . The very low pupil-teacher ratios shown for the rural counties, especially in the secondary schools, reflects the basic causes of some of the most persistent and difficult problems the rural schools have to meet. Since the salary paid to the teachers is the chief component of the per pupil cost, few pupils per teacher tend to keep the salaries low, to load more grade and subject assignments upon each teacher, and to inhibit the number and quality of the instructional services offered [p. 31].[13]

TABLE 3

AVERAGE RURAL SCHOOL SYSTEM INPUTS BY GEOGRAPHIC DIVISION, 1955–56

Division	Total Current Expenditure per Pupil*	Members of Instructional Staff per 100 Pupils*	Average Salary per Member of Instructional Staff	Average Number of Pupils Enrolled per Secondary School
North Central				
East North Central	$275	4.6	$3,600	234
West North Central	296	6.2	2,960	121
South (white)				
South Atlantic	225	4.4	3,440	281
East South Central	212	4.7	3,000	222
West South Central	260	4.8	3,500	170
South (nonwhite)				
South Atlantic	127	3.8	2,650	248
East South Central	102	4.2	1,880	159
West South Central	139	4.1	2,290	93
West				
Mountain	308	5.3	3,600	156
Pacific	376	5.1	4,360	194

* Per pupil in average daily attendance.

The estimating technique adopted was to "fit" equation (3) by least squares technique, all variables being expressed in their logarithms. Specific sets of school systems variables were substituted in this equation for the quality index. The equation was further modified by the introduction of a dummy variable for the nonwhite states and by the use of two regional dummy variables, one for the South and the other for the Pacific division. The interpretation of the regional dummy variables is that of a factor which deflates prices and/or shifts the level of the production function. The estimates were constrained so that the production function is homogeneous of degree one. Results of the various estimates appear in Table 4. The estimating equation is given by:

[13] Office of Education, *Biennial Survey of Education in the United States—1954–56*, Chap. 3, Statistics of Local School Systems: 1955–56, Section IV, Rural Counties, 1959.

$$C = pQ\alpha_2 A N^{\alpha_1} (Q \Sigma \beta_i N_i)^{\alpha_2 - 1} K^{\alpha_3} \tag{4}$$

$$Q = BZ_1^{\gamma_1} Z_2^{\gamma_2} \cdots Z_n^{\gamma_n}$$

where C is the return to one unit of schooling, and p is the proportion of the return to a white person that a nonwhite would expect to receive from one unit of schooling of comparable quality.

The empirical results, although somewhat surprising, are consistent with the observations made by the authors of the rural school survey. The effect of teacher salaries is always positive; i.e., an increase in salaries would be expected to improve the quality of schooling. Conversely, high teacher-pupil ratios are associated with lower quality. These contradictory forces are embodied in the total expenditure variable. The simple correlation between per pupil expenditures on teacher salaries and total pupil expenditures is high ($r = .98$), and, for this set of data, the two series for practical purposes are indistinguishable. Per pupil expenditures on teacher salaries is the product of average teacher-salary and the teacher-pupil ratio. Since these forces work in opposite directions, the net effect of expenditures per pupil is ambiguous. If the effects of salary dominate, the net effect will be positive. If the effect of high teacher-pupil ratios is dominant, the net effect will be negative. For this particular group of data, the effects are approximately offsetting and the net effect of expenditures per pupil is zero (regression equation (1)). The nature of the opposing forces in the salary and the teacher-pupil variables is indicated in regression equations (2), (3), (5) and (6).

The partial effect of total current expenditure, holding average teacher salaries constant, is negative (regression equation (2)). This indicates that increased expenditures for given salaries have been accompanied by increased teacher-pupil ratios. If increased teacher-pupil ratios are associated with smaller, less efficient schools, the detrimental effect is obvious. As one would expect, the partial effects of increased expenditures per pupil become positive when the teacher-pupil ratio is held constant (regression equation (3)). In this case increased expenditures are associated with increased salaries. Note the similarity between regression equations (2), (3), and (5). This similarity indicates the strong relationship between total expenditures and expenditures on teacher salaries. It does not seem to matter whether salaries, the teacher-pupil ratio, or total expenditures are omitted for these data; controlling for any two of these variables also specifies the third. This happens to be an anomaly of this set of data and would not be expected for other analyses.

The positive effects of teacher salaries can be interpreted as the

TABLE 4

REGRESSION ESTIMATES OF EQUATION (3). THE DEPENDENT VARIABLE IS THE ESTIMATED RETURN TO ONE UNIT OF SCHOOLING (OBSERVATIONS ARE FOR FORTY-FIVE STATES)—STANDARD ERRORS ARE IN PARENTHESES*

Variable	Coefficient Estimated	Regression Number						
		1	2	3	4	5	6	7
1. Nonwhite	log (p)	−.275 (.330)	−.551 (.331)	−.473 (.325)	−.361 (.334)	−.515 (.324)	−.524 (.329)	−.434 (.326)
2. Quantity of schooling, $(\Sigma\beta_i N_i)$	$\alpha_2 - 1$	−.840 (.277)	−.600 (.268)	−.626 (.266)	−.707 (.276)	−.596 (.262)	−.576 (.273)	−.615 (.276)
3. Number of persons employed, N	α_1	.490 (.224)	.342 (.270)	.355 (.221)	.401 (.233)	.342 (.219)	.327 (.227)	.374 (.228)
4. Nonlabor inputs, K	α_3	.350 (.122)	.258 (.122)	.271 (.121)	.306 (.127)	.254 (.121)	.249 (.124)	.241 (.126)
5. Total current expenditure per pupil in attendance, Z_1	$\alpha_2\gamma_1$	−.007 (.134)	−.396 (.206)	.216 (.162)	.057 (.144)			
6. Average salary per member of instructional staff, Z_2	$\alpha_2\gamma_2$		.710 (.295)			.287 (.183)	.286 (.185)	.268 (.188)
7. Members of staff per 100 pupils, Z_3	$\alpha_2\gamma_3$			−.598 (.260)		−.399 (.205)	−.360 (.243)	
8. Enrollment per secondary school, Z_4	$\alpha_2\gamma_4$				.096 (.077)		.025 (.081)	.087 (.069)
R^2		.906	.919	.918	.910	.919	.919	.914

* The standard t test should not be applied because errors are not normally distributed.

effects of teacher quality. However, the effects of the teacher-pupil ratio are likely to be more ambiguous. High teacher-pupil ratios are associated with small schools and a widely diversified teaching load. As schools become larger, the opportunities for teacher specialization increase. The empirical results indicate the possibility of quality enhancement through teacher specialization. However, we cannot be sure of all dimensions lying behind the phenomenon of school size or what appears to be the same thing, the teacher-pupil ratio. Comparison of regression equations (5) and (7) indicates that the effects of the teacher-pupil ratio are registered in the size of secondary school. It is possible that size of school is a proxy for teacher specialization; yet it is conceivable that there are other factors which also assume importance. Perhaps larger schools generate student competition. The opportunities for speculation on this point are enormous and provide insight about an extremely relevant issue. One implication of the effect of school size and the teacher to pupil ratio is obvious: there are gross economies of scale which originate with school centralization or consolidation. Of course this analysis only indicates positive gross returns and does not compare these returns to their costs. However, if these effects are as important as the data indicate, a serious evaluation of the real cost of the additional pupil transportation involved in school centralization is in order.

Summary

In the course of this paper, I have attempted to delineate a logically consistent method for the analysis of the quality and productivity of schooling. The empirical results lend support to the hypothesis that education can be considered as a factor of production and that differences in school systems can affect the productivity of schooling.

The parametric estimates are for a production function of value added by the male rural farm population. The estimates indicate that the factor shares of physical labor and education are approximately the same, 38 percent, and that the share of nonlabor inputs is about 24 percent. The estimates also imply that the return to one unit of schooling will be 35 percent less for nonwhites than for whites when school system inputs and the physical labor to education as well as the nonlabor inputs to education ratios are held constant.

It is gratifying that superior teacher quality does apparently enhance the productivity of schooling. However, the economies of scale phenomenon are somewhat surprising. We have devoted a considerable amount of effort to the investigation of problems arising from urban schools that may be too large, but we have failed to recognize the high costs implicit in small rural schools. The majority of the rural-trained youth will eventually seek urban employment in which the effects of

inferior schooling must be realized. Of course these empirical results are tentative. The aggregation at the state level and the lack of availability of school systems data for earlier periods are likely to have glossed over much of the substance of the factors affecting quality. Hopefully, less aggregative analyses can provide insight about the many issues left unanswered in this work. The economist's tools are adapted for analysis of costs and returns and there is no reason that these tools cannot be applied to the analysis of school systems. Indeed, the real value of economic evaluation lies precisely in the analysis of markets, such as the one that distributes education, in which price guidelines are so imperfect.

DISCUSSION

LEE R. MARTIN: The three papers are quite different and very interesting. Smolensky offers a classical example of theoretical reasoning, with a sprinkling of empirical content. Becker and Chiswick follow up Becker's recent published work by attempting a generalized conceptual framework for research in the economics of human capital formation, converting this into a statistical formulation, and testing it by regressing earnings in the South and non-South on years of schooling. Welch reports on the results of his cross-sectional analysis of the determinants of the returns to education, taken to be jointly determined by schooling and quality of education. Each deserves comment.

Smolensky proposes an interesting approach to the problem of a dynamic, consensus poverty line, basing it on external diseconomies of poverty to the nonpoor. His proposal is in line with growing sentiment for making the poverty line relative; that is, making it move up (and down?) with real income measures (mean, variance, and skewness, perhaps). He should be urged to carry his suggestions further, in order to give it a real empirical test.

Smolensky's most important conclusion—the one from which his pessimism derives—may be subjected to more critical questioning. Perhaps his conclusion follows from using concepts in which the full employment assumption may still be imbedded. While the marginal private cost of staying in school may even now be less than the marginal private benefits in some aggregate or average sense, there may be large numbers of drop-outs who regard their employment probability as being so low that schooling is judged not to be worth while as investment or consumption. Those in disadvantaged groups and those with underdeveloped learning capacity may be two large categories of drop-outs.

The more favorable employment opportunities of today do not relate to drop-out decisions of a few years ago, unless very foresighted expectations could be assumed to prevail among youth. As long as the proportion of non-drop-outs who actually find regular employment remains substantially less than unity, drop-out decisions may not be entirely irrational although not highly rational either. Somewhat more optimism might be justified in the use of financial incentives by the society to keep more youth formally engaged in education, if the monetary and fiscal authorities succeeded in making employment opportunities grow more rapidly than the labor force for several years, and if the additional employment opportunities could be made both relevant and visible to disadvantaged youth, and to below-average school performers.

Becker and Chiswick have regressed earnings on years of schooling, reporting only a small sample of their statistical results in this paper. If their techniques can be refined and extended, they seem to have opened up many possibilities for bringing individual and aggregate expenditures for education within the scope of optimizing technology. Nevertheless, in spite of the exciting

look I have had over a new horizon, I find several relevant questions I cannot answer. I will raise these questions, in the hope Becker will address himself to them briefly:

1. The coefficients of determination came out less than I would have anticipated; have you tried to explain their absolute magnitude?

2. In both regions of the U.S., rates of return rise with education; Schultz's early work seemed to indicate falling rates. Which seems more plausible?

3. The differences in educational quality between the South and the non-South may be larger than the quantitative differences. Would adjustments for quality be likely to make interregional differences in rates of return even larger?

4. Do you have any thoughts on how to conceptualize the quantities of other human capital?

Welch's path-breaking efforts also deserve praise although he raises more questions for me than he answers. By analyzing the income of rural farm adult males by themselves, he raises the issue of whether these incomes are influenced more by events in the nonfarm sector than by what happens in rural areas. On theoretical grounds, one might prefer to regard farm and nonfarm incomes as being jointly determined by similar if not identical sets of variables, although this might raise serious empirical difficulties.

Welch uses weights to transform years of schooling into units of schooling: 8 years completed = 1 unit of schooling; 12 years completed = 2.26; and so on. Interesting inferences might be drawn from using Welch's weights and Becker's rates of return, with a budget constraint on the educational budget, to estimate which levels of education to emphasize, solely on economic grounds.

The quality indexes are difficult to interpret without seeing the results; the indexes are not shown separately for each "state." The quality indexes were derived from a multiple regression on four terms which were combinations of four basic elements: enrollment, number of teachers, salary per teacher, and nonteaching expenditures. Salaries and enrollments were each used separately and directly as variables, but the other two terms include both teachers (varying directly) and enrollments (varying inversely). I am not able to visualize the multicollinearities that might arise in a regression of this sort.

In spite of these difficulties, the analyst gets results that appear to be consistent with other research and with expectations. However, the coefficients of the four quality variables do not appear easy to explain. As far as I could work it out, the sum of the quality exponents ($\Sigma\gamma_i$) was less than 1 for each regression, implying diminishing marginal productivity for educational quality improvements.

Welch apparently considered the results of his regression 7 more valid, although no reasons are given. It could be guessed that none of the quality variables made a great contribution to the high R^2's.

Let me close by complimenting Smolensky, Becker and Chiswick, and Welch for their pioneering efforts and by hoping to have a later look at the complete results of the two empirical papers.

ALICE M. RIVLIN: Exactly five years ago our distinguished chairman gave a presidential address entitled, "Investment in Human Capital." If you attended the 1960 meetings you will recall that most members of the Association looked blank when they heard this title. Investment in what? Professor Schultz spent a large part of his address establishing his right to talk about the subject at all.

This year we have two whole sessions and parts of others devoted to one segment of the human resource field: the economics of education. We have just heard three interesting and provocative papers, no one of which was thought to need a lengthy introduction explaining what is meant by investment in human capital and why it is important. We have moved beyond that stage to the harder business of building a body of theory and knowledge about investment in people. These developments are in no small measure attributable to the creativity and leadership of Theodore W. Schultz.

Of the three papers I find myself with the least to say about the Becker-Chiswick effort—simply because it is difficult to evaluate a partially constructed theory. Their objectives are admirable. It would be useful to have a well-worked-out theory of income distribution which took into account human investment decisions. I am glad Becker and Chiswick have undertaken to provide such a theory and I am impressed by the elegance of their beginnings.

Becker and Chiswick explicitly recognize that educational investors have unequal access to funds and build this capital rationing into their theory. They also need to take account of three other important peculiarities of the context in which educational investment takes place: (1) big differences in the quality of a year of schooling, (2) the limited information many students have about opportunities to invest in themselves and the quality of the investment, and (3) the positive and negative consumption elements in education. The negative consumption elements are important and tend to be forgotten. I suspect the primary reason why people stop school before economists think they should is not a shortage of funds, but a distaste for the whole educational process.

Let me move on to Welch's interesting and ingenious attempt to measure the quality of schooling. The first part of his paper is devoted to demonstrating the proposition that labor is imperfectly mobile in the United States. Using some variant of multiple regression with dummy variables for states, Welch fits an equation to 1960 census data. He finds that the incomes predicted by this equation for persons with no schooling differ from state to state when other variables (age, color, farm capital, and Spanish surname) are held constant. He points out that differences in returns at non-zero levels of schooling might be due either to immobility or to school quality differences, but maintains that differing returns to men without schooling (unimproved men, so to speak) indicate immobility of labor.

This demonstration seems a bit tenuous. In a heterogeneous and subsidized industry like farming it is difficult to isolate the returns to labor. I am not convinced that Welch has done so simply by holding "farm capital" constant. Moreover, I am dubious about the statistical validity of extrapolating fitted

functions back to zero in a country where very few observations fall in the neighborhood of zero years of schooling, even in farm areas. Nevertheless, although I find the demonstration weak, I would not quarrel with the proposition being demonstrated—that labor is imperfectly mobile in the United States. This non-Chicagoan would be willing to accept the proposition on faith.

Finding no easy route to the isolation of returns to quality, Welch devotes the rest of his paper to statistical estimation of the relationship between returns to schooling by state and certain characteristics of state school systems which seem likely to affect quality. Much of his effort is devoted to finding an appropriate dependent variable—a return to one unit of schooling in the state which excludes the return to the personal attributes of the individual and to other factors of production. He does this by subtracting from incomes at a given schooling level the predicted incomes of persons with no schooling in that state. As I mentioned, I am uneasy about these predicted zero points. But I am much more uneasy about another feature of the estimation; namely, that the dependent variable is a return on education possessed by all rural males over twenty-five. Almost none of this stock of education was acquired in the last decade and much of it was acquired three, four, or even five decades ago. Rural education has changed rapidly in the last few decades, and the changes have been greater in some areas than in others. Furthermore, although labor mobility may not be perfect, it is certainly not zero, especially among farm laborers. Undoubtedly, the chances that a man was educated in the area in which he now lives are greater for farmers than for nonfarmers. Nevertheless, while migration into the farm population may be small, out-migration is large and educationally selective. Those with the most years of schooling tend to leave. Maybe those with the best quality schooling also leave, a factor which could obscure the relationship between returns and quality even without the time-period problem.

Perhaps we should ask ourselves why we want to look at the quality variables. The Welch data are suggestive of policy conclusions such as: to improve quality, raise teachers' salaries, and consolidate rural schools. But if we are going to examine this type of question should we not choose a more current measure of the output of the school system (achievement test scores, for example) to go with our measures of current input? Herbert Keisling at Indiana University and Thomas Ribich at the Brookings Institution have worked along these lines.

Frankly, I found the Smolensky paper disturbing. Judging from the title, I expected the paper to contain an evaluation of educational investment as a means of alleviating poverty. I expected the author's pessimism to arise out of evidence that the rate of return on investment in education of the poor was low.

My expectations were erroneous. Smolensky does not even discuss investment in education until the very end of the paper, and then he is pessimistic for a mystifying reason—not because the rate of return on education is low, but rather because he believes it to be high.

The paper starts with a discussion of the poverty line. Smolensky does not

say what he means by a poverty line or how to find one, but he does give a rule for changing the poverty line over time: "The rise in the poverty line over time should be such that were the incomes of the poor to rise at the same rate, the poor would, by voluntary market actions, satisfy the evolving desires of the community due to changes in its income level for those things in which there are externalities in consumption; e.g., housing."

One implication of this quotation is that the more invisible the poor are the less we should do for them. If the rest of us can only keep the poor segregated in slum-ghettos where we do not have to look at their housing or go to school with their children, we can forget about them, because their misery will not affect our property values.

If we include commodities other than housing, I see no reason why the externalities in consumption of these different commodities should rise at the same rate with respect to the income increases of the poor. The poor cannot be counted on to spend increases in their income in such a way as to maximize the external benefits to the rich. Left to their own devices they may buy cars instead of housing, or buy whiskey instead of inoculations. If the affluent do not care about the needs of the poor but only about the externalities in their consumption, it will be efficient to give the poor goods rather than money. In other words, Smolensky's formulation does not yield a poverty line stated in terms of the income of the poor, only a market basket of goods which the rich have an incentive to give the poor. Smolensky seems aware of this at the beginning of the paper, but not at the end where raising the incomes of the poor becomes a goal in itself.

Smolensky has faith that the externalities in consumption are such that the rich will actually have an incentive to improve the lot of the poor, but this incentive is not self-evident. Take a sprawling metropolitan area like Washington where most of the poor live in the central city and the affluent live in the suburbs. What would happen to property values in the suburbs if the housing of the poor were substantially improved? It is at least possible that suburban values would fall, since one of the major reasons for living in the suburbs—getting away from the slums—would have been removed. It is not obvious that externalities in consumption can be counted upon to induce the rich to help the poor.

If we skip over the murky discussion of why a full employment policy fails to help the poor, we come to Smolensky's reasons for pessimism about investment in the education of the poor. His main reason for pessimism is apparently that he finds the poor irrational. They are not now taking advantage of educational opportunities with high private rates of return. Smolensky apparently feels this irrationality on the part of the poor bars society from any efforts to increase the education of the poor in order to reap social benefits.

This is peculiar reasoning. The poor have little knowledge of educational opportunities and many of them hate school. If the social returns to their staying in school are thought to be high, I see no reasoning for not bribing them to do so. If the private returns are also high, as Smolensky alleges, so much the better.

There may be valid reasons for pessimism about investment in the educa-

tion of the poor. Private returns may not actually be high for low-income children, especially Negroes. Early deprivation may have sapped their ability and motivation to learn and racial discrimination may limit their future earnings. Moreover, the costs of keeping drop-outs in school may be so high as to make it improbable that the social returns would justify such costs. But these reasons are not given by Smolensky. His reasons for pessimism elude me.

ANDRE DANIERE: All of us are now convinced that a satisfactory model of income distribution will require reference to investment in human capital—more precisely, a theory of investment decision that brings into play an individual "demand" curve for investment (showing expected marginal rate of return at each investment level) and an individual "supply" curve (showing marginal "interest" cost). This, at least, can serve as a gross simplification for purposes of rough analysis, as long as we take for granted the relative rigidity of our time structures of investment in education.

At the same time, the authors fail to recognize—or, at least, to make explicit—the limited operationality of their model. In moving from the analysis of individual decisions to a determination of income distribution over a population, an all-important link is the relationship between supply of persons with different amounts of human capital and equilibrium earnings in each category; i.e., the constraints imposed by the labor market. When the model is expanded accordingly, the D curve, which is defined for a given schedule of earnings, ceases to exist as an initial condition. In a dynamic analysis of the process leading to equilibrium, it can show the temporary conditions faced by an individual investor, given the temporary schedule of earnings, and thus help determine the next adjustment in investments, which in turn leads to earnings readjustments, etc. Upon completion of the process, or in a direct static determination of equilibrium, the D schedule comes out as an implication of equilibrium: it is such that, if all individuals adjust their investment to it (given their supply curves), the resulting set of individual investments brings about a schedule of earnings (in the labor market) that is consistent with it.

Now there may be numerous situations in which the schedule of earnings can be taken as independent of the distribution of individual investments, so that (1) an independent D curve can be specified which, together with individual supply curves, determines the distribution of investments, and (2) this distribution, together with the earning schedule, determines income distribution. Marginal or short-run effects of shifts in supply curves may qualify, as may comparisons of income distributions between regions of a nation with a common labor market but different supply situations. The independence assumption must be justified in all cases, however, and the necessity for it should be emphasized in presenting the model for general consumption.

Turning now to the empirical analysis, the most serious criticism one can make is that the authors claim too much for their theory. One could, of course, complain of the cavalier way in which they specify the basic relationship between log of earnings and years of education. But whatever the rough

model can do, a more refined model would do even better, or so can one expect as a central tendency.

The central finding is a strong association between low educational level of a population, large variance of education completed within the population, and high rates of return on education at all levels. There are at least three possible "explanations" consistent with the body of theory developed in the paper. In either of the two diagrams below, the three thick supply curves are those of the upper, middle and lower third of the U.S. northern population, distributed by degree of access to capital. The thin supply curves have the same definition for the southern population. In Figure 1, the southern distribution is all to the left of the northern one, but with the same variance; i.e., the same degree of inequality of opportunity. In Figure 2, the southern distribution coincides with the northern one in the upper third, but is elongated toward lower levels; i.e., there is greater inequality of opportunity in the South. The set of empirical results comes out in Figure 1 either through the assumption of a common *D* curve with marginal returns falling more than in proportion to investment level (in years of education), or through the introduction of two separate *D* curves, the southern one being higher and flatter than the northern one. In Figure 2, the empirical results come out without reference to the curvature of *D*. However, whenever a single *D* curve is assumed, the difference in rates of return is nil for any point specification of years of schooling: it only materializes by broad levels of schooling because more people are at the lower end of each category in the South than in the North.
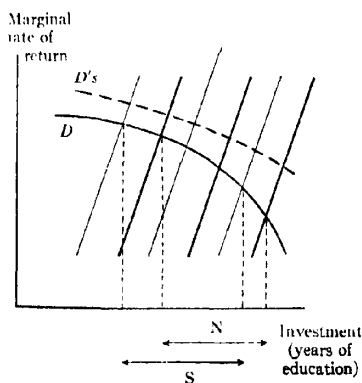
FIGURE 1
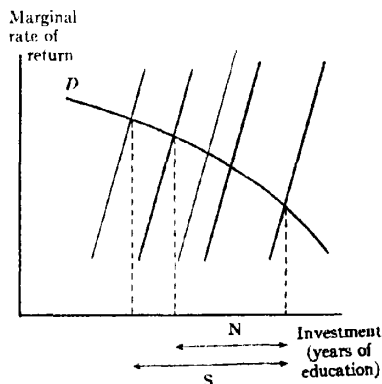
FIGURE 2

Once again, it is clear that the theory is too "open" as it stands: we cannot know what really happened—or predict what will—without reference to the labor market. In particular, the authors are wrong to conclude (or presume) that greater inequality of opportunities is behind the greater variance of education in the South: as exemplified in Figure 1, their theory is not tight enough

for that. In truth, they cannot even account for the higher rates of return of southern education, for their theory, as such, does not tell us anything about the shape of D. Except for specification of the relationship between earnings and years of schooling, what we have is a set of raw empirical findings (those outlined at the top of the previous paragraph), and some elements of a theory that could account for them.

Note, finally, that the rather belabored observations offered with respect to variance of earnings, coefficient of determinations, etc., constitute little more than direct logical implications of the empirical findings already listed, and thus do not add to the weight of evidence in favor of the theory. One supplementary element of information is indeed introduced: the variance of the residual (representing individual luck, ability, and postschool training) is larger in the South, but less so relatively than the variance contribution of school education. The authors want us to believe that this state of affairs is deducible from their theory and previous empirical evidence; but I am afraid their argument is less than convincing. At best, one can make the *ad hoc* assumption of same variance of the residual in both regions, and thus expect that a higher variance of earnings and a higher R will go hand in hand with higher variance of education (explanatory variable(s)) and higher values of the parameter(s) (the so-called "adjusted rate(s) of return"). All this is not very far from "mechanical curve fitting and *ad hoc* probability mechanisms," and it contributes little to the fulfillment of promises repeatedly made by "maximizing behaviorists."

THE ECONOMICS OF PUBLISHING

THE MARKET FOR PROFESSIONAL WRITING IN ECONOMICS*

By ALEXANDER J. MORIN
Aldine Publishing Company

When I was a graduate student just after the war, I succumbed as readily as my fellows to the great self-preserving myth of the academic community: that the highest virtue attached to the purest use of reason, which could best be pursued within that community. By this standard, I appear before you doubly damned, first as a renegade from academic economics and second as a commercial publisher, and on both counts as an exponent of reason most impure. But perhaps it is best so, because I am going to be talking about the economic behavior of economists, and on this subject the foresight of a publisher and the hindsight of a renegade can, I hope, be combined to sensible advantage.

I shall be concerned here only with "professional" writing, defined as the output of accredited members of the guild that is addressed either to other guild members or to apprentices. I shall also confine my discussion to writing that is intended for publication (though you might note that probably the greatest bulk of professional writing in economics today remains in the form of typed or mimeographed memoranda and reports prepared for government and industrial employers). To set the stage, I shall give you four sets of figures:

1. The rate of output of new publications in economics and sociology (including new editions, revisions, reprints and pamphlets as well as new books) has doubled in the past four years, rising from about 1,600 in 1961 to about 3,200 in 1964.[1]

2. The number of books in print dealing with economic subjects (a much narrower classification) rose from about 540 in 1960 to about 925 in 1965. Apparently, nearly half the economics books now in print were published in the past five years.[1]

3. The number of members of the American Economic Association rose by 84 percent in the past ten years, 29 percent in the past five

* I wish to acknowledge the advice and criticism I have received from Milton Friedman, Harry Johnson, and George Stigler. They may properly be held responsible for any gross errors this paper may contain, since they unwisely set a standard of perception and wit that I have tried vainly to emulate. I accept full responsibility for all minor errors.

[1] These data were generously provided by Mr. Chandler Grannis, Editor of *Publisher's Weekly*, the journal of the book trade in the United States. The absolute figures undoubtedly contain classification errors, collection inconsistencies, and other inadequacies and are used here only to indicate the direction and the rough rate of change.

years, 15 percent in the past two years, and 7 percent in the first nine months of this year (1965) alone.[2]

4. The number of graduate students enrolled for advanced degrees in economics in American universities rose from about 5,000 in 1960 to over 7,000 by 1963. On the basis of U.S. Office of Education projections, the total must now be around 9,000, and by 1973 it will be around 14,500.[3]

It is plain from all this that economic writing is a growth industry, whether measured by number of firms, quantity of output, size of market, or—presumably—share of the national income. There are now about 15,000 firms in this industry,[4] and I propose to consider their output as a somewhat peculiar kind of commodity and to look into some of the conditions of its production and distribution.

Professional writing in economics has two sectors, which can be characterized broadly as commercial and noncommercial. Commercial output is that which is purchased for resale, usually by the payment of royalties, and its economic motivation is obvious. Noncommercial output includes everything that is written and published without payment to the author, in journals, monographs, scholarly treatises, volumes of proceedings, government reports, etc. It must be clear to you at once that noncommercial writing involves costs and risks like any other productive effort, and if it did not similarly result in profits of some kind, all of you—being economic men—would go into some other line of business.

Of course, noncommercial writing is one of the things an economist is supposed to do; along with teaching and consulting, it is an activity by which his professional status is defined. The demand for this commodity is not direct but is derived from demand for economic skills more generally. What we have here is a joint product; its costs

[2] Derived from American Economic Association membership data provided by Professor Harold F. Williamson, Secretary, who also was good enough to furnish me with early proofs of the report cited in footnote 4 below.

[3] Derived from U.S. Office of Education data obtained through the courtesy of Dr. Preston Valien. Absolute figures are available for 1960 and 1963; the projections for 1965 and 1973 are made on the assumption that the number of students enrolled for advanced degrees in economics will rise at the rate projected for total enrollment for advanced degrees in all fields.

[4] Any count of the number of economists depends on a particular definition of the term For example, the American Economic Association had over 14,000 members in 1965 and the National Register of Scientific and Technical Personnel reported 12,143 economists in 1964, but the U.S. Census of Occupations classified 22,424 people as economists in 1960. The reasons for the substantial discrepancies in these figures are explained in the very interesting report on "The Structure of Economists' Employment and Salaries, 1964" that is being published as a supplement to the *A.E.R.* for December, 1965, and I will not go into them here. We can, I think, accept some figure approaching 15,000 for the number of persons who may be defined as professional economists, by training, specific occupation, or main interest; and we can assume that all professional economic writing is done by people within this group.

and profits are for the most part inseparable from the costs and profits merely of being a professional economist.

But writing is clearly preeminent among all the possible activities of an economist in establishing his status and therefore in determining his rewards. Dr. Frank S. Kristof, in a Columbia dissertation, prepared an elaborate regression analysis relating number of pages published to salaries of the faculty of three large midwestern universities just after the war. He found that (in terms of 1948 prices and incomes) these salaries rose by 50 cents per published page, regardless of the form of publication, up to a maximum of 1,400 pages.[5] This may be a spurious correlation; both published output and salaries may be related to other independent variables, such as intelligence, or energy. Nonetheless, Dr. Kristof's figures are illuminating, and the light they cast is made glaring by Professor George Stigler's estimate that a major article in a major economics journal is worth to its author between $10,000 and $20,000 in increased lifetime earnings. It is surely doubtful that any other activity of professional economists—teaching or consulting or whatever—would have a comparable long-run effect on their basic salary level or perhaps even on their total income.

The economic benefits of professional writing are recognized and reinforced by the "publish or perish" doctrine now prevalent in American academic life, and spilling over to affect the behavior of nonacademic professionals as well. The doctrine has an autonomous and brutalizing force of its own: theirs not to reason why, theirs but to write or die. This generalized pressure to publish may emphasize only one of the ways in which specialized knowledge can be put to use, but it accurately reflects the preferences of those who actually pay for the working time of economists. These buyers, like most purchasers of labor services, are concerned first with the productivity of their labor force, and publications are the only evidence of professional output that is readily measurable. Accordingly, deans and foundation administrators, grants and tenure committees, are obliged to concern themselves not so much with the form or content of the published work as with its tangible weight.

This lack of selectivity is transmitted in turn to the writers themselves, who sensibly seek immediate publication for every kind and scrap of writing they produce in the course of their professional lives: dissertations, book reviews, reports originally made to government or industrial employers, conference speeches, critical notes, commentaries

[5] After 1,400 pages the relationship ceased to hold. My information about Dr. Kristof's dissertation came verbally from Professor George Stigler, who at the time of the study, had 1,370 pages of published writing to his credit. Professor Stigler's estimate of the effect of a major article on lifetime earnings, cited later in this paragraph, also came verbally and so far remains unpublished.

on the work of others, commentaries on commentaries on the work of others—and, of course, economic essays, monographs, and books. Most of this writing is inevitably addressed to small and specialized audiences, and much of it—judging by what crosses my desk—is not addressed to any audience at all. Doctoral dissertations, for example, have become a sort of professional muscle-building exercise that is intended to satisfy only a small board of examiners, and their essentially private nature sets the pattern for much subsequent work. Such exercises are not ordinarily suitable for larger audiences and should be put on public view only when the performer is exceptional in grace or stature. But the pressure to publish does not encourage normative judgments of this kind.

The consequent growth in the supply of professional writing has been largely noncommercial in nature, and it has been met mostly by the creation and expansion of vehicles of publication established for this specific purpose out of necessity by the professional community itself. For example, thirteen (40 percent) of the thirty leading economics journals published regularly in the United States were established after 1945. Similarly, of the seventy-four university- or foundation-sponsored publishing houses that will bring out economics books if given sufficient inducement, 26 percent were established after 1945. Furthermore, each of these presses has greatly expanded its output, for books in economics presumably as much as for books in other fields; the total output of thirty-three of these presses that were in existence in 1938 had nearly doubled by 1964, rising from 707 books to 1,209.

Noncommercial output today dominates professional writing in economics, but the distinction between commercial and noncommercial work is in fact of fairly recent origin and is proving transitory in effect. Well into the twentieth century, all educated men were expected to be familiar with the principles of political economy, and the community of the educated was sufficiently large in relation to publishing costs to support the commercial publication not only of Mill and Marshall but also of the more specialized work of Edgeworth and Fisher. Professional economists communicated with each other noncommercially in the scholarly journals, as they still do; but their major output was in the form of books intended to educate and edify the larger membership of the intellectual elite. The dissolution of this elite and the displacement of "political economy" by "economic science" were both part of a general process of functional specialization in our society, and the increasingly narrow focus of most economic writing, which coincided with a rapid rise in publishing costs, compelled the expansion of the noncommercial outlets supported by the professional community. But in 1965 the number of accredited and apprentice profes-

sionals in economics and its major subfields has by itself become large enough to make profitable once more the publication of relatively specialized output, and the process is continuing.

The consequent blurring of the distinction between what is commercial and what is not is seen in the increasingly ambiguous role of the university presses. These presses were originally established within the academic community to meet its special needs of internal communication; their editorial judgments were to be based only on considerations of scholarly competence, and their financial losses—which under these circumstances were thought to be inevitable—were to be borne by the community they served. But now the university presses, or at least the larger among them, have moved into the shadowy world of the demimondaine. The growing size of the academic market has acted both as carrot and as stick: it has for the first time brought to the directors of these presses the temptations of potential profits, and simultaneously it has driven them into competition with commercial publishers for certain manuscripts that were formerly theirs by default. As a result, they have been led to emulate the skills of the commercial campus-walkers but without abandoning their amateur status (and the advantages of costless working capital and tax exemption that go with it). Accordingly, the publication of a book by a university press is these days less and less often accompanied by prestige alone and more and more often accompanied by the promise of royalties to its author.

The other side of this coin is that the commercial publishers have found that scholarship pays. There are now a considerable number of firms that bring out at their own expense specialized economic writing of a kind that was formerly left to the mercies of the subsidized publishers. This sort of commitment of resources may be made because the publisher is intellectually convinced of the merit of a book, or for prestige purposes, or because the firm wants to develop a list of textbooks out of monographs by particular authors or in a particular field; but in most instances it is undertaken simply in the hope and expectation that enough copies of a book will be sold to make the venture profitable.

The size of the market for these publications can be indicated in several ways. For example, take the circulation figures of the scholarly journals in economics. These are quite low, except for the *American Economic Review*, which is a perquisite of guild membership, and for journals in the fields of business or labor, which are written with a minimum of technical language and are addressed to lay economists rather than to professionals. Perhaps the most representative figures are those for the *Journal of Political Economy* and the *Quarterly Journal of Economics*, each well established and edited with high profes-

TABLE 1

SALES OF SELECTED BOOKS IN ECONOMICS

Year	Chamberlin: *Theory of Monopolistic Competition**	Schumpeter: *Theory of Economic Development*†	Samuelson: *Foundations of Economic Analysis*‡	Friedman: *Essays in Positive Economics*‡	Becker: *Economics of Discrimination*‡
1933	351				
1934	476	93			
1935	494	506			
1936	658	226			
1937	860	97			
1938	1,030	79			
1939	1,131	86			
1940	314	76			
1941	961	25			
1942	642	58			
1943	614	63			
1944	447	71			
1945	445	55			
1946	1,437	307			
1947	3,063	186§			
1948	2,976	—§	1,046		
1949	2,362	292§	1,223		
1950	1,573	1,055	720		
1951	1,138	648	519		
1952	655	608	137§		
1953	637	392	—§	784	
1954	569	378	878	368	
1955	508	289	491	202	
1956	656	505	478	170	
1957	1,129	342	474	170	806
1958	949	366	420	221	231
1959	789	426	547	255	138
1960	1,048	472	668	249	107
1961	1,010	775	848	294	100
1962	1,139	516	873	272	81
1963	834	250	848	431	138
1964	1,060	319	1,167	505	142
Totals	31,955	9,561	11,337	3,921	1,743

* Excluding 14,000 sheet sales in Great Britain and British territories.
† Sales in the United States only.
‡ Sales throughout the world.
§ Out of stock for part or all of the year.
SOURCE: These figures were generously made available by Mr. Thomas J. Wilson, Director of Harvard Univ. Press (publishers of the Chamberlin, Schumpeter, and Samuelson books), and Mr. Anders Richter, Managing Editor of the Univ. of Chicago Press (publishers of the Friedman and Becker books).

sional standards—and both with a circulation to approximately 5,000 individuals and libraries throughout the world.

Or take the sales records (shown in Table 1) of five books that seem

to me representative of specialized professional writing in economics at its best. The first two—Chamberlin's *The Theory of Monopolistic Competition* and Schumpeter's *Theory of Economic Development*—have become recognized as classics, the first soon after its publication in 1933 and the second certainly since attention shifted to problems of development after the war. Samuelson's *Foundations of Economic Analysis* is a basic work of modern mathematico-economic thought. Friedman's *Essays in Positive Economics* contains the most important and original of the shorter products of our distinguished chairman; and Becker's *The Economics of Discrimination* is a first-rate study dealing with an important contemporary problem. Surely it would not be unreasonable to assume that the Chamberlin and Schumpeter books should be read by all undergraduates majoring in economics, that the Samuelson and Friedman books should be mastered by all candidates for higher degrees, and that Becker's book should be used by a reasonably large group of professionals in the field.

Well, what of their sales? About 32,000 copies of *The Theory of Monopolistic Competition* were sold in the thirty-two years after its publication in 1933, while *Theory of Economic Development* sold a little less than 10,000 copies in thirty-one years (but the latter figures, unlike the others cited here, are for sales in the United States alone). Over 11,000 copies of *Foundations of Economic Analysis* were sold in its first seventeen years of life, about 4,000 copies of *Essays in Positive Economics* in twelve years, and fewer than 2,000 copies of *The Economics of Discrimination* in nine years. There are obviously two ways of responding to any of these figures, equally valid but differing in point of view. "Great Heavens," you might say, "is it not astonishing that fewer than 4,000 individuals and libraries throughout the world have felt it necessary to own Professor Friedman's book of essays?" Or you might say, "Good Lord, is it not astonishing that there are in the world nearly 4,000 individuals and libraries who actually paid for a copy of Professor Friedman's book?"

But it is more relevant to our immediate purpose to look at the trends within these totals. If we compare the sales of these books for the five-year periods 1955-59 and 1960-64, it is clear that many more copies were sold in the later period; the total for the group (excluding Becker's book, which was not published until 1957) rose by 45 percent. Even more important, sales of 500 or more copies have been typical for each of these books in recent years. These increments are in addition to the initial sales of each book that took place within a short period after its publication—and together, they are entirely sufficient to bring an avid gleam to the eye and a royalty-bearing contract to the hand of any sensible publisher. I doubt that any of these books would

have been welcomed by a commercial publisher as recently as twenty years ago.

These sales figures are still more meaningful if we look at them in terms of the size of the potential market for the products of your industry. Who buys the output of published economic writing and for what purposes?

First, there are the libraries. Publishers are always told by authors that "all the libraries will buy my book," and while this may be true, it is not generally recognized that the numbers involved are usually small. Fewer than 1,500 colleges in the United States granted bachelor's degrees in 1962-63, and of these, only 569 granted degrees in economics; only 223 universities granted Ph.D.'s, and of these, only 62 granted them in economics.[6] Only the least specialized book finds its way into general undergraduate collections; for all other books, the academic library market is confined to institutions where research takes place. If we add the few public libraries with research collections and the specialized government, business, and other research libraries, the total market does not exceed 500 or so copies for the ordinary professional book in this country.

Small though it may be, this market is especially important to publishers because it is relatively certain and not very discriminating. This is because librarians have been very properly trained into a passion for completeness, which they (and their consultants) can indulge at someone else's expense. I am reasonably confident that I could achieve the usual 500-copy sale to libraries of the Dubuque, Iowa, telephone directory merely by putting it in a good cloth binding and calling it something like "A Spatio-Economic Model of an Urban Communication Network." But that is all I would sell, to libraries or to anyone else.

Second, there are the core of professional economists who are consumers as well as producers of economic writing. Again this is not a very large group, judging by 1964 data from the National Register of Scientific and Technical Personnel. It is probable that most published professional work in economics is both produced and consumed by Ph.D.'s (5,061), or by those employed by educational or nonprofit institutions other than government (5,526, of whom two-thirds had Ph.D.'s), or by those engaged primarily in teaching or research (5,396, of whom 60 percent had Ph.D.'s).[7] Once we remove overlap, and even if we add the non-Ph.D.'s employed by government and industry who attempt to keep up with the growing literary output of economics, the total is not likely to exceed 6,000. But these economists, in buying for their personal libraries, are less dependable and more dis-

[6] U.S. Office of Education figures, obtained through the courtesy of Dr. Preston Valien.
[7] Figures based on analysis of National Register questionnaires, reported in "The Structure of Economists' Employment," *op. cit.*

criminating than institutional purchasers. Before they will spend money on a book, they have to be convinced that it is in some way necessary to them, either because it will play an immediate part in their own work or because it is generally important to the maintenance of their professional status.

It is these two groups together—the libraries and the economists who are highly trained and oriented toward research—that comprise the basic market for professional writing in economics. It is here that we find the subscribers to the professional journals and the buyers of Becker's *Economics of Discrimination* and similarly specialized books. It is, as you see, a rather small market in absolute size, and the proportion of the total that can be expected to buy any but the most exceptional publication is much smaller still. Output that reaches this market alone does not ordinarily result in publishing profits, and it is this kind of writing that today remains noncommercial in the strict sense.

Third, there are perhaps 10,000 professional economists who are engaged mainly in government, business, or industrial work and who have at most a master's degree.[8] Judging by my experience in trying to sell books to these people, their interest in theoretical work is low and their pragmatic needs are high. The business economists especially seem to buy few books that are not in some direct way related to the performance of their specific duties, and they are a diffuse and uncertain market for professional economic writing.

Fourth, economic writing is a commodity that can be exported with some ease, and there exists a substantial international market, at least for work that is not parochial in emphasis and data. The export market (including Canada) for American economics books that are reasonably general in content may amount to 50 percent or more of their total long-run sales. Most of these sales are in Canada, Great Britain, and other English-speaking countries; the remainder is scattered throughout the world, with a notable concentration in Japan. The British market can often be joined to the American by arrangements for combined printings made well in advance of publication, and this sometimes makes possible the profitable commercial publication of work that otherwise would remain in noncommercial hands. Such arrangements are more common for specialized work than for straightforward textbooks, which ordinarily are geared to curricular patterns that differ considerably from country to country.

Fifth, there are the apprentices to the guild, the graduate students, and the size and growth of this market was noted at the outset of this paper. Unless graduate students have changed greatly since my day,

[8] *Ibid.*

they read only what they must and buy only tools for the kit they think they will need later in their professional careers. But there are now a great many graduate students, who have research and fellowship funds that are a good deal more ample than in the past. As a result, they buy many books, collectively if not always individually. They buy synthetic textbooks, of course, but they may also buy classical and modern treatises that are used in lieu of textbooks. They may buy the standard works in their major subjects, and they may even buy the monographs that are important in their special fields of research. These are the buyers who each year add increasing amounts to the total sales of Professor Friedman's book and the others discussed earlier.

But note again Dr. Becker's book. My generalizations about the graduate student market may apply to all books in economics, but their effect is significant in commercial terms only for books of a reasonable degree of generality in fields that are central to the contemporary graduate curriculum. For such books, the graduate students may contribute several thousand copies to sales; and when these are added to sales in the core market of institutions and professionals and in the foreign market, the total becomes interesting to profit-seeking publishers. Accordingly, in recent years commercial firms have begun not merely to publish royalty-bearing books of this kind but even to commission them.

Up to this point I have been discussing economic writing—whether commercial or noncommercial—that is addressed to professional and would-be-professional audiences. I turn now to the largest and most important category of strictly commercial professional writing: the preparation of undergraduate textbooks.

The size of the market for these books is not clearly known, since we have no figures for the number of undergraduates majoring in economics. However, we do know that nearly 10,000 bachelor's degrees in this field were conferred in 1962-63,[9] and if we assume that this figure bears the same relation to enrollment in economics that the total number of degrees bears to total enrollment in that year, some 79,000 undergraduates were then majoring in the field. And if we further assume that this figure will rise at the same rate as total undergraduate enrollment, by now there must be some 100,000 economics majors, and by 1973 there will be around 130,000. These assumptions may be greatly in error—will we get our share of each new freshman crop?—and these estimates may be wildly inaccurate; but cut them in half, if you like, and the results will still inspire awe.

Undergraduates, except for those in advanced courses in advanced

[9] U.S. Office of Education figures, obtained through the courtesy of Dr. Preston Valien.

schools, buy few books other than texts. The pleasant intrusion of inexpensive paperbacks into lists of supplementary readings has not substantially altered the basic reliance of instructors and students on conventional, comprehensive textbooks of the familiar type. There is really little that need be said about the market for this kind of professional writing; the enrollment figures of your own colleges speak for themselves, as do the publisher's representatives prepared to provide you with refreshment at these meetings and awaiting your return to your offices.

In closing, however, there remains something to be said about the writing of textbooks. The academic investment in pure reason has often led to a defensive contempt for these books. They are not taken very seriously in the review sections of the scholarly journals, and they are at best disregarded and at worst disdained in judging the professional competence of their authors. Paul Samuelson, for example, is not only envied for the income he is reputed to have received from his introductory text—which seems to triple at each convention I attend—but I have also heard him criticized for it, as if this money were somehow tainted, obtained by unprofessional means.

I think this attitude, in which "textbook" is a dirty word, is nonsensical and irresponsible, and I would like you to join me in a campaign to stamp it out. I believe that it is the highest professional obligation of each teacher, scholar, and scientist to transmit to the next generation his understanding of the state of knowledge in his field—and this is precisely what is accomplished by a good textbook. It is no mean task of scholarship to synthesize new ideas with old, bearing always in mind the background and special needs of one's audience, in such a way as to raise the level of professional discourse in the future. This is what every textbook writer should hope and try to achieve, even while he is counting his advance royalties, and for this reason every such effort should be taken seriously.

We have a great tradition of books that have served this purpose for students of economics, from Smith and Ricardo through Mill and Marshall to Taussig and Samuelson. It is a tradition that deserves to be perpetuated rather than denigrated by responsible men.

THE PRICING OF TEXTBOOKS AND THE REMUNERATION OF AUTHORS

By PAUL M. HORVITZ
Office of Comptroller of the Currency

The textbook industry is one of particular interest to economists, economists are active both as consumers and as producers of the ind try's output. Despite the time economists spend with the products the industry, relatively little research has been done on some of basic economic problems of the industry.

College enrollment will continue to rise in the future and so sales of textbooks. Many new textbooks will be published in the n several years, and economists will write their share of them. Textbc publishers have benefited from increased enrollments and the trend ward larger per student expenditures on textbooks. Their earni have increased at a rapid rate. While it is clear that textbook pı lishing is a profitable business, it is not so clear that textbook writ is generally profitable. This paper examines two questions of conc to authors and publishers. First, how are authors paid for their eff and, second, a related question on which economic analysis sho shed light: how are textbooks priced. The focus in this paper is on c lege textbooks. The study is based on a questionnaire sent to auth of recent economics textbooks and on consideration of some elem tary aspects of price theory.

Pricing Textbooks

The process by which publishers determine prices is somewhat m terious. Most publishers have formulas that, while differing in deta are roughly similar in concept and yield similar results. Generally formula starts with an estimate of first-year sales. An estimate is ma of the direct costs of a first printing equal to estimated sales. This c can be estimated quite accurately. While per-unit costs could be lc ered somewhat by a printing run larger than estimated sales, there i strong aversion to carrying inventories. This is not irrational. Inven ry-carrying costs can easily outweigh the modest savings from lar printings. The resulting cost figure is then multiplied by some numl to determine list price. This number varies from publisher to publisl and from book to book, but is generally around 3½ to 5.[1] This provi the margin for overhead, royalties, advertising, and retailer profit.

[1] A higher multiplier is generally used for trade books, primarily because of the la retailer markup on trade books. These comments are based upon discussions with sev

If the resulting price is roughly in line with prices of competing books, the book will be published at that price.[2] If the formula-determined price turns out to be substantially higher than the going price, some reconsideration is in order. If the reason for the high price is inadequate volume, one alternative is a decision not to publish. Another alternative is to price the book on the basis of a printing of more than one year's sales, though this necessitates carrying heavier inventories than desired.[3] The book could be priced on the basis of a "split-run," that is, pricing on the basis of more than one printing, but this would violate what appears to be almost an article of faith in the industry that each printing (including the first) should pay its own way.

If the formula price turns out to be too high because of unusually high costs (a book with a large number of tables, illustrations, or complex mathematical symbols), an attempt may be made to reduce costs by cutting material (if the author is willing) or by setting type for the book in England or Japan where production costs are lower. One author responding to the questionnaire faced this problem and, reluctant to cut material, resolved it by agreeing to forego royalties on the first 500 copies sold.

It is not immediately obvious whether this procedure maximizes profit. Publishers tend to view the situation as one of a kinked demand curve—an increase in price above the prevailing level will result in a substantial reduction in sales, but a price cut will not significantly increase sales. The fact that there is some dispersion in textbook prices tends to cast doubt upon the applicability of the kinked demand curve approach to the textbook field.

My guess would be that neither a higher nor a lower average level of prices for his textbooks would increase a publisher's profits. This does not necessarily mean, however, that profits are being maximized. I have suggested that profit maximization might require relating book prices to the age of the book on the grounds that the profit maximizing price for a new book is necessarily higher than the profit maximizing price of that same book when there are used copies available.[4] This approach does not necessarily imply any change in the average level of prices.[5]

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publishers and review of the very limited literature on the subject. The best discussion of pricing is in Sir Stanley Unwin's *The Truth About Publishing* (Macmillan Co., 1960).

[2] The figure may be rounded up. A book will probably be priced at, say, $7.95 rather than $7.60, or $7.50 rather than $7.10.

[3] Occasionally inventory costs can be reduced by printing more than one year's sales but binding only a portion of those copies printed.

[4] Cf. *A.E.R.*, Sept., 1965, p. 844.

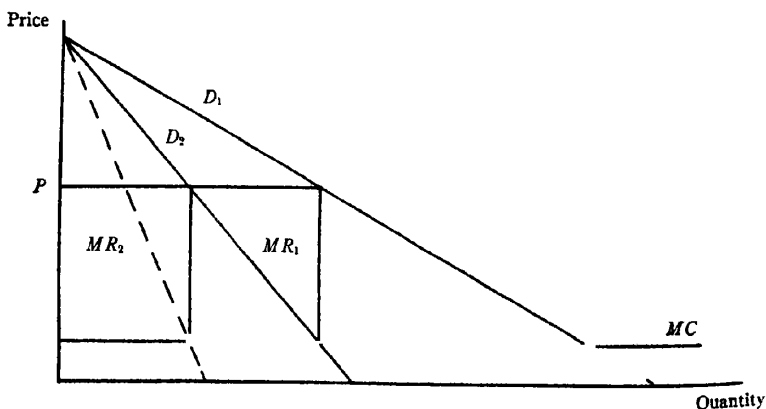
[5] One further question of profit maximization and textbook pricing has been frequently raised. Is it consistent with profit maximization for best-selling and poor-selling textbooks to carry the same price? Does profit maximization require that McGraw-Hill charge a higher price for Samuelson than for McConnell? It has been argued that the fact that these books are priced roughly the same indicates that the publisher is not maximizing

Paying the Author

While estimating profit is difficult, publishing firms are able to estimate costs quite accurately. The cost item of particular interest to economists is the author's remuneration. There are several ways in which an author conceivably could be paid. He could sell his manuscript outright to a publisher for a fixed sum. If the book is more successful than anticipated, the publisher reaps the benefit; if less successful, the publisher takes the loss. This is a rational manner in which to handle the uncertainty associated with forecasting sales of a new book. The publisher is generally better able to bear this risk than the author and simply on the basis of numbers is better able to insure against the danger of disastrous failure. The number of firms bidding for manuscripts would assure the author of a competitive return.

This procedure could be carried back a step or two further. Publishers could contract with authors before the manuscript is completed or even before it is started. This would reduce the risk to the author of putting time and effort into a product which is not marketable. It is a small step from this procedure for the publisher to hire the potential author, putting him on the publisher's payroll while he is working on the book. In fact, in the very high risk (from the author's point of view) field of elementary school texts, many publishers do this.

Despite the potential advantages of this system of remuneration, I

profits on the book for which demand is greater. In fact, assuming constant marginal costs (a reasonable assumption in this range of sales), the same price for the two books is consistent with profit maximization if the elasticity of demand is the same for the two books at all prices. For example, if demand for Samuelson were twice that for McConnell at all prices, profit maximization would require that both books be sold at the same price. Thus, in the diagram D_1 is twice D_2 at all prices, and the profit maximizing price, P, is the same for both.

have been unable to discover any recent college economics text which was bought outright by the publisher. Of course, contracts involving a large advance and a relatively low royalty rate approach this system, since publishers do not seem to attempt to recover any excess advance from authors. In fact, the practice of guaranteeing a specified level of sales, common in trade publishing, is now being used in the text field.

The general procedure, of course, is for authors to be paid on a royalty basis; that is, the author receives a specified percentage of the sales price of the book. The range of royalty rates is fairly wide—running from 10 to 16 percent of list price—but there is a strong central tendency. Most recent economics texts carry royalty agreements of 15 percent.

There is another means of remunerating authors that, while now quite uncommon, is of increasing importance. A few recent books have been published on a profit-sharing basis; that is, author and publisher share, on an agreed-upon basis, in the sales revenue remaining after costs are deducted. The usual arrangement apparently is for publisher and author to share equally in the net receipts (total revenue less certain costs). One obvious difficulty is specifying how costs are to be calculated. This system, of course, adds to the risk of the author. If the book is a poor seller, he will receive no income under this system, while under a royalty arrangement he receives income from the first sales. On the other hand, a very successful book will give the author a greater return than he would have received under the most favorable royalty arrangement. The profit-sharing plan is of analytical impotance because it overcomes some author-publisher conflicts that are inherent in the royalty arrangement.

Conflicts in Pricing

It is inherent in the royalty arrangement that pricing results in a conflict between the interests of author and publisher;[6] that is, the price which maximizes profit for the publisher is higher than the price which maximizes royalty payments for the author. Profit is maximized where marginal revenue equals marginal cost. Since the author is paid a percentage of total revenue, the optimal price from his point of view is where marginal revenue is zero. With negatively inclined demand curves and positive marginal costs, $MR = MC$ at a higher price than that at which $MR = 0$. (See Figure 1.) In other words, the publisher obviously must consider the cost of any additional books that will be sold as a result of lower prices, but the author, paid a fixed percentage of the price, is not concerned with costs.

Publishers have long been aware that their authors frequently argue

[6] The first exposition of this point is found in Arnold Plant, "The Economic Aspects of Copyright in Books," *Economica,* May, 1934, p. 185.

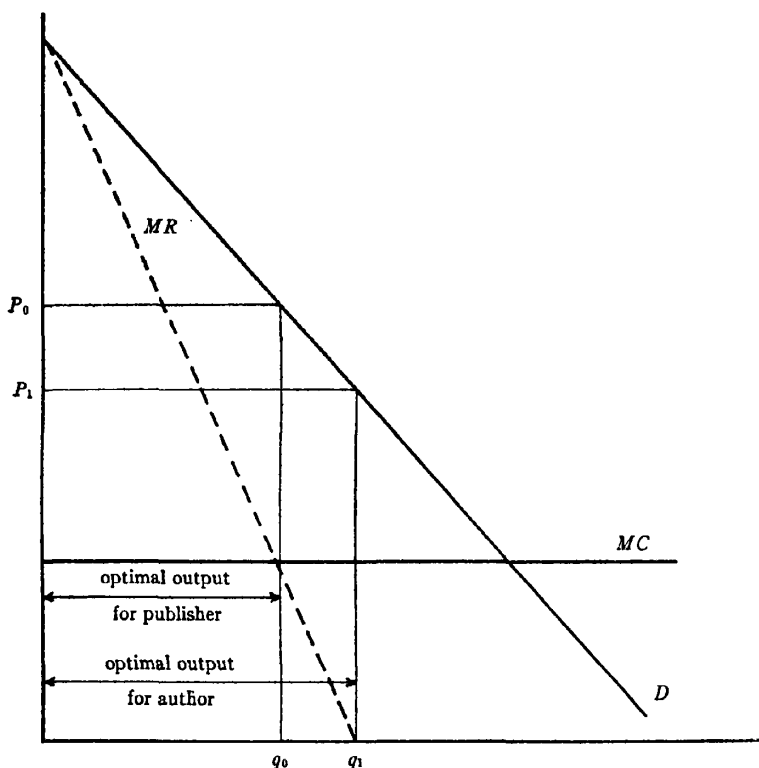



FIGURE 1

for a lower price. Unwin concludes that authors do so because they tend to overestimate the elasticity of demand and underestimate costs of production. This analysis, of course, misses the point that costs are irrelevant to the author and that the only assumption about elasticity necessary to justify a lower price from the author's point of view is that it be greater than one.[7]

Other Conflicts

There are other conflicts between author and publisher that result from the royalty arrangement. Advertising is perhaps the most important. Publishers frequently complain that authors ask for exorbitant

[7] And obviously it must be greater than one at the price the publisher has selected. If not, marginal revenue would necessarily be negative and he could earn more by raising the price, thus increasing revenue and decreasing costs.

advertising expenditures.[8] Authors complain that publishers fail to advertise their book adequately. Of course, under the royalty arrangement, a $1,000 additional advertising outlay that produces $100 in additional sales is advantageous to the author. Publishers do not seem aware of this conflict and in discussing advertising with authors stress the fact that sales are not very sensitive to advertising.[9]

Distribution of complimentary copies is a special case of the advertising conflict. Neither author nor publisher want to give a book to someone who would otherwise buy it. However, the author wants free copies distributed as long as there is some chance that additional adoptions will result. He is interested in distributing an extra hundred free copies if the result is an additional ten copies sold. Obviously, the publisher, who must pay for the hundred, would not consider this a profitable transaction.

A more significant distortion of optimum decision making resulting from the royalty system is found in the production process. Publishers may be inhibited from using higher-cost production techniques by the need to pay the author on the basis of the higher price that results. Suppose, for example, that the publisher estimates that using color in a textbook will increase costs by $5,000 and revenue by $5,500. There would seem to be a net gain by use of color. However, under his contract, the author may be entitled to, say, 10 percent of this additional revenue. In such a case the publisher will not find it desirable to use color. This situation arises, not only with respect to use of color, but also with regard to charts, illustrations, mathematical symbols and perhaps even pages (the publishers' preference for shorter textbooks may be based on the view that shorter books sell better but it may result from the fact that they cost less to manufacture). As Unwin puts it:[10] "If an author gets 10 percent royalty on a 7s 6d *cloth* book, and it seems advisable to do a *leather* edition at 10s 6d, ought he to expect 10 percent on the increased cost of the leather binding?"

A more basic conflict between interests of author and publisher lies in the differences in risks taken and investments made in publication of a new textbook. Publishers are constantly in search of new manuscripts for textbooks. A high percentage of the time of publishers' college salesmen is spent in attempting to unearth manuscripts under way and to encourage professors to write textbooks. Contracts are typically offered at a fairly early stage in the writing process. This is virtually

[8] This problem is perhaps more serious in trade than in textbooks.

[9] It may be stressed that, particularly in the case of textbooks, profit may not be the only concern to the author. The author's reputation (and hence his value in the academic market) may be favorably affected by wide knowledge of his authorship. In some cases advertising may fail to sell more books but may succeed in selling the author.

[10] *The Truth About Publishing*, p. 50.

costless to the publisher since, while the contract binds the author to the publisher, it does not commit the publisher to publish the manuscript. The publisher does not have to make any substantial investment until the completed manuscript is in hand and the decision to go ahead is made. It thus costs the publisher little to encourage authors to write books and to offer contracts without making any careful analysis of the quality of the manuscript or its chances of success.[11] Obviously the policies of publishers differ greatly in regard to the quality of their precontract investigation.

As a result, an author may be encouraged to put effort equivalent to more than a year's full-time work into a book which will not be published or which will yield him an insignificant return on his investment. It is possible, and perhaps is the general result, for a textbook to yield a satisfactory return to the publisher but an inadequate return to the author. Of course, the author in most cases has other than purely financial objectives in writing a textbook. Desire for prestige, desire to expound a new approach, and response to a "publish or perish" policy lead to some textbook writing in which the prospects for financial success are not important considerations. Even if profit seeking is not the primary motive for writing most textbooks, however, it is desirable that the potential author be aware of the potential conflicts between his interests and those of his publisher.

Survey Results

In order to determine the extent to which economist-authors are aware of these potential conflicts and to learn something of the income derived from textbook authorship, a questionnaire was sent to ninety-eight authors of textbooks advertised in recent economics journals. Response was excellent, with seventy-one questionnaires returned.

All publishers and most authors seem to believe that there is no inherent conflict of interest between author and publisher in regard to pricing. Most authors assume that since they are sharing the revenue with the publisher, what is best for the publisher is best for them. While we should not be surprised at this attitude if held by authors of English or physics texts, it is somewhat surprising how many economists have been willing to leave the pricing decision completely to the publisher. Only seven of the authors queried in my survey participated to any extent in the pricing of their book. Several indicated that it was quite naïve of me to suggest that the author (even an economist)

[11] The only significant cost to the publisher in signing a contract is the advance that may be necessary. Advances run around $1,000 and hence are no great barrier to casual signing of contracts. There are also costs associated with editorial time spent on the manuscript and payments for reviews. It should be pointed out, however, that many publishers bend over backwards to be fair to their authors and may make an unprofitably great effort to salvage a poor or marginal manuscript.

should participate. As one author put it: "They know more about pricing the merchandise than I do."

When asked if they were satisfied with the pricing of their book, nearly all said they were. It is interesting to note, however, that of the five who indicated dissatisfaction, all would have preferred a lower price.[12]

The conflict was somewhat sharper in the matter of advertising. Less than half of the respondents to my survey felt that their publisher had adequately advertised their book. One fairly frequent criticism of advertising should be mentioned. Several authors complained that the publisher did not adequately promote their textbook in trade channels. Books in banking, investments, real estate, etc., have a potential market among practioners that authors felt was not adequately exploited.

The potential conflict between publisher and author with regard to distribution of free copies of the book failed to materialize. Only three authors indicated that their publisher failed to distribute an adequate number of free copies.[13]

The questionnaire asked about payment arrangements. All but one of the respondents were on a royalty basis. As noted above, 15 percent is the most common royalty percentage. In some cases the royalty is expressed in terms of list price and in others is based on net price, but since the relationship between list price and net price is fairly standard (list = 125 percent net), it is easy to convert these to standard basis. For example, 15 percent of net price equals 12 percent of list price. Many contracts are on a variable basis with the royalty changing with sales. A fairly common arrangement is 10 percent on the first 5,000 copies, 12.5 percent on the next 5,000, and 15 percent on all in excess of that. These royalty rates are comparable with those paid on nontextbooks.[14]

The strong central tendency in royalty agreements is rather strange in view of the very wide distribution in sales of textbooks. A few books sell over 30,000 copies per year, while many sell less than 3,000. Of the forty-three replies to my questionnaire giving information on first year sales, twenty-six reported sales of 3,000 copies or less. Only three sold more than 9,000 copies.

There does not seem to be any correlation between royalty percentage and sales. Publishers appear willing to make the same royalty arrangement for an advanced book with small potential sales as for an

[12] Moreover, three of those who indicated satisfaction said they would have preferred a lower price. No author indicated a preference for a higher price for his book.

[13] However, six did indicate they felt that wider free distribution would have increased sales. This willingness to see things from the publisher's point of view is also evidenced in the advertising question. Many authors who felt that the publisher spent enough on advertising also felt that more advertising would have increased sales.

[14] See *Publishers' Weekly*, Mar. 1, 1965, p. 54.

elementary book with much greater probable sales. Several publishers apparently have a standard royalty agreement which is offered to all (or nearly all) authors without any attempt to discriminate on the basis of probable sales.

It seems clear from these data that most textbook writing is not likely to be a profitable use of an economist's time, particularly in view of the increasingly attractive alternatives available to economists.[15] Publishers, however, will continue to seek to persuade potential authors to write. Some of the conflicts between the interests of publisher and author obviously are ameliorated under a profit-sharing agreement. Profit-sharing agreements are not a solution to all problems, however. Allocations of costs would become a source of difficulty. It would still be difficult to determine the profit maximizing price or amount of advertising expenditures. It would not completely resolve the fact that the author is often interested in wide distribution of his book even at a cost of some reduction in profit. Furthermore, most authors do better under a royalty arrangement than they would on a profit-sharing basis. For these authors, however, it is likely that the direct income derived from their books will not be sufficient incentive to write. For those who feel compelled to write and who are confident that they have a good product, the profit-sharing arrangement does appear to have real advantages over the traditional royalty contract.

[15] I refer here only to royalty income. Promotions, pay raises, etc., that may be immediate by-products of authorship may make the venture profitable.

THE ECONOMIC RATIONALE OF COPYRIGHT*

By Robert M. Hurt
Princeton University

Completed and Edited
By Robert M. Schuchman
University of Chicago

This session is in part a memorial to Robert M. Hurt who died in the spring of 1965. His death is a tragic loss to many of us personally, and also to the profession of economics. Robert Hurt was a brilliant young man widely trained in economics and the law who combined his training with a sense of judgment, perspective, and unusual insight. He had a rare personal courage in following ideas where they led him and in standing up for what he believed to be true. His professional career had just begun, its promise foreshadowed by his early work, and reinforced for those of us who were fortunate enough to know him by his personal qualities. The paper presented today was left incomplete. It has been completed and edited by his close personal friend Robert M. Schuchman, also a lawyer, and also with interest in economics who is currently a Fellow in the Law and Economics Program at the University of Chicago.

—Milton Friedman.

The most spectacular and at the same time traditional forms of government encouragement of innovation are certain limited monopoly grants to private individuals. A copyright is a grant of the aid of state coercion to the creators of certain "intellectual products" to prevent for a period of years the "copying" of these products. My comments on the philosophy and economics of copyright will be limited to the early objects of copyright: books and periodicals. This unfortunately precludes a discussion of copyrights in musical compositions—the only category in which compulsory licensing has been widely required and the area in which intelligent economic analysis has been most successfully pursued. Such an investigation would, in all probability, require a separate paper.

We may group the various justifications offered in favor of copyrights under two headings: (1) those which are based on the rights of the creator of the protected object or on the obligation of society toward him and (2) those which are based on the promotion of the general well-being of society.

Under the first classification we should discuss two important theories in some detail and one more in passing: (1) the natural property right of a person to the fruits of his creation, (2) the moral right to

* The editor of this article is much indebted to Reuben A. Kessel, J. Randolph Norsworthy, Edmund W. Kitch, Burton C. Gray, and E. G. West for their suggestions and criticisms.

have his creation protected as an extension of his personality, and (3) his right to a reward for his contribution to society.

1. The claim that an author has an inherent property right in his writings, which right is merely recognized by the statute, has such wide acceptance that it seems at times to brook no opposition. Jurists, political philosophers, and economists have developed two divergent views of property which illustrate the conflict among supporters of the copyright system.

First, property rights can be viewed as a device whereby scarce resources will be subject to exclusive control rather than exploitation at will by all comers, with the result that they will be used in an economically efficient manner. This theory was latent in the Roman law development of the rights of property, with its emphasis on *dominium*, or exclusive control over tangible objects. The origin of the claim to a property right—working of the soil, gathering of the objects, gratuitous grant by the government, plunder, or theft—is not necessarily relevant; rather it is important that someone has control.

Second, property rights can be viewed as the right of each person to the exclusive control of the products of his creation. If a man brings a given commodity into existence, one who appropriates this commodity without the consent of the creator is guilty of theft, a proposition which becomes self-evident with the use of right reason. This later theory of property has its roots in scholastic jurisprudence and finds its most famous expression in John Locke's *Second Treatise*. It is inextricably tangled with the first theory in Blackstone[1] and finds its strongest refuge today in justifications for the expansion of patents and copyrights.

If one finds some credence in this line of argument, copyright is an ideal application. Locke derived a property right in the toiler from his cultivation of a small plot of land. However, most of the value was "found," not created, and the toiler's monopoly over the plot restricts all others from using the plot, a use to which everyone has a right in common in the absence of the toiler's activity and resultant exclusive legal right. On the other hand, a copyright truly deprives others of nothing which they would have had in the absence of the owner's creative activity. This line of reasoning was conclusive to Herbert Spencer: ". . . [A] production of mental labour may be regarded as property in a fuller sense than may a product of bodily labour; since that which constitutes its value is exclusively created by the worker."[2]

Following this approach, it becomes apparent that an argument

[1] 2 Blackstone, *Commentaries* (Ehrlich ed., 1959), pp. 115, 116.
[2] Spencer, *The Principles of Ethics* (1908), pp. 108-09.

which negates the property basis of patents does not apply to copyrights. As was pointed out over a century ago,[3] the patent monopoly does deprive others of a right which they would have had in the absence of the patentee's activity: the right to make the invention independently or to derive benefits from its application as a result of its discovery by a third person. Henry George, among others, used this distinction to justify support for copyrights and rejection of patents.[4]

The natural property right theory in its traditional form asserts a preexisting and perpetual absolute right of control over the benefits from a created work, to be parceled out at the discretion of the author. The main difficulty with this argument stems from an attempt to apply generally the principle upon which it is based. I can find no theoretical basis for isolating writings of authors from those benefits conferred for which the actor has no present means of collecting.[5] Others do benefit without paying when one buys and clears swampland, develops new mathematical techniques, or new methods of inventory management. When a large manufacturing firm builds a plant in a new area, many small businesses may as a result find it profitable to locate there, and we might be able to devise a legal mechanism which would allow the manufacturer to collect for his services, similar to the reduced rent enjoyed by a department store when it attracts other businesses to a new shopping center. The question raised here is not whether any satisfactory method of collecting for these "externalities" can be found, but whether government assistance should be extended to enforce the collection. As Sir Arnold Plant has said, "a special case for a monopoly for publishers cannot rest on the general proposition that if business men are enabled to make monopoly profits, some of them will be devoted to good works."[6]

2. Immanuel Kant defended copyrights by treating an author's works not as objects the benefits of which should accrue to the author, but rather as extensions of the personality of the author and subject to protection as such. This theory plays an important role in both the theoretical justification and actual content of the French, German, and Swiss copyright systems. Article 6 of the French Act of 1957 represents a practical application of the doctrine of moral rights: "The au-

[3] Phillips, *The Law of Patents* (1837), pp. 4-5; see also Finkelstein, *Antitrust Laws and the Arts*, in 10 Conference on the Arts, Publishing, and the Law 55, 66 (Univ. of Chicago Law School, 1952).

[4] George, *Progress and Poverty* (Schalkenbach ed., 1955), p. 411 n.

[5] The French approach is more consistent than the Anglo-American rationale. French legal protection is extended to authors for emotional, rather than strictly "economic" reasons, and their courts have been much more reticent to extent copyright protection to nonintellectual creations than their Anglo-American counterparts.

[6] Plant, *The New Commerce in Ideas and Intellectual Property* (1953), p. 15.

thor shall enjoy the right to the respect for his name, his authorship and his work. This right shall be attached to his person. It shall be perpetual, inalienable, and imprescriptable."[7]

The emphasis of this conception of copyright is on the author's privacy and reputation rather than his commercial interests, and as a consequence under French law the rights granted are virtually inalienable and in some cases perpetual. The three most important are: (1) the paternity right—the right to be identified as the creator of his works and be protected from plagiarism; (2) the integrity right—the right to protection against alteration or deformation of one's work, and the right to make changes in it; (3) the publication right—the right not to publish at all.

A lengthy discussion of these asserted rights is unnecessary here, because they could be substantially protected without resort to a copyright system. The enforcement of tort law protection of the paternity right would be rather simple, and the integrity right, although it would raise administrative problems, could also be enforced through the grant of a private cause of action. The right to withhold from publication, which is protected perpetually in France, is also covered by the perpetual common law copyright in the United States. The right of privacy, recognized as a legal right in the United States and the Commonwealth and invoked in analogous situations, is probably broad enough to cover protection of unpublished manuscripts in the absence of a copyright system.

3. Finally, as with patents, it is often argued that a temporary monopoly grant is a convenient way of fulfilling society's obligation to reward writers for their contributions to the general welfare.[8] This approach was adopted and a property justification rejected in the report by the Register of Copyrights in support of the pending United States copyright bill.[9] However, copyright seems to be an inefficient device for simply rewarding authors. The grant of tax relief to successful authors or direct payments for unremunerative literary creation would probably be more convenient, and these alternative forms of reward are actually given today. The Internal Revenue code, § 1302, allows certain authors to "spread-back" the income from the sale of a literary work over a period up to thirty-six months, in order to mitigate the tax consequences of receiving lump-sum compensation for work which requires long periods of unremunerative preparation. The multitude of book prizes and literary awards constitutes an important private method of rewarding authors for their contributions to society.

[7] Law of Mar. 11, 1957, art. 6 (France); see commentary in Bonnefoy, *La nouvelle législation sur la propriété litteraire et artistique* (1959), pp. 79-80.

[8] See e.g., Phillips, *op. cit. supra*, n. 3, at p. 12.

[9] 1 U. S. Copyright Office, Copyright Law Revision, c. 1 (1961).

For those who reject the approaches outlined above, the copyright system will almost inevitably be judged by its effect upon some notion of the general welfare: it is held to be a necessary supplement to the free market in promoting the best allocation of scarce resources according to the priorities of human wants.

It is often argued that potential publishers must incur technological and risk costs which may be avoided by competitors who can copy a first edition, with the result that the price will be driven below the first publisher's costs; hence, there will be no incentive to publish in the first instance. The private return to publishers and authors, in the absence of copyright protection, is held to be smaller than the economic value of their literary products to society. The general welfare will therefore be enhanced by enacting copyright legislation which encourages the creation and publication of manuscripts that otherwise would not have come into existence. This argument will be examined here by raising three questions:

1. Does the copyright system induce the creation of new goods which would not have been created in the absence of copyrights?

2. Assuming that new goods are produced, are these goods valued higher by consumers than the goods which would have been produced if resources had not been diverted to literary production by the copyright system? In other words, is social welfare enhanced?

3. If there is a benefit from the copyright system, is it offset, at least in part, by various administrative costs and frictions inherent in the system?

The most important, although at times unstated, premise of copyright apologetics is that literary effort and production will decrease or virtually disappear if copyright protection is removed. In any discussion of this assertion, the incentives or motives of authors and publishers should be distinguished. Both are beneficiaries of the copyright system, but their degree of dependence upon it is quite different. We shall discuss the position of the author first, and then that of the publisher.

The massive literary and dramatic production during the centuries before copyright protection was enacted demonstrate that there are other motives for the creation of intellectual property than the expectation of monopoly profits. Many authors commit ideas to paper without any intention of having them published. Some, like Franz Kafka, express a wish that their creations be destroyed upon their death. Other authors actually pay for all or part of the costs of publication of their creations, in order to satisfy other desires than direct economic remuneration. These desires include the propagation of partisan ideas; notions of altruism, as in the case of religious and moral tracts; desire

for recognition; and enhancement of one's reputation. There are also cases where authors pay for the costs of publication in order to promote some other pecuniary interest than sale of the writing itself, such as advertising copy and scholarly publication induced by the "publish-or-perish" rule of most universities. Finally, there are authors who submit manuscripts without any hope of receiving financial remuneration.

While the productions of the authors described above may include the most unreadable and esoteric, it is more likely that they contain a large part of the very contributions to cultural and scientific progress which the proponents of copyright deem to be in need of monopoly protection. Moreover, most articles in the scholarly journals are submitted without pecuniary compensation. But there are types of creative effort which require pecuniary reimbursement for the author before the work will even be conceived; this occurs when the costs of creation, as opposed to publication and distribution, constitute a significant proportion of total costs, and these costs need not be incurred by copiers. Prime examples of this kind of product are encyclopedias, almanacs, mass circulation periodicals, technical subscription services for professions (such as citators and digests for lawyers), and motion pictures. Finally, we have those works which are created primarily, if not exclusively, for the purpose of providing an income for their authors.

It seems fairly clear that without some device to assist authors in receiving compensation for their services, some works with high costs of creation, as well as literary creation induced by the expectation of incremental income from subsidiary and reprint rights, may not be produced at all. However, it does not necessarily follow that the grant of a copyright monopoly is the only such device possible, nor that it is the most desirable device. If we wish to encourage works which require long periods of research or high costs of creation before they reach the publishing stage, it may be preferable to support authors during the period of production rather than during the moment of potential income protected by the copyright laws. This can be done through private patronage by tax-exempt foundations, universities, and the like, or even by government support for desired literary creation.

The role of the publisher in the realm of intellectual property is substantially different than that of the author. Although there are religious and partisan publishing houses whose function is to subsidize certain types of literary products, most publishing enterprises are engaged in business for profit. The function of the publisher is almost exclusively entrepreneurial: he contracts with an author for the right to issue a book, then contracts with others to have the book printed

and bound, and finally contracts with bookstores or book clubs to have the book sold to the reading public. A publisher need not invest in physical equipment, such as paper, printing presses, or bookbinding machinery. Consequently, capital requirements for establishing a publishing house are low (estimated at $100,000 for a moderate-sized company),[10] the industry is highly competitive, the number of firms is large, and entry is so free that publishing has been called "Adam Smith's kind of industry."[11]

The very ease of entry into the publishing business is the major factor cited in favor of the necessity for copyright protection. A copying competitor can avoid the costs of royalties and editing, and, with the aid of new methods of electronic copy making, he can seriously reduce the cost of composition as well. Most important, a copier avoids the cost of risk: he can wait on the sidelines until a literary venture is accepted by the consumers, and then proceed to market reprint editions at a price below the costs of the initial publisher, compelling the latter to undergo a loss. The publishers argue that even with copyright protection, they produce most books at a loss. These are allegedly published because they are subsidized by the profits from the few successful ventures and by returns from subsidiary rights, such as book club distribution, paperbound reprints, and dramatization, film, broadcast, serialization and translation rights.[12]

This gloomy description to the contrary, there are methods of avoiding bankruptcy available to publishers, even without a monopoly grant. The most important factor is the advantage of being first on the market with a new book. This advantage is somewhat analogous to that of the fashion leaders in the ladies garment industry in the United States, who thrive even though unprotected by patent or copyright against the pirating of designs. During the nineteenth century, there was no copyright protection for the publishers of foreign books in America, yet American publishers deemed the right of first publication sufficiently valuable to justify the voluntary payment of royalties to British authors in order to secure a first edition.[13] In fact, English authors often received more from the sale of their books by American publishers than from their British royalties.[14] In his minority report as a member of the 1876-78 Royal Commission on Copyrights, Sir Louis Mallet argued in favor of the abolition of the copyright monopoly in economic terms: ". . . [I]t will always be in the power of the first publisher of a work so to control the value, by a skilful adaptation of the

[10] Lacy, "The Economics of Publishing," *Daedalus*, Winter, 1963, p. 45.
[11] *Ibid.*, at p. 46.
[12] *Ibid.*, at pp. 49-50.
[13] Plant, "The Economic Aspects of Copyright in Books," *Economica*, 1934, p. 172.
[14] *Ibid.*

supply to the demand, as to avoid the risk of ruinous competition, and secure ample remuneration both to the author and himself."[15]

Mallet's argument should be analyzed further. The publisher's fixed costs of production on each title are very high relative to total cost, and the combination of high fixed costs and declining marginal costs means a rapid decrease in average costs. The entrepreneurial function of the publisher includes the ability to gauge demand conditions correctly and publish a first edition that will saturate demand at the projected price. Should a copying competitor publish a rival edition? The copier's fixed costs would be lower, but with marginal costs for both publishers relatively low and declining, it would probably be necessary for the copier to publish an edition in the same size range as the first edition in order to bring his average costs significantly below those of the first publisher. The result obviously would be a doubling of the copies offered for sale. If the first publisher had estimated the demand correctly, there will be a large volume of unsold books until the price is lowered sufficiently to reach a new market, since demand will not clear the market at the traditional price. Marginal costs for both publishers being about the same, the first publisher is equally capable of slashing prices to dispose of his stock.

The mere threat of retributive behavior by the first publisher should be sufficient to deter the copier until the first edition is sold out and the first publisher has made his profit. As a matter of fact, this was precisely the method used by American publishers in the nineteenth century to protect their uncopyrighted works by British authors from the copiers. If a pirated edition appeared, the first publisher responded with "fighter" or "killer editions"—extremely cheap editions designed to drive prices below even the copier's costs, in the hope of establishing a reputation for retributive conduct which, while self-defeating in the short run, was highly effective in curbing the pirates in the long run.[16]

In addition to being first on the market, there are other means available to a publisher to secure his position without copyright protection. The contract with the author may reserve exclusive rights to new introductions, additions and revisions by the author to subsequent editions issued by the first publisher. Moreover, the first publisher can obtain prepublication orders from interested groups and individuals, a business method which is used today. Finally, the authorization of the first edition by the author may be a marketable asset, at least among those readers who strongly believe in the right of an author to the fruits of his creation. This device is currently being used to promote an authorized soft cover edition of the *Lord of the Rings* trilogy of J. R.

[15] *Ibid.*, at p. 193.
[16] *Ibid.*, at p. 173.

R. Tolkien, which is in competition with a copied edition that pays no royalties to Mr. Tolkien.[17]

We return once again to the question whether certain works are published solely because of copyright protection. It seems clear that not all publication is dependent upon such protection. But copyright protection does lead to the creation of new goods by encouraging the assumption of greater risks. Publishing ventures in which the possibility of profits from reprints and subsidiary rights offsets the fear of probable loss on a first edition are very likely the creatures of copyright.

Having tentatively concluded that copyright protection encourages the creation and publication of some literature, we may inquire whether the reallocation of resources induced thereby is conducive to the general welfare. Two important theoretical assumptions support the welfare argument for copyright. First, without copyright protection, the private economic return from literary creation (to author and publisher) will necessarily be smaller than the economic benefit of this activity to society. As a result, it is argued, the quantity of literary creation will be insufficient to maximize welfare. Second, literary production, like education, should be encouraged because it has greater intrinsic merit than its alternative product, perhaps because of its long-run neighborhood effects.

The second of these assumptions is the more difficult to assess. It is in the nature of a primitive value judgment and is not amenable to analytical treatment. We can intuitively discern books which are less meritorious than given alternative products under any conventional value standard. Even if literature is an intrinsically superior product, it still does not follow that copyright protection is the best device for inducing the optimal number of books. We are compelled to return to the first assumption, that copyrights are necessary to close the gap between private return and public benefit.

As an initial step towards measuring the welfare effects of literary creation, we should try to distinguish the categories of books which are encouraged and the returns which are enhanced by copyright protection. A convenient method of accomplishing this is to isolate the different kinds of risks taken in the book industry. They may be divided analytically into three categories:

1. Those ventures which are expected to and do cover costs even in the absence of copyright protection. Copyrights are not necessary for the encouragement of these risks. Rather, copyright protection

[17] Mr. Tolkien's authorization states that "this paperback edition, and no other, has been published with my consent and co-operation. Those who approve of courtesy (at least) to living authors will purchase it, and no other."

artificially enhances the private returns on these ventures and leads to the distortions of monopoly pricing. We can justify the existence of copyrights here only if the enhancement of private returns tends to equate them with the public benefit from the ventures involved.

2. Submarginal ventures which cannot cover costs despite the assistance of copyright protection. If the author and publisher correctly anticipate the returns on these ventures, they will not be undertaken regardless of the existence of copyrights. But if copyright protection exists, average returns for literary production will be enhanced as a result of the monopoly gains enjoyed by the successful ventures in our first category. This will encourage the production of some submarginal literature, insofar as it engenders a fallacious expectation of returns. Unless we are prepared to quantify the assumption that literary production is more meritorious than its alternative product, it would seem that a misallocation of resources occurs when literary creation is encouraged by this fallacious expectation.

3. Literary ventures which are able to cover costs only if copyright protection is available. Even if we assume that a first publisher successfully protects his first edition by the threat of retributive conduct, in the absence of copyrights he will be compelled to share his return on subsequent editions with competing copiers. If he cannot cover costs in this situation, he will not publish at all. With copyright protection, the risk taker will be able to take full advantage of the secondary markets. Through the use of rational price discrimination, what otherwise would have been a submarginal venture will be able to command an average price which covers average costs. The ventures in this category constitute the only literary production which is economically feasible solely because of copyright protection. The encouragement of these ventures will add new commodities to the market. The welfare effect of these products of the copyright monopoly will be a function of this increase in the alternatives available to consumers, discounted by the loss of the alternative product of the authors and publishers induced to engage in literary production.

We may now summarize our welfare analysis of the copyright system. The encouragement of ventures which require discriminatory pricing to cover costs may be accounted on the positive side of the welfare equation, either on the ground of the intrinsic relative merit of literary creation, or because the market is held to be an inadequate mechanism for equalizing private return and public benefit. On the other hand, copyright protection will cause scarcity pricing of those ventures which can cover costs even in the absence of such protection; this will presumably lead to the production of less than the optimal number of copies of each title in this category, thereby decreasing con-

sumer welfare. Finally, the encouragement of submarginal literary production due to fallacious expectation will be reckoned on the negative side of the welfare ledger, unless we apply the intrinsic merit thesis and assume that it has no relation to consumer preferences.

As a postscript to this analysis, we shall make a brief examination of some costs and frictions inherent in the copyright system. The serious legal problem of "extension of monopoly" which plagues the field of patents is not too important in the copyright area. The only serious cases in this country involving the use of copyrights as a tie-in device concern the motion picture industry. In *Alden-Rochelle* v. *ASCAP,*[18] the court held that a combination between the American Society of Composers, Authors and Publishers, the agent for most American composers of popular music, and motion picture producers was an illegal restraint of trade and an attempt unlawfully to extend the monopoly of copyrights in motion pictures and musical compositions; more specifically, ASCAP was declared to have achieved monopolistic domination of the music integrated in sound films, in violation of Section 2 of the Sherman Act.[19]

The United States Supreme Court ruled in the case of *United States* v. *Paramount Pictures, Inc.,*[20] that the practice of "blockbooking," i.e., the tie-in sale of a group of motion pictures by producers, was illegal since it " 'adds to the monopoly of a single copyrighted picture that of another copyrighted picture which must be taken and exhibited in order to secure the first.' That enlargement of the monopoly of the copyright was condemned below in reliance on the principle which forbids the owner of a patent to condition its use on the purchase or use of patented or unpatented materials."[21]

It has also been held, in *Interstate Circuit, Inc.* v. *United States,*[22] that motion picture copyright privileges do not authorize a contract between theater owners and movie distributors which restricts the terms on which the films may be rerun in other theaters.

Beyond these few instances, the copyright system does not seem to have raised many antitrust problems. Governmental expenditures for court and administrative costs are minor as compared with those for the patent system. Copyright law is relatively simple and certain and does not require the technical expertise needed in patent infringement actions. Lay ignorance concerning copyright laws will result in some wasted resources, and some people may prefer to forego literary activity rather than obtain written permission from copyright owners when com-

[18] 80 F. Supp. 900 (S.D.N.Y. 1948).
[19] See also *N. Witmark and Sons* v. *Jensen,* 80 F. Supp. 843 (D. Minn. 1948).
[20] 334 U.S. 131 (1948).
[21] 334 U.S. at 157.
[22] 306 U.S. 208 (1939).

pelled to do so by law. However, we may conclude that the inherent costs and frictions of copyright administration are too insignificant to have a serious effect upon our welfare analysis of the system.

Conclusion

In so preliminary an inquiry as this, we can do little more than try to find the direction in which further investigation would be useful. It is clear, however, that any judgment concerning the desirability of a copyright system will be a function of our criterion of justification for it. If we agree with the French revolutionaries that natural rights in literary property do exist,[23] then we would likely support a copyright law as a codification of preexisting rights. If we believe in the theory that an author is entitled to certain moral rights as an extension of his personality, then traditional tort law protection could satisfy our objectives short of a copyright monopoly grant. If we are attracted by the analysis of the Register of Copyrights,[24] that society has an obligation to support the creators of literary products, our goal could be achieved by other methods of reward than copyright, such as tax exemptions for royalties or payment of cash bounties for literary creation.

Finally, we must consider the view that copyright protection should be judged by its effect upon economic welfare. Here we enter an inconclusive area of speculation. However, we can say that the traditional assumption that copyrights enhance the general welfare is at least subject to attack on theoretical grounds; the subject certainly deserves more investigation and less self-righteous moral defense.

[23] The French revolutionary, Le Chapelier, defended the concept of the *droit d'auteur* with the statement that "La plus sacrée, la plus personnelle de toutes les propriétés, est l'ouvrage fruit de la pensée d'un écrivain." Quoted from "Le Moniteur Universel" in Dock, Étude sur le Droit d'Auteur (1963), p. 152.

[24] *Op. cit. supra*, n. 9.

DISCUSSION

Rueben E. Slesinger: Among the issues raised by Horvitz in his enlightening discussion of textbook pricing one that stands out questions whether publishers maximize profits. He speculates as to how much of a premium a publisher might charge for a best selling textbook in contrast to a less popular volume. But, since the usual practice for a publisher with several competitive books is to price them at approximately the same price, the profit-maximization guide is questioned.

A further question might be asked: what is it that makes for a best selling textbook? Might this not be a case of monopolistic competition with a high degree of product acceptance (brand) associated with a given text, enhanced by the publisher's advertising policy and the prestige of the author? Price differentials between competitive texts do not appear to be a significant determinant of purchase.

It is appropriate to investigate the demand side of the market and examine why teachers select particular texts. Many adoptions are based on the prestige associated with given textbooks, with little more than a cursory examination at the time of adoption. A variation in the price of a new book becomes a minor consideration in the adoption decisions. Most teachers often have little more than a hazy knowledge about the price of books that they adopt.

Publishers seem to price their new textbooks near that of the leaders. This is not to imply collusion; rather it is the application of market knowledge in an industry characterized by competition among small numbers. Although price leadership is common, there may be varying leaders as different publishers are fortunate to win a "hot" manuscript.

As to the possibility of stimulating inroads into a market by price cutting for new textbooks, it is apparent that publishers do not seem anxious to stir up price wars. The use of paperbacks might be considered as a type of market discrimination, without an overt price war. Seldom do we note a publisher offering cut prices to move a text that is not selling; here, there might be some conflict between the interest of the author whose royalty payments might be increased through such extra sales and the publisher who is interested in the general stability of the industry.

Another assumption is that publishers tend to view the demand for a particular textbook inelastic enough so that a price cut does not significantly increase sales. A priori experience tends to bear out this assertion. The question of price elasticity of demand is more important in setting the original price for a book rather than in making later adjustments. Although the tendency has been to increase the list price continously throughout the life of the book (probably a cost-push effect), there are few examples of the listed price being dropped as a book passes through additional editions and initial fixed costs are spread.

Horvitz' discussion of the areas of possible conflict between authors and

publishers and the techniques for payment of royalties is enlightening. It would have been interesting to have carried this analysis further to study the dissatisfaction of those authors who are unhappy with their publisher relationships. Many authors are so pleased at finding a publisher in the competitive academic world that they might be reluctant to voice criticisms, either as to sales policy or royalties.

It would be well for both aspiring economists and those who have already aspired to read Mr. Morin's paper. Economists are likened to businessmen engaging in "economic behavior" in the marketing of their writings. The statistics are generally discouraging as to writing in economics as a source of income.

There is no doubt that the "publish or perish" guideposts of many universities have been responsible for a great deal of the outpouring of economic writing, especially of the noncommercial type. Indeed, much of the motivation behind the proliferation of journals seems to stem from the increasing incidence of this policy. One may question how much of what is contained in many of these recent journals, although not written for pecuniary gain, is noncommercial. One beneficial effect, however, has been the opportunity given to less recognized economists to "break into publication" and to enhance their chances of having an article accepted later by a more prestige-type journal.

A repercussion emanating from the publications pressure involves the effects on the "quality of teaching" as pressures that causes one to "seek publication for every kind and scrap of writing" increase. This is complicated, as Morin indicates, by the lack of selectivity on the part of administrators in judging what has been written.

With reference to the author's comments on who should be counted as an economist, economics is unlike many other professions: there are no "professional examinations" that must be passed prior to acceptance as an "economist."

Another area of investigation that would be revealing would involve a study of the degree to which a small group of economists dominates the prestige journals. Also, what is the concentration among this group of the better selling "commercial works"? There is strong suspicion that success in the "noncommercial" area enhances the chances of success in the commercial field.

One item, not mentioned in discussing the relations between author and publisher, merits some mention. Perhaps the proliferation of manuscripts might relate to the practice of the "campus-walkers" representing the publishers and acting as "bird dogs" in unearthing manuscripts. It is not difficult to secure a contract for a manuscript, but this by no means assures that the work will be published. If the manuscript is rejected, the author now has an investment in time, and so begins the peddling process to find another publisher. Possibly a more thorough analysis of potential manuscripts prior to the signing of contracts might be helpful.

The heart of the Hurt and Schuchman analysis may be summed up in their questioning "whether unanimity on this subject [copyrights] is due to sound thinking or mere habit." The authors examine possible justifications for copy-

right protection, and distinguish between the interests of authors and publishers. Although items other than books, articles, and the like are mentioned in passing, the major attention is given to the latter.

The rationale behind the extension of copyright protection appears to be both a protection for and an encouragement of "mental effort," regardless of its specific form. If one person writes a book, why should he be in better position to receive monopoly privileges than another who develops an "artistic design" or a computer program? If there is any justification for the use of copyrights, it can only be sustained on a fair and equitable basis for all types of mental effort.

Copyright protection is the basic technique by means of which an individual exerting mental effort may reap added pecuniary gain for his efforts. True, in the academic area publish or perish practices may stimulate authorship for noncommercial reasons, but these publications constitute only a small fraction of all the items that are subject to copyrighting.

The best argument for the continuation and extension of copyrights appears to be the one which looks upon a copyright as a "temporary monopoly grant" by which society fulfills its obligations and rewards the owner for his contribution to general welfare. Although many "bad" things may be copyrighted, there also are many "bad" things that reach the market in general. Preferably, the market should be the guide of what is "good or bad" and not some congressional edict. Suggestions to encourage mental effort other than by copyright, such as tax relief, are inequitable; other persons, too, find their incomes lumped in particular years.

The authors go far to develop the welfare concept as a basis for the granting of copyright privileges; they examine the influence on the allocation of resources that copyrights might exert. What is overlooked, however, is that the subject may contribute little else than his time and his opportunity costs are low.

So long as the idea of property rights continues to be basic to American law, the concept of a property right in mental works and their reproduction seems to be justified as an equitable manifestation of this philosophy.

ROBERT W. FRASE: The three papers presented by the panel speakers deal with many separate aspects of book publishing.

Mr. Morin's paper is a useful description of the market for professional economic writing. It contains nothing to which I would take exception. If I were not severely limited in time and space, I should have liked to amplify his brief comment on the foreign market, especially for economics textbooks.

Mr. Horvitz' paper deals with possible conflicts of interest between publishers and authors in the pricing and advertising of college textbooks. He makes what appears on the surface to be a good theoretical economic case that under the usual percentage royalty system authors would benefit from prices and advertising expenditures that would be uneconomic for publishers. Nevertheless, he finds that in their response to his questionnaire directed to authors of economics textbooks, these economist authors seem on the whole content to leave

these matters in the hands of their publishers. I believe that his respondents have arrived at the right conclusions.

If he had sent a questionnaire to publishers of college textbooks, Mr. Horvitz would have found that his assumptions about publishing practice were not correct in a number of important respects. Limitations of space require me to cite only one example. College textbook publishers do not set the price of a textbook at a level which is estimated to recover all costs plus a profit on the sales of a single year. On the contrary, the price is generally set in terms of a three- to five-year period representing the prime sales life of a book, which generally involves several reruns as well as the original printing. As in the case of the Hurt paper, which I shall discuss in more detail, this is an example of the difficulty of applying economic theory to practical situations. Theory is not a useful tool unless the facts from which it starts are correct.

The Hurt paper deals with a much broader subject: the economic justification of the temporary monopoly granted by the copyright law. Although the subject is copyright in general, his analysis is limited to the writing and publishing of material for books and periodicals. As a practicing rather than an academic economist and one who has spent a large part of his professional life in the book publishing field, I confess that Mr. Hurt's questioning of the basic economic justification of copyright came as something of a surprise, not to say shock. In the United States alone the sales volume (receipts of publishers) of the book publishing industry is now about $2 billion a year. In physical terms this is over 25,000 new and revised book titles a year, over 150,000 books in print, and sales of over 1.2 billion copies—or more than six copies per capita, one of the highest rates of book consumption in the world. The annual rate of growth in sales has been on the order of 10 percent per annum in the entire period since World War II in both the domestic and export markets.

This record would seem to indicate in purely pragmatic fashion that the economic basis of American book publishing is sound; and it has been a matter of faith in publishing circles that copyright was the indispensable legal and economic foundation of the book publishing industry.

Recovering somewhat from my surprise, however, I must admit that the proper function of the academic and theoretical economist is to let no assumptions go unchallenged. It was with great interest, therefore, that I examined Mr. Hurt's analysis of the economic justification for copyright.

Reduced to basic terms, Mr. Hurt's argument comes to five conclusions, on each of which I would like to comment:

1. "*However, copyright seems to be an inefficient device for simply rewarding authors. The grant of tax relief or direct payments for literary creation would probably be more convenient, and these alternative forms of reward are actually given today.*" This statement is so sweeping and so little supported by argument or evidence that it is hard to take seriously, either as economic analysis or social prescription. Tax relief is only helpful when one has an income on which to pay taxes. Yet here tax relief is suggested as a substitute for a means of earning income. Direct payments for literary creation—

and here literary prizes are mentioned—would not only require vast increases in the sums now available for this purpose but would also deprive authors of their principal means of maintaining financial, and therefore literary, independence. It would take us back to the days when wealthy patrons and state subsidies could determine literary content rather than leaving content to the test of the market place.

2. *Copyright is not necessary to encourage literary production by authors.* Here again reference is made to the days before copyright laws, when authors were dependent on patrons and state subsidies and sinecures, as proof that literary creation can be encouraged in other ways. This fact can be admitted as a theoretical point, but the burden of proof is certainly on anyone who claims that our vast contemporary output of books of every description could be encouraged and paid for in some more effective manner than our existing method based on copyright.

3. *The publisher in most cases does not need the protection of copyright to get him to perform his entrepreneurial functions of putting up capital and taking risks.* The argument for this proposition cites as historical evidence an interpretation of nineteenth century U.S. publishing history. American printers and publishers did bring out unauthorized editions of British books—mostly popular fiction—in great quantity prior to our granting British and other foreign authors the right to American copyright in 1891. However, this fact does not support the argument that copyright protection is unnecessary to the publisher, because the historical situation has not been fully stated. Books by American authors were copyrighted in that period and undoubtedly made up the majority of books published. In addition, "trade courtesy" substituted in large measure for legal copyright in the case of the British works which were pirated here. The pirating American publishers for many years recognized the rights of the first of their number to bring out a pirated edition of a particular British work, and ordinarily would not compete with him on that edition. It was only when this trade custom broke down in the 1870's and 1880's that the American pirating publishers—who had been resisting international copyright—joined with American authors and succeeded in persuading the Congress to grant copyright protection to foreign authors.

4. *In one case it is admitted that copyright probably is responsible for bringing certain works into existence—those on which a loss may be anticipated on the original edition but a profit gained from reprints and subsequent rights.* This statement is certainly true but the author probably did not realize what a large proportion of literary works this category would include. For many years now the American production of adult trade books taken as a whole—general books in hard cover in original or substantially revised editions—has resulted in a loss made good by a small profit on the subsequent sale of rights, principally to book clubs and paperbacks.

5. Based on the assumption that copyright is not necessary to the publishing of books—except for the limited number depending on reprint and subsequent rights income—Mr. Hurt's final conclusion is that "*we can say that the traditional assumption that copyrights enhance the general welfare is at*

least subject to attack on theoretical grounds; the subject certainly deserves more investigation and less self-righteous moral defense." My view of this statement is that the factual assumptions about the business of book publishing are so defective that it is impossible to say whether this conclusion has any merit or not. Certainly he has not even made a prima facie case. The subject is an interesting one in both theoretical and practical economics; and should be undertaken again with a great deal more effort spent on developing a sound foundation of facts on which the theoretical treatment can be based.

ARMEN A. ALCHIAN: Mr. Morin's remarks about the function of a textbook come as music to my ears. As a textbook author I shall say no more than second his motion. The other two papers were extremely instructive but embarrassing. Horvitz made me feel like an idiot and Hurt-Schuchman made me feel like an antisocial monopolist. But only for a few moments. On second thoughts I began to have doubts. Let me then take a few minutes to suggest modifications or further problems to think about. These may even suggest some interesting teaching questions for class work.

The royalty induced conflict between author and publisher in which authors should want a lower price than the publisher is a conflict that may be ameliorated by different estimates between author and publisher of the elasticity of demand for the book. It just happens that in my case I believe my estimate of elasticity was much lower than the publishers, so I wanted a higher price than finally adopted. I argued that the book was adopted initially by an intermediary, the instructor, who did not himself buy the book. He would be less sensitive to the price in adopting it than he would if he were to buy it. But my publishers persuaded me to settle for a lower price nearer those charged by the most popular texts. Had I been more careful I would have insisted on the power to set the price, but I doubt I would have ended up with the same publisher, for he remained adamant. I wish I knew who was right. We are indeed price-searchers as well as product-searchers.

The matter of the instructor intermediary poses a difficult problem for the author, not only in pricing, but in writing. The author must write with one eye on the instructor and one on the student. I am convinced, but very possibly incorrectly, that the content and style of a book could be much tougher and more rigorous if the student were the sole authority as to what book to use. Even worse is the practice of adopting a common text by majority vote. Shades of television programming! Fortunately we do not require a common text at all colleges—as is the case in some high schools—so that not every piece of past doctrine, erroneous and correct, has to be included.

But returning to the royalty arrangement, all by attempts to analyze it via the standard cost-and-demand apparatus fell apart. The variety of dimensions open to selection by the author and publisher makes any cost function dependent only on the number of books completely useless. I know of no better example to use to justify the analysis suggested by Demsetz in the *Economic Journal* (Sept., 1964), "The Welfare and Empirical Implications of Monopolistic Competition."

Despite all the conflicts of interest in the royalty agreement and its apparent inefficiencies, it persists. One might think that it would pay the publisher to buy out the author's royalty rights and escape that per-unit tax on each book sold. This is the classic tax inefficiency. But they do not buy out the authors. One explanation could be that they have different estimates of the tax proceeds, the author being more optimistic than the publisher. Another explanation could be that despite this inefficiency, there are other offsetting advantages. A profit-sharing arrangement which would avoid these conflicts about pricing and quality involves opening the books of the publisher to outsiders—to a greater extent than with just sales reports.

Commission payments are a type of royalty payment. They persist in many businesses. The royalty system persists. I am more persuaded by such persistence in open markets where any one else is free to try some other arrangements as prima facie evidence of the overall private efficiency of royalties despite all our incomplete analysis to the contrary. But just more persuaded, not completely convinced.

Incidentally it is not true, as suggested by Horvitz, that authors are less able or willing to bear the risks of large losses. The income sacrificed while writing a book is large.

Some of Mr. Horvitz' survey results surprised me. I do not believe that only two-thirds of the authors thought the publisher advertised enough. Not really. They must have not been thinking of salesman efforts and many other devices the publisher can use to get the book read more thoroughly. The authors may have meant that the publishers advertised as much as they thought it was sensible for the publishers to have advertised, not how much the authors thought it was in their interests. I know of no limit to the amount of advertising that I would find beneficial to me, at someone else's expense.

One feature that Horvitz has written about elsewhere (*A.E.R.*, 1965) deserves some attention here: the ogre of used books—that phenomenon which cuts publishers' sales and royalties in half. Remarkably little seems to be known about used book sales of paperbacks relative to hardbound books. If it is true that paperbacks have a very small used book sales potential, we may have a way of defeating used book encroachment on royalties.

The Schuchman-Hurt analysis should help dispell ideas that seem prevalent that a copyright is something different from a patent. Furthermore, I am sure that most publishers are so used to operating with a copyright monopoly that they will think Hurt's analysis strikes at the foundations of the publishing business. Not at the foundation; just at the present selling methods.

THE ECONOMICS OF BROADCASTING AND ADVERTISING

THE ECONOMICS OF BROADCASTING AND GOVERNMENT POLICY

By R. H. Coase
University of Chicago

The broadcasting industry, with its various methods of finance, its intricate organization and its close, and peculiar, relations with the government offers a rich field for study by the economist. But I will not dwell on those aspects of the industry which would mainly be of interest to students of industrial organization. I wish to consider a more general question. I want to examine the part the economics of broadcasting has played in the formulation of government policy and to consider what conclusions we should draw from this in making our own policy recommendations and in the conduct of our researches.

We must first note that economic factors are taken into account in a world in which ignorance, prejudice, and mental confusion, encouraged rather than dispelled by the political organization, exert a strong influence on policy making. I will illustrate this by a quotation from the Canadian Royal Commission on Broadcasting of 1957 (the Fowler Commission). The Commission was discussing the proposal to finance the broadcasting service by means of an annual license fee for set-owners. This is what they said:

> This certainly seems a logical way for a group of people to make a joint purchase of a service they want. It is not strictly equitable as presumably a flat fee would be charged and all licencees would not make the same use of the service; but it seems much more equitable than the ear-marking of a particular tax paid both by those with radio and television sets and by those who hope never to have one in the house. The yield from the licence fee is reasonably predictable and the support required or desired to be given can easily be adjusted upwards or downwards, by changing the amount of the individual licence fee.

But the Commission concluded:

> The flaw in this approach to the problem is that virtually nobody favours it, and many people feel quite strongly in their opposition. The arguments against it range from the inefficiency and excessive cost of collection, through the likelihood of evasion and difficulty of enforcement, to the simple claim that a licence fee is a nuisance tax and generally unpopular.

Thus we see that a method of finance which was considered "logical," "more equitable" than the alternatives, which could be "easily adjusted" to meet the changing requirements of the service was politically impossible because it would be "generally unpopular."[1] The

[1] See the *Report of the Royal Commission on Broadcasting* (1957), pp. 274-75.

main reason for this unpopularity, heightened in Canada, as always, by describing the hardship a license fee would impose on the old-age pensioner, is an objection to paying for anything. This is, of course, bad economics. It is our duty to point this out, but at the same time we can hardly ignore, in deciding whether or not to advocate particular government policies, the extent to which political considerations will prevent the execution of those policies in a manner which is economically efficient. In Britain the BBC is financed by the proceeds of a license fee, but the BBC has not usually been allowed by the Treasury to receive the total proceeds, while the level of the fee has not always been as high as officials of the BBC desired. If the withholding of part of the license fee and the reluctance to raise it were due to political considerations unconnected with the needs of the service and if most licensees would have been willing to pay a higher fee to secure the additional service that this could have made possible, a reason for the introduction of commercial television is provided which would have been absent if one assumed that the license fee was fixed at an "optimum" level, however that may be defined.

Of course we may hope that, over time, the influence of the economics profession will be such as to make it more difficult to gain political advantage by the propagation of bad economics. But there are other features of the system which I think we must regard as permanent. The first is that the businessmen in the broadcasting industry will try to make as much money as possible. I know that there are some economists who would argue that businessmen merely seek a reasonable return on their capital. But consider the facts. We know from recent figures that the profits of television stations in the first fifty TV markets represented, on an average, 36 percent of gross revenue and that the rate of return on capital for some stations was 200 or 300 percent per annum (after taxes). One wonders what an unreasonable rate of return would be. I personally believe that the only reason the rate is not 500 percent per annum (after taxes) is that the businessmen have not yet discovered how to achieve this. If one reads the trade press about changes in programming, it is apparent that the aim is almost invariably to gain audience (which facilitates the sale of time) and that the adjustments in rates which occur are designed to increase the receipts of the stations. I do not wish to moralize. As has been said, a man is seldom more innocently employed than when he is making money.

So much for the first permanent feature. The second is that we cannot expect a regulatory commission to act in the public interest, particularly if we have regard to its actions over the long period. I am not primarily thinking of the fact that commissions in the United States tend to be responsive to the wishes of Congress or committees of Con-

gress or that, appointed and reappointed by the executive, their views are liable to be in tune with those who have political power. What I have in mind is a feature which, with the best will in the world, it seems to me very difficult to eliminate. However fluid an organization may be in its beginning, it must inevitably adopt certain policies and organizational forms which condition its thinking and limit the range of its policies. Within limits, the regulatory commission may search for what is in the public interest, but it is not likely to find acceptable any solutions which imply fundamental changes in its settled policies. The observation that a regulatory commission tends to be captured by the industry it regulates is I think a reflection of this, rather than, in general, the result of sinister influences. It is difficult to operate closely with an industry without coming to look at its problems in industry terms. The result is that the commission, although thinking of itself as apart from and with different aims from the industry, will nonetheless be incapable of conceiving of or bringing about any radical changes in industry practices or structure. In fact, the regulation of the broadcasting industry by the Federal Communications Commission resembles a professional wrestling match. The grunts and groans resound through the land, but no permanent injury seems to result.

It is of course wholly proper that public discussion of broadcasting policy should have centered on programming, since public policy must be appraised by considering its effects on the programs. But the discussion, particularly in Britain but also in the United States, has taken on a somewhat peculiar character and has tended to confuse rather than clarify the basic issues. Perhaps the best example is to be found in the Pilkington Report of 1962.

The report examines the question of whether it is desirable "to give the public what it wants." It states that at first sight this aim seems to be "unexceptionable" but adds that, "when applied to broadcasting it is difficult to analyse." The reason is that not everyone wants the same things. It is somewhat surprising that the Committee should have thought this peculiar to broadcasting. Had they realized that this was a general problem that every economic system has to solve in dealing with every product or service and that, in most Western countries, it is solved with the aid of a pricing system, I think the Committee would have been led to a more useful discussion of the question. I would not wish to imply that we fully understand the logic of a pricing system or that special institutional arrangements are not necessary for its tolerable performance. But an understanding of the pricing system does lay bare the nature of the problem. As it is, what follows in the report is a discussion of an economic problem without benefit of economics.

I can give you the flavor of the argument by quoting some passages,

the character of which will not be altogether unfamiliar to those of you who know only the American literature:

> No one can say he is giving the public what it wants, unless the public knows the whole range of possibilities which television can offer and, from this range, chooses what it wants to see. For a choice is only free if the field of choice is not unnecessarily restricted. The subject matter of television is to be found in the whole scope and variety of human awareness and experience. If viewers—the public—are thought of as "the mass audience," or "the majority," they will be offered only the average of common experience and awareness; the "ordinary"; the commonplace—for what all know and do is, by definition, commonplace. They will be kept unaware of what lies beyond the average of experience; their field of choice will be limited. In time they may come to like only what they know. But it will always be true that, had they been offered a wider range from which to choose, they might and often would have chosen otherwise, and with greater enjoyment . . . . "[T]o give the public what it wants" is a misleading phrase: misleading because as commonly used it has the appearance of an appeal to democratic principle but the appearance is deceptive. It is in fact patronising and arrogant, in that it claims to know what the public is, but defines it as no more than the mass audience; and in that it claims to know what it wants, but limits its choice to the average of experience. In this sense we reject it utterly. If there is a sense in which it should be used, it is this: what the public wants and what it has the right to get is the freedom to choose from the widest possible range of programme matter. Anything less than that is deprivation. . . .

It is I think apparent that these passages, full of sound and fury, do not give us any criteria by which to decide whether any particular program should be transmitted. It is easy to talk about "the widest possible range of programme matter" but there is surely some point at which, as more and more resources are devoted to increasing the supply of programs, the gain from additional broadcast programs is of less value than the loss in output elsewhere. And if the resources devoted to broadcasting are limited in this way, it follows that the provision of programs which are liked by one group will have deprived some other group of programs that they would have liked. According to what principles is it to be decided which demands are to be satisfied? The Committee never tells us this. But later they tell us how this problem should be solved. I will quote another passage:

> The broadcasting authorities have certainly a duty to keep sensitively aware of the public's tastes and attitudes as they now are and in all their variety; and to care about them. But if they do more than that, this is not to give the public "what someone thinks is good for it." It is to respect the public's right to choose from the widest possible range of subject matter and so to enlarge worthwhile experience.

Up to now we have heard of respect for the public's right to choose and of need for the widest possible choice. It is at this point that the trap closes. They continue:

> Because, in principle, the possible range of subject matter is inexhaustible, all of it can never be presented, nor can the public know what the range is. So, the broadcaster must explore it, and choose from it first. This might be called "giving a lead": but it is not the lead of the autocratic or arrogant. It is the proper exercise of responsibility by public authorities duly constituted as trustees for the public interest.

Thus the committee avoids the question of how it should be decided which programs to transmit and for the phrase "what the public wants," they substitute another and better, "what the public authority

wants." What the public authority should want, how it would get the information which would enable it to do what it should, and how in practice it would be likely to act are questions which all disappear in a cloud of pious platitudes.[2]

In the United States it is improbable that many would seriously suggest that a public authority such as the Federal Communications Commission should be given the power to determine in detail what programs should be broadcast, and while sentiments similar to those found in the Pilkington Report will no doubt continue to be expressed in the United States, there is no likelihood that they will lead to the establishment of a broadcasting system operated by some organ of the government (leaving aside the question of whether this would be held constitutional by the Supreme Court). The broadcasting system in the United States is likely to continue as a decentralized system, operated in the main by private enterprise. What programs will be broadcast will therefore be determined by the economics of the industry. Put shortly, the programs that will be broadcast will be those that it is most profitable to broadcast. I would not wish to argue that all the businessmen consider is money. A television station operator earning 200 percent per annum (after taxes), if he had been grasping and less aware of the finer things, might no doubt have earned 210 percent per annum. I do not doubt that some programs will be broadcast which reduce the profits of the station, but I am quite certain that the broad pattern of programming will be determined by profitability. My view is that we should not bewail the fact that businessmen maximize profits. We should accept it and use it. The task which faces us (and the task of good government policy) is to devise institutional arrangements which will lead the businessman, as it were by an invisible hand, to do what is desirable (by making it profitable for him to do so).

I would emphasize that belief in the invisible hand does not imply that the government has no part to play in the economic system. Quite the contrary. If it is in general true that men, following their own self-interest, act in a way that is of benefit to society, it is, to quote Edwin Cannan, "because human institutions are arranged so as to compel self-interest to work in directions in which it will be beneficient."[3] Our task as economists is to help in the devising and improving of those institutions. In doing this, we should not ignore the noble side of human nature when this can be brought into play. But we should never forget the words of Alfred Marshall to which Robertson has drawn our attention: "progress chiefly depends on the extent to which the *strongest*

[2] These quotations will be found in the *Report of the Committee on Broadcasting*, 1960 (Cmnd. 1753, June, 1962), pp. 16-18.
[3] See the *Econ. Rev.*, July, 1913, p. 333.

and not merely the *highest* forces of human nature can be utilized for the increase of social good."[4]

I think we should ponder these words of Cannan and Marshall when we contemplate the institutional framework within which the broadcasting industry operates in the United States. It is obviously incredibly bad. But how should it be improved? The allocation of the major resource used in the industry, the radio frequency spectrum, is carried out by a method which is inefficient, inequitable, and inflexible. I have explained my grounds for holding this view on other occasions and there is no need for me to spend much time going over them now, particularly as there is really no dispute about the correctness of my position. If I may quote Dr. Goldin when he was with the FCC, but giving, I need hardly say, his personal views, the present procedure for choosing among competing applicants for the radio frequency spectrum is "ritualistic, formalistic, wasteful and inefficient."[5] I have proposed that radio frequencies should be disposed of to the highest bidder because it would avoid the costs of the present procedure, would tend to allocate these frequencies to those who could use them most efficiently, would prevent the unjustifiable enrichment of those (commonly wealthy) private individuals who obtain these grants from the FCC, and would facilitate changes in the use of radio frequencies when this seemed to be called for.

I would not argue that there should be no government regulation of the broadcasting industry. But such regulation is not inconsistent with use of the pricing system. There is no industry which is not in some way regulated. What is extraordinary if we contemplate the allocation of the radio frequency spectrum is that it makes no use at all of the pricing system. Of course there would be difficulties in introducing a pricing scheme. Dr. Goldin has said that "after the initial shock of rationally considering the use of the pricing mechanism in frequency allocations, the virtually unanimous view of communications specialists" would be that these practical difficulties were too great and he adds that until I, or a friendly ally, make a study of how such a system would actually operate, my suggestion will not get into "the mainstream."[6] I think that this is right. The FCC is rather like a whale stranded on the seashore, waiting while the local inhabitants, ignorant of whale anatomy, try to show it the direction in which it should swim. If we are to get sensible government policy in this area, it will, I am afraid, have to come from the work of economists outside the government service (and, for that matter, outside the industry).

[4] See A. C. Pigou, ed., *Memorials of Alfred Marshall*, p. 310.
[5] *Land Econ.*, May, 1965, p. 168.
[6] *Ibid.*

The position is the same if we think of another fundamental question: the finance of the industry. With commercial broadcasting, the person who pays for the broadcast of a program is the advertiser. It follows that the programs broadcast are those which maximize the profits to be derived from advertising. The market for broadcast programs is one from which the consumer is barred: what he would pay plays no part in the determination of programs. The result is that some sectors of the public feel that they are not being catered for. The FCC is uneasily aware that all is not well. And so it has exhorted the businessmen to act in the public interest and, incidentally, against their own. It seems clear that in this case the highest motive was not the strongest.

The obvious way of dealing with this problem is to introduce some form of pay-television. If this were done, consumers who were willing to pay more for resources used in the broadcasting industry than were the advertisers could secure the kind of programs they wanted. This proposal has been strongly opposed by the broadcasting industry. This opposition comes, as Dr. Frank Stanton of CBS has told us, not because the industry has any "economic axe to grind," but because it would not be in the best interests of the public.[7] It is I think a universal rule that businessmen never act from higher motives than when they are engaged in restricting potential competition. Of course, the opposition has been successful. Proposals for pay-television were first made in the late 1940's, but in spite of determined attempts to secure the approval of the Federal Communications Commission, all that has been granted is authorization for experimental pay-television services operating under restrictive conditions which make it impossible for pay-television to realize its potentialities. So far only one such service has been started, that in Hartford, Connecticut. I do not know what the future will be. But there are no signs that the Federal Communications Commission intends to change its policy of support for the commercial broadcasting system.

What should be done? The task of charting a sensible future for the broadcasting industry is not one which can be left to the industry, which has its own interests to protect. It cannot be left to the Federal Communications Commission, which cannot conceive of any future which is not essentially a repetition of the past. Who, therefore, is to perform this task? I suggest that it has to be assumed by academic economists. You may recall what Adam Smith said about university education: "The parts of education which are commonly taught in universities, it may, perhaps, be said are not very well taught. But had it not been for those institutions, they would not have been com-

[7] CBS Statement on Pay-Television by Dr. Frank Stanton, President of Columbia Broadcasting System, May 19, 1955.

monly taught at all; and both the individual and the public would have suffered a good deal from the want of those important parts of education."[8] The position seems to me similar in the present case. I would not argue that academic economists are technically the best qualified to investigate what government policy should be towards the broadcasting industry. But unless they do it, no one else will.

[8] Adam Smith, *Wealth of Nations* (Modern Library edition), p. 721.

THE QUEST FOR QUANTITY AND DIVERSITY IN TELEVISION PROGRAMMING

By David M. Blank
Columbia Broadcasting System

From its birth, commercial television has been subject to a wide variety of demands relating to performance. It has been able to satisfy many of them. But two demands have created what has become a perennial source of tension because they propose somewhat conflicting objectives. One demand has been for "more significant and more diversified" programming. The second has simply been for the availability of more "choice" within the existing framework of program offerings.

Generally, the first demand has been based on the view that an insufficient fraction of television programming has been of high quality or of high purpose, or conversely that too large a fraction has been of mediocre caliber aiming at the lowest common denominator among the public in order to yield the largest size of audience. Put another way, too little television time has been devoted to serious drama, serious music, ballet, art, etc., and too much to light entertainment that simply amuses or entertains—but does not edify or challenge or uplift. These criticisms, for shorthand purposes, have often been summed up as charging a lack of diversity in television programming.

The conventional answer to this criticism has been that the public chooses its programs freely and that broadcasters simply respond to consumer choice; that is, the public gets the kind of programming it desires. But to this answer, three kinds of response have been made.

First, and at the most naïve level, has been the claim that television programming has simply not met the desires of the bulk of the American public; in other words, that the broadcasters have simply misunderstood consumer demands.

A second response is the assertion that while commercial television programming may satisfy a majority of the public, a large minority of the American public is vastly dissatisfied.

A third and more sophisticated level of criticism argues that, while a majority of the public may be satisfied with television fare, they are not really very satisfied. In fact, the peculiarities of this particular market, it has been argued, are such that competition leads not to optimum response to consumer desires but rather to the acceptance by the consumer of a kind of second best. As a consequence, so the argument goes, only a small minority of television viewers are really getting what they most want.

Nearly all of this discussion has been conducted on a theoretical level and, I must in all honesty add, on a largely introspective basis. Theory and introspection have their proper place in economic analysis but at some point they must be tested by the actual reactions of the marketplace. There has always been an enormous quantity of data on consumer reactions in this market in the form of survey (or "ratings") information but some of the critics have not been willing to accept this information as authoritative, for various reasons. I shall refer to some of these data later. However, in the last several years, a substantial volume of actual market experience has accumulated, sufficient volume to shed new and, I think, persuasive light on this whole question. To my knowledge, none of the critics of commercial television programming have referred to these new data. The purpose of this paper is not to engage simply in further debate on the quality or purpose of television programming but rather to bring the evidence of the marketplace to bear on this question. Presumably economists, if not others, will pay heed to it.

The first criticism—that broadcasters have been completely mistaken in their view of what the public wants—is basically the same as one that Tibor Scitovsky made at a session of these meetings in 1961 about the current functioning of free markets. At that time he argued that there is an inherent bias in the workings of the competitive market process against so-called "minority" needs and tastes and that, in fact, the increasing importance of scale economies has resulted in "a tendency of producers in an increasing number of fields to play safe and not to risk imaginative innovations in the new products, services and publications they put on the market. . . ."[1] Indeed, Scitovsky indicated that, at worst, this tendency "can lead to a serious misreading of the public taste and the imposition of a mythical majority taste that in fact few people have."[2] He gave as a prime example of such an occurrence the auto industry's presumed error in traditionally diagnosing the public as having a yearning for larger and more powerful cars, a view discovered to be erroneous only through the miraculous intervention of foreign competition!

In fact, of course, as we know from the experience of the past four years, Professor Scitovsky's view of the public's desires, colored no doubt by his own attitude toward size and power of cars, has proved to be largely erroneous, and any preference by the public for small cars has proved to be mostly a passing fancy—a view advanced by William Baumol at the same 1961 meeting.

There is overwhelming evidence that the view that television com-

[1] Tibor Scitovsky, "On the Principle of Consumers' Sovereignty," *A.E.R.*, May, 1962, p. 266.
[2] *Ibid.*, p. 267.

pletely mistakes its audience is also fallacious. Of all the entertainment and informational media today, television has the broadest appeal to the largest number of people. About 93 percent of the families in the U.S. own television sets. The average television family watches television for five hours or more per day, and has for roughly a decade. About 60 percent of all television sets are in use during peak evening hours. There is not more than a 10 percent variation from the national average when families are classified by income or education of head of household and when one measures the amount of viewing by each such class. Similarly, the distribution of the nighttime television audience, in terms of age and sex, is not substantially different from the distribution of the population residing in television homes.[3]

If these data suggest mass disenchantment with the offerings of the television medium, I hesitate to envisage the amount of viewing that would occur if broadcasters were better able to meet consumer demands!

The second and third criticisms of television programming are based on the view that, because of the peculiarities of the television market, numbers of consumers are seriously dissatisfied with the product offered them. Presumably this dissatisfaction must apply to large numbers of people and must involve substantial amounts of dissatisfaction, for no one expects any industry to satisfy every whim of every consumer. And presumably the burdens imposed by the unique relationship between time and television programming are not part of this complaint. By this I mean that in television, unlike nearly all other entertainment and informative media, a viewer has to be ready to view a program when it is broadcast, not when it might be convenient for the viewer. If one is a viewer who prefers minority fare, he has to be willing to rearrange his life to take advantage of those programs prepared for him, or else he will simply be unable to enjoy programming of the kind he would normally enjoy in other media.

One aspect, as I have indicated, suggests that the television market mechanism leaves largely unsatisfied a major segment (albeit a minority) of viewers. Among those holding this view must be included Professor Scitovsky and some members of the Federal Communications Commission. Also included, at the margin, perhaps should be Peter Wiles who in a recent article in the *Economic Journal*[4] outlined a model of the American television industry in which he assumed that 10 percent of the audience currently watching mass entertainment really prefers programs with more intellectual content. This 10 percent, he felt, is richer and willing to pay much higher prices for its more specialized programming than the rest of the public.

[3] These data are based upon information provided by the A. C. Nielsen Co.
[4] Peter J. D. Wiles, "Pilkington and the Theory of Value," *Econ. J.*, June, 1963.

The other aspect hypothesises that most people are somewhat dissatisfied; that is, they are not normally able to view programs they would prefer but accept typical television programming as simply better than nothing. A vocal supporter of this view is Jerome Rothenberg.[5] Apparently Peter Steiner developed a similar model for radio broadcasting over a decade ago.[6] Rothenberg suggests that the following is "a not implausible characterization" of viewers' preferences: ". . . the first and other high choices of most people represent rather special . . . types of programs. . . . Each group that agrees on its high choices will probably be small, and the high choices of any one such group will probably be ranked rather low by most other groups. The only types of programs which will be ranked in middling positions by most groups will be relatively undifferentiated types: neither especially interesting nor uninteresting; non-controversial; . . . a lowest common denominator. Yet . . . these latter types of programs can outdraw the high choices of any group, and by substantial numbers."[7] To this structure of viewers' preferences, Rothenberg joins the probability of there being only a few television stations in any area and of the fact that the financing of television programs is done by advertisers, who seek mass audiences,[8] rather than by viewers. He concludes that "nothing whatever in the foregoing guarantees . . . [that] the result [will be] programming for majority taste."[9]

Large amounts of evidence on the actual preferences of the public can be derived from the pay-television experiments of recent years. Pay-television, it will be noted, has been developed and promoted on the ground that it can provide the "diversified" fare that audiences have been presumed to be missing in commercial television. As you may know, there have been three major tests of pay-television in the recent past.[10] One was sponsored by International Telemeter, a subsidiary of Paramount Pictures, in a suburb of Toronto. A second is sponsored by RKO-General, based upon equipment developed by Zenith, in Hartford. A third was sponsored in Los Angeles and San Francisco by a new corporation—STV—specifically set up for the purpose

[5] Jerome Rothenberg, "Consumer Sovereignty and the Economics of Television Programming," *Studies in Public Communication,* Autumn, 1962.

[6] Peter O. Steiner, "Program Patterns and Preferences and the Workability of Competition in Radio Broadcasting," *Q.J.E.,* May, 1952.

[7] Rothenberg, *op. cit.,* pp. 48, 49.

[8] Rothenberg leans heavily on the assumption that there is an immutable law of advertising which indicates that "repetition of ad exposure bears disproportionate sales payoffs . . . ." He deduces from this law that advertisers will prefer to concentrate their commercials in as few programs as possible in order to maximize "the size of the *continuing* audience over the series of programs." In fact, there is no such law. And most advertisers in television today are scheduling their commercials in order to achieve as much reach as possible rather than as much repetition.

[9] *Ibid.,* p. 47.

[10] In earlier years, there was a fourth test—in Bartlesville, Oklahoma—that was somewhat more limited in scope than the more recent tests. In even earlier years, there were several experiments of very short duration in Chicago and Palm Springs.

of entering the pay-television business. The first and third of these operations involved distribution of programs to subscribers by cable. The second involved the use of a television station to broadcast pay-television programs over the air to subscribers. The first and third of the systems are not now in operation. The Hartford system is continuing to operate.

The pay-television operations have provided a wide array of program fare for their subscribers. These include newer motion pictures, special entertainment productions (including Broadway and Off-Broadway plays, opera and ballet, concerts and recitals, variety, night club and cabaret acts), educational features, and sporting events not broadcast on commercial television.[11] If there were vast dissatisfaction with commercial television, one would perhaps expect one or more of these pay-television operations to have produced evidence of substantial consumer demand. Or, to put it another way, one might expect sufficient numbers of people to have subscribed and to have paid sufficient sums to make it evident that pay-television was viable. In fact, however, all the available evidence indicates that not one of the systems has been able to combine a sufficiently high proportion of subscribers with a sufficiently high level of per-subscriber weekly payments to make the thought of direct entrance into pay-television very attractive to entrepreneurs. In the Toronto area, when almost half of the families of the wired area were subscribers to the pay-television system, weekly revenues averaged much less than a dollar a week.[12] At this level of saturation, such revenues are, in my view, clearly insufficient to support a viable wired pay-television system.[13]

Several years later, when the weekly minimum was substantially

[11] In Hartford, 599 programs were shown during the first two years of operation. Of these 432 were feature films (including 18 foreign-language films), 79 were live sports events, 35 were specially produced entertainment features, and 53 were educational features. (*Joint Comments of Zenith Radio Corporation and Teco, Inc. in support of Petition for Nation-wide Authorization of Subscription Television,* Before the Federal Communications Commission, Docket 11,279, Mar. 10, 1965, pp. 73-82.) In Los Angeles, from July 17 to September 22, 21 percent of all programs shown were feature films (including foreign-language film), 15 percent were baseball games, 19 percent were specials, 33 percent were educational programs, 6 percent were children's programs, and 6 percent were sports films. In San Francisco, from August 14 to October 20, 29 percent of all programs shown were feature film (including foreign-language film), 10 percent were baseball games, 18 percent were specials, 34 percent were educational programs, 4 percent were children's programs, and 5 percent were sports films. ("Study of Consumer Response to Pay TV," Oxtoby-Smith, Inc., Aug. 9, 1965, pp. 25-26.)

[12] "Study of Consumer Response to Pay TV," p. 20. All of the data on average per-family revenues in Toronto, Los Angeles, and San Francisco were gathered by sample surveys.

[13] The major cost elements in a wired pay-television system are the distribution cable, the drops to individual homes, the program selector units, programming and administration. Broadcast pay-television omits the first two cost elements but substitutes the costs of operating a station. For wired systems, subscriber saturation in a given area is inversely related to per-family costs, because of the importance of the distribution cable in the total cost picture. Per-family costs in a broadcast pay-television system are less seriously affected by variations in saturation.

raised, saturation of the wired area dropped to less than 15 percent and per-family revenues rose to only a little over a dollar per week. This again is insufficient, in my view, to support a wired system.

In Los Angeles, during the period when the pay-television system was in operation, saturation in the wired area averaged a little less than one-third and per-family weekly revenues a little more than a dollar.[14] In San Francisco, during the same period, saturation averaged about 20 percent and per-family weekly revenues, a little more than a dollar. This combination of saturation and revenue augured poorly for the pay-television operation, before the California vote that, perhaps temporarily, outlawed pay-television in that state.

In Hartford, per-family weekly revenues approach two dollars,[15] but only with a minute fraction of the Hartford area subscribing (less than 4 percent). Whether Hartford could hold to this level of per-family revenues as it expanded its saturation is highly questionable, and an operation at this level is so marginal as to be of little competitive consequence in the broadcasting industry and affords little indication of consumer dissatisfaction with free television programming.

At any rate, the evidence from these three enterprises does not support the view that there are substantial profits to be earned by firms catering to the alleged desire by the public for diversified program fare. This is a major reason why the rush of entrepreneurs of several years ago to invest in pay-television systems has now largely vanished.

But, it may be suggested, it is unfair to compare the public's response to free program fare with its response to programs for which it has to pay. Perhaps if both types of programs were made available at equal prices, the public's desire for diversity would become more apparent. Pay-television provides evidence on this score as well. The subscribers to each of the pay-television systems were a selected group, indeed a self-selected group. They include, one would imagine, the bulk of those who felt the need of additional types of program fare not now provided by commercial television. They certainly consist of families above average in income; thus 58 percent of the Hartford subscribers, 62 percent of the Los Angeles subscribers, and 83 percent of San Francisco subscribers had family incomes of $7,000 or more, as against only two-fifths for the U.S. as a whole.[16] These subscribers have been offered, it will be remembered, a vast array of programs, ranging all the way, for example, from "Lolita" to programs entitled, "Meet Your Federal Government" and "Artisans of Florence," all the way from New York Rangers hockey games to "Tchin Tchin" and an

[14] "Study of Consumer Response to Pay TV," p. 20.
[15] *Joint Comments of Zenith and Teco* . . . , pp. 31, 84.
[16] *Joint Comments of Zenith and Teco* . . . , p. 21; "Study of Consumer Response to Pay TV," p. 22.

evening with Joan Sutherland.[17] What did these viewers choose to watch out of this array of programs?[18] In Hartford, the highest-rated program in the first two years of operation was that important cultural event: the Liston-Clay prizefight. No less than 83 percent of all subscribers viewed this stirring program.

The next to the lowest rated program, out of the 599 separate programs offered, you will be interested to learn, was entitled, "You and the Economy," in which several professors of economics from neighboring institutions participated. This program was able to induce literally only one subscriber out of 4,717 to watch its performance![19]

More seriously, the average cumulative rating per feature for motion pictures shown in Hartford was 20; that is, an average of 20 percent of total subscribers watched each film shown. The average rating of athletic events was almost 10. The average rating of specials (plays, opera and ballet, concerts, etc.) was 8.7. The average rating of educational features was less than 1. In Los Angeles the average rating of baseball games was 12;[20] in San Francisco, 13. The average rating of U.S. movies was 11 in Los Angeles; 6 in San Francisco. The average rating of specials in Los Angeles was 3; in San Francisco, 2. The average rating of educational programs was 1 in Los Angeles, and 1 in San Francisco.

In terms of total expenditures by subscribers, 84 percent of all revenues in Hartford were derived from motion pictures.[21] Another 11 percent came from sports. Less than 5 percent came from specials and educational features. In Los Angeles, all films accounted for 39 percent of all revenues and sports for an additional 44 percent. Only 18 percent was received from specials and educational features.[22] In San Francisco, the figures were quite similar.

In other words, at fairly comparable prices for all offerings, 80 to 90 percent or more of all subscribers' expenditures was directed to motion pictures and sports.[23] Surely this is not the picture of an audience yearning for diversified fare! Indeed, if there is an unsatisfied minori-

[17] *Joint Comments of Zenith and Teco . . .*, pp. 73-82.

[18] The prices charged for programs did not vary very much among program types. Thus charges for lectures and educational programs averaged $.71 per program during the second year of the Hartford operation, charges for feature films averaged $1.03, and average charges for the remainder of the program types ranged between $1.37 and $1.62. (*Joint Comments of Zenith and Teco . . .*, p. 19.)

[19] The lowest rated program was entitled, "Presidential Leadership," and had no viewers at all.

[20] "Study of Consumer Response to Pay TV," p. 25. Baseball was the only live sport offered in Los Angeles and San Francisco.

[21] *Joint Comments of Zenith and Teco . . .*, p. 19.

[22] "Consumer Response to Pay TV," p. 25.

[23] In Los Angeles and San Francisco, more than half of all programs offered were specials and educational features; less than half were films and sports. In Hartford about 15 percent were specials and educational features; the remainder, movies and sports.

ty, these data suggest it has been a minority demanding more motion pictures and sports rather than more serious drama and exciting chess games.

The second demand on commercial television to which I referred earlier has been for additional stations or, to put it another way, for additional signals in many communities which were served by fewer than three stations and therefore were unable to receive full three-network service. Without detailing the history of this area of conflict, I can summarize by saying that there have been two separate problems. In some larger markets, where three or more stations could be supported, the FCC allocations did not or could not provide for three or more equally competitive facilities. In smaller markets, three or more stations have never been able to be supported, regardless of FCC allocations.

There is a long history of efforts to resolve both the basic difficulties: UHF assignments, deintermixture, drop-ins, the all-channel receiver, networks' special sales plans for small markets, etc. But a recent and rapidly expanding phenomenon bears witness to the intensity of desire of the American people for more choice within current programming.

If one looks around the television industry for the great new success story of the last half-decade, it is clear that it must be CATV. CATV is a shorthand term for community antenna systems, which are enterprises that bring into a community signals from distant television stations and distribute these signals locally through the use of cable. At the end of 1964, there were about 1,400 CATV systems in the United States, with about 1.4 million subscribers. Subscriptions to community antenna systems have been growing at the rate of 15 percent per year in recent years.[24] Entry into this market has been growing rapidly; more systems started in 1964 than in any prior year and 1965 shows even greater activity.[25]

What does CATV generally provide? Initially it offered commercial television signals in areas where it was difficult to get any service or, later, where only a single service was available.[26] Now we are getting evidence that even where families can get two network signals, they are willing to make initial payments and to incur a continuing monthly charge of $4.50-$5.00 in order to get service from the third network. Thus, a tabulation of CATV systems that have started operating in the

[24] *Economic Analysis of CATV Growth and Impact*, submitted by CBS to the FCC, July 22, 1965, as Exhibit A of Comments filed in Docket 15,971, and reprinted in *Television Digest*, Aug. 2, 1965.

[25] "Reply Comments of Association of Maximum Service Telecasters, Inc.," Docket Nos. 14,895, 15,233, and 15,971, before the Federal Communications Commission, Sept. 17, 1965, Attachment A, p. 5.

[26] Because of distance from transmitters or because of terrain problems.

last nine months shows that at least 15 percent are instances in which a third network service was essentially what was being provided by the CATV.[27]

The second and third networks and the independent stations carried on CATV systems, of course, provide not "diversified" programming in the sense in which this term has been used in this paper but rather further choice within the more restricted entertainment universe of commercial television! Apparently for this kind of choice, which I have here termed an increase in the "quantity" of television signals rather than an increase in "diversity," a sufficient fraction of the public is willing to spend sufficient sums of money to help create a profitable and rapidly growing new industry. Indeed, when a CATV system wishes to upgrade its offerings, it increases the numbers of channels its cable system can offer and brings in not three but a larger number of outside commercial stations.[28] The programming of such stations is largely duplicative except for some variety in motion pictures during the early and late evening and in sports and syndicated fare.

In other words, judging by the experience of the last half-decade, the vote of the public in the marketplace appears to have been in favor of an increased quantity of signals providing similar kinds of fare, not for more "diversified" program fare.

The market tests have not been perfect, and plausible hypotheses might perhaps be invented that would explain away the public's response. But I think that the weight of the evidence is substantial and that our conclusion must be that the public has opted for a substantial preponderance of entertainment programming[29] of the kind now being broadcast. Their desires have been for more such, particularly more movies and sports, and the evidence to show that there are large numbers of people eager for other kinds of programming is difficult to find.

[27] *Television Factbook*, No. 35 and Addenda. Examples of such systems can be found in Temple, Texas; Stevens Point, Wisconsin; Steubenville, Ohio.

[28] Plus an occasional educational station, as well as radio stations.

[29] Plus, I should add, a significant and growing amount of hard news.

SUPPLY AND DEMAND FOR ADVERTISING MESSAGES

By LESTER G. TELSER
University of Chicago

A common economic criticism of advertising is that it waste: resources. The argument runs as follows: the amount of advertisin; supplied is excessive relative to the demand because in most cases ad vertising is provided at a zero price to potential buyers while the cos of advertising is positive to society. Since advertising employs scarc economic resources, one would think that the suppliers of advertisin would prefer to sell it at a positive price if they could. However, ad vertisers may believe that the amount of advertising that would be de manded at a positive price is less than the amount they should provid to maximize their profits. The advertising expense is borne by the con sumers, who pay higher prices for the advertised goods. In additior since most advertising is not supplied at a positive price separatel from the goods and services being advertised, it is concluded that buy ers have more advertising foisted off on them than they would be will ing to purchase in a separate market for advertising services. Thi implies a departure from marginal cost pricing and a consequen waste of resources [2].

I propose to assess the validity of this argument. At the outset thi requires consideration of whether consumers have as limited a range c choice between advertised and unadvertised goods as suggested by thi criticism of advertising.

It is generally true that unadvertised items often command lowe prices than physically similar advertised goods. Hence the continue existence of both types of goods suggests that some of the pric differential is due to the advertising for which consumers are willing t pay as revealed by their foregoing the alternative of finding and pu chasing cheaper, less advertised goods. This defense of advertising a: sumes the existence of physically similar articles of consumption whic differ in advertising intensity and in price. The provision of both ac vertised and unadvertised goods serves to establish implicitly an ap proximation to marginal cost pricing of advertising services. One coul not even infer a departure from marginal cost pricing in marke where all available goods are alike with respect to the advertisir input and price, since this may accord with consumer preferences. I other words, even product uniformity may cater more closely to co sumer tastes if we allow for the additional costs of diversity. Thou

this may seem a weak defense of advertising to some because it assumes per se usefulness of advertising, most would agree that some kinds of advertising serve a useful purpose. I should like to return to this point later.

The argument that advertising wastes resources emphasizes that advertising and the physical commodity are in joint supply and, moreover, that consumers cannot buy the advertising separately. There are two parts to this criticism. The first refers to the total resources that are engaged in advertising under the present system which does not generally sell advertising separately, and the second refers to the satisfaction of consumer needs in a market which does not provide for the separate sales of product components.

The first criticism emerges more clearly in the case of goods or services that are less controversial than advertising. Many goods are joint products the parts of which are seldom sold separately to consumers. It does not follow there is a loss of efficiency because there can be economies of joint supply. For example, there is probably as large a separate demand by consumers for carburetors as there is for catalogues and directories which are actually sold to consumers. From this one cannot infer that there is an excessive supply of carburetors because they come "free" inside automobiles. To put it more forcefully, some might argue that cars have too much horsepower because individual consumers cannot specify how much horsepower they "really" want. However, production economies explain why we buy complete cars instead of the parts which we could assemble ourselves in our back yards. If it were possible to make cars to order at the same cost as large lots, then, of course, manufacturers would have an incentive to do so. The economies of large scale explain why some consumers might have to compromise their preferences because of the taste of the masses. When there is a separate market for components, it is true that minority tastes can be gratified, but this may be so costly that at the prices which suppliers would require, the minority would be unwilling to buy.

Similarly, there are economies that can explain why advertising services are seldom sold separately. Probably one of the more important savings from the joint supply of advertising with goods and services is the reduction of transaction costs thereby made possible. Were advertising sold separately, it would be necessary to allocate resources to the collection of fees from receivers of advertising services [1]. It may well be that the total resources in advertising under the current system is less than would be required under an alternative system of separate pricing of advertising services. For instance, the current system of commercially sponsored television might supply more entertainment at

a given cost than an alternative system which would require the networks to collect fees directly from viewers instead of from sponsors. This can be true despite two shortcomings of the present system. First, at present one may enjoy the entertainment which the advertising supports without necessarily buying the product being advertised. Second, at present it is necessary to estimate audience size so that networks and sponsors can agree on charges. If there were pay-television, then automatically there would be an estimate of audience size. However, even in the case of newspapers and magazines there are complications in estimating readership and the composition of the reading audience notwithstanding the explicit sale of both magazines and newspapers. Generally, readership exceeds paid circulation, but the part of the reading audience pertinent to a given advertiser depends on the nature of the product being advertised and the composition of the audience, which are both hard to estimate [7].

Suppose, to take another example, that advertisers were to mail information about their goods and terms of sale to potential customers and sought to collect charges for this service. No doubt there would be some consumers who would be willing to pay for the information thus provided, but first it would be necessary for the seller to bear the expense of locating such potential buyers. Since the composition and size of potential markets change over time, tastes change, products are modified, etc., a system of direct pricing and sale of advertising messages might prove more costly than the present system, although at present there are many who receive useless advertising messages and who are often offended by the content of the messages.

The role of transactions costs emerges more easily with the help of a diagram. In Figure 1 the quantity of advertising messages is shown on the horizontal axis and the price per advertising message on the vertical axis. The demand for advertising messages by consumers of final products is AC. There are two supply schedules: one which applies if there is a separate market for advertising messages and the other which applies if advertising is supplied jointly with the physical commodity. The first supply schedule is A_1B_1. To simplify the exposition it is assumed to be perfectly elastic. If there is a separate market for advertising messages, then the supply price would include a component A_2A_1 to represent the transactions costs per message in addition to the component OA_2 which represents all other costs per advertising message. Hence the price of advertising messages would be BB_1 and the quantity of advertising messages supplied by the sellers of commodities to consumers would be OB. The total resources engaged in advertising would be OBB_1A_1.

If advertising were sold jointly with the physical commodity, then

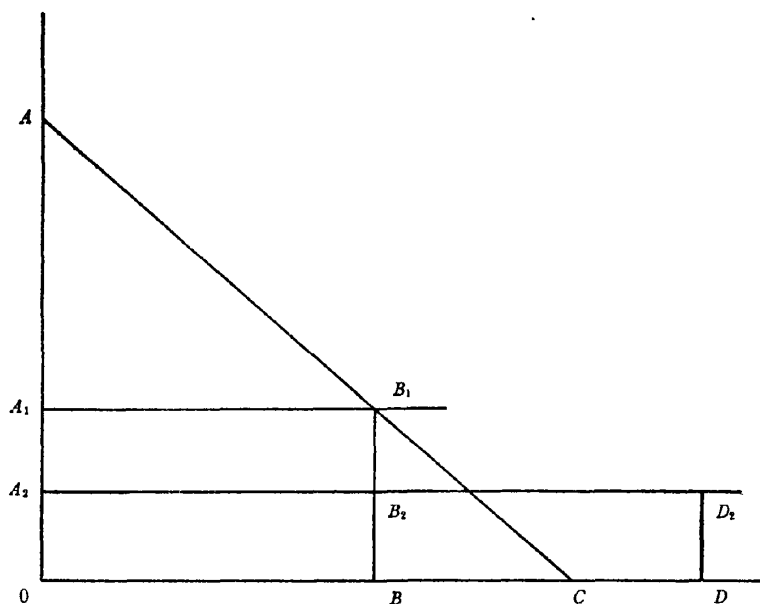



FIGURE 1

the supply of advertising messages would be A_2D_2 since the provisioners of advertising could save the expense of the transactions costs that are required if there is a separate market for advertising messages. Thus in the absence of a separate market for advertising messages the quantity provided by the sellers of commodities would lie in the range CD. The point D has the property that it makes the total resources engaged in providing advertising messages given by the area ODD_2A_2 equal to the area OBB_1A_1. DD_2 is the implicit price which consumers of advertised goods pay for the advertising messages since it is the amount by which the price of advertised goods exceeds the price of physically similar unadvertised goods.

If the provisioners of advertising have the alternative of either selling the advertising messages separately or jointly and if there is competition among sellers, then that method of providing advertising would be chosen which requires the smaller cost. Hence if there is no separate market for advertising messages, then the quantity offered is in the range CD since within this range the total resources engaged in advertising is less than the total that would be engaged were there a separate market for advertising services.

Thus although more advertising messages would be offered at a zero

price than at a positive price, the total resources engaged in the supply of advertising messages would be less than if the advertising messages were sold separately from the physical goods. The novelty of this argument arises from the fact that the usual marginal conditions do not give the correct answer since a global comparison between two alternative methods of organization is needed.

One may hardly doubt there is a demand for some kinds of advertising. As evidence, observe that newspapers without advertising do not survive; yet it is also true that few newspapers contain nothing but advertising, partly because of economies from providing "news" jointly with advertising which enables newspapers to cater to the tastes of a more diverse audience.

A disadvantage of there not being a separate market for advertising is that consumers find it more difficult to avoid advertising they do not want and to receive advertising they do want. Blanket dissemination of advertising messages via the mass media cannot ensure as close catering to desires of individual consumers as would be the case were there a separate market for advertising services.

This point is illustrated in another way by restriction on the content of advertising messages. Advertising messages have perforce a broad appeal which is not tailored to specific desires of individual consumers. Advertising is judged successful either if the consumer purchases the good on a trial basis or if the consumer is inspired to obtain additional information about the product at the point of purchase. However, only some of the disadvantages of not having a separate market for advertising services are overcome by the provision of complementary marketing services at the point of purchase which are designed to satisfy more closely individual needs. Moreover, advertising messages typically extoll the article being advertised but are silent about competing products. Consumers might learn more if advertisers denounced rivals in addition to praising themselves because the conflicting claims might reveal more truth. Yet it is easy to understand why advertisers do not criticize their rivals and also why public policy should not necessarily encourage mutual disparagement. Criticism runs the risk of libel suits and the consequent possibility of large losses from damages. Were public policy to encourage dissemination of critical claims, society would have to invest more resources in the judicial system.

The conclusions thus far reached suggest that the present system of advertising which lacks separate markets for advertising services does not cater as closely to individual consumer information requirements as would the alternative. Hence as a result it is probably true that more advertising is provided. However, the total resources including the cost of collecting fees for the separate sale of advertising messages

might exceed the current investment in the advertising industry. This is an instance of a phenomenon not peculiar to advertising. Clearly, we could all be made better off were it possible to make goods to order at the same cost as the limited variety of mass produced goods. Thus it is that there are some benefits under the present advertising system analogous to the savings consequent under mass production techniques, which offset the disadvantages of the inability to cater to each individual needs.

I have stated there is a consumer demand for some kinds of advertising messages. Development of a theory of demand for advertising messages requires consideration of a broader model of marketing which explains how consumers acquire and maintain knowledge about goods and services.

Conventional demand theory starts from the proposition that there are given consumer tastes, incomes, a set of goods, and prices at which unlimited purchases can be made by individual consumers. This is a useful model for many purposes, but it does not go far toward explaining advertising's role. Tastes depend partly on awareness of available goods and services. There cannot be demand unless buyers know who are the sellers, what they are selling, and the terms of sale. Advertising is merely one of the methods used to create a demand for products. However, neither advertising nor other lawful methods of persuasion can maintain demand for a product which consumers find unsatisfactory or are unwilling to buy at prevailing prices.

Analysis of the advertising process begins by assuming that consumers have initially a stock of knowledge about goods and services of which part comes via advertising messages and the rest through various channels including conversations with friends, experience with products, shopping trips, and discussions with salesmen. Not only does acquisition of knowledge about goods and services take time and trouble, it may also require money outlays. Moreover, the stock of knowledge about products becomes obsolete with the passage of time because terms of sale change, new products appear and old ones disappear or change, tastes alter, and there is forgetting. All these factors underlie the consumer demand for information about goods and services of which one source is advertising messages.

Sociologists stress the importance of conversations among friends in disseminating information about goods and services. Mathematical models of these diffusion processes are inspired by work done on epidemics. In consumption behavior one may postulate three classes of consumers for a given product: those who do not know about the product, those who currently buy it, and those who formerly bought it. The latter class includes both those who came to dislike the product as well as

those who no longer have need for it. Suppose, for example, that former users who dislike the product inform nonusers of this fact when the two meet. Suppose that users like the product and that sometimes upon meeting a nonuser they transmit favorable information about the product to the nonuser. If meetings among members of the three groups occur at random, then, depending on the probability of the relevant information being exchanged on given contacts, there is a time path of the proportion of consumers who buy the product which describes the rise and fall of goods in the market place.

Processes which describe the buying path can display a large variety of shapes, because something can happen at a meeting between representatives of any two out of three classes. In previous work [7] I found that it takes a brand longer to reach a peak than to decline and fall to its trough. Thus the life cycle of brands is asymmetrical.

In addition to exchanges among classes of consumers, one may assume that new potential users enter the market at a given rate and old users disappear at another rate. Taking these two factors into account permits study of the effects of turnover and of changes in market size [3] [5]. These two factors explain the more intensive advertising of new products. During a product's early life there is a large fraction of the potential market ignorant of its existence. Advertising is an efficient avenue of informing potential users. Since actual sales are small and advertising outlays may be large, the ratio of advertising to sales is higher for new than for established products. During later stages, assuming the product gains acceptance, sales are larger and advertising declines. Thus the ratio of advertising to sales is lower for established products.

Essential to the maintenance of the stock of knowledge about goods is the identifiability of sellers and/or the branding of goods. Without these the knowledge gained in each instance could not be effectively used on subsequent occasions. Sellers must keep their identity and the identity of the goods they sell before the public so as to capture the benefit of good will from prior experience. The success of distributed lag models in measuring the effect of advertising on sales is partly due to the fact that advertising adds to the stock of knowledge about products and stimulates sales for a period of time following the transmission of advertising messages [4] [6]. In other words, the half-life of an advertising message may be fairly long. An interesting direction for further research in advertising would be an investigation of the factors underlying the half-life of advertising messages. Some of these are implicit in my discussion of the influences which render obsolete the stock of knowledge.

These considerations suggest that the demand for knowledge about

products differs because of various circumstances. Thus if a commodity remains unchanged over a long period and has a steady clientele, it tends to be less promoted. Hence it is less advertised. Commodities which do not satisfy these stability conditions tend to be more promoted and more advertised. It is clear, for example, that the postwar rise in the ratio of cigarette advertising to sales, which increased from 7.9 percent in 1947 to a high of 13.9 percent in 1959, is largely due to the introduction of many new varieties of cigarettes during this time. Similarly, the high advertising percentage with respect to sales of toiletries is explained by the relatively short life of brands in these product classes and the constant introduction of new brands. Breakfast foods are also intensively advertised, because buyers are subject to the influence of a group with notably fickle tastes—children between the ages of three and twelve—who tend not to eat a decent breakfast.

Although it is readily conceded that sellers of new products find it advantageous to advertise, one may argue that there is too frequent introduction of new products in such commodity classes as toiletries, soaps, drugs, and cigarettes. Obviously, a cosmetics firm, for example, would prefer that its lipstick secure a numerous, affluent, and faithful clientele. It introduces new lipsticks because it finds old ones becoming unprofitable. Usually this results from declining sales due to many factors of which competition from rival lipsticks may not even be of greatest importance. A firm does not lightly bear the expense of constant introduction of new brands. These actions are forced on them by consumer dissatisfaction with available brands. Consumer desire for new varieties drives firms to cater to their demands. The demand for new products contains a derivative demand for the various means of promotion including advertising.

The incidence of promotion between the retail and manufacture level is determined by product characteristics and by the cost to consumers of alternative channels of knowledge about products. An important product characteristic is purchase frequency. Frequently bought items are more likely to be advertised by the manufacturer and less subject to promotion at the retail level. Such goods are more easily judged by consumers because a trial purchase is inexpensive and usage may reveal product characteristics. Less frequently bought items which are relatively expensive are harder to evaluate in advance. Such goods tend to be more intensively promoted at the retail level. The retailers become the focus of consumer attention and to a degree act as agents for consumers in a capacity of expert buyers. Examples are furniture and clothing. These goods are among the least advertised by manufacturers while they are heavily promoted at the retail level by

newspaper advertising and by sales personnel. Not all expensive and infrequently bought articles are largely promoted at the retail level—automobiles are the obvious example. They are purchased more frequently than furniture but less often than clothing; yet fashion plays as important a role in the demand for cars as for clothing. Aside from questions about the relative state of competition in the two industries and its effect on the usage of advertising, it should be noted that as a percentage of sales both clothing and cars are about equally advertised. The larger amount of automobile advertising may reflect the larger demand for advertising messages about the more expensive commodity. For both automobiles and clothing there are publications which specialize in providing detailed information at a positive price to the interested public. Moreover, general magazines and newspapers carry as "news" much information about both of these goods particularly during model changes in Paris and Detroit.

The size of the market and the costs of contacting potential buyers are critical determinants in the choice of promotional techniques. Of equal importance is the nature of the firm's production cost schedule. If the minimum average cost of production occurs at a low output rate relative to market size and there is rising marginal cost, then there is little incentive to advertise in mass media, because the demand schedule generated could not be supplied save at so high a price that there would be no purchasing. Hence a necessary condition for the use of mass media techniques of promotion is the absence of substantial production diseconomies.

For some products it is economic to have salesmen call on customers, while for others it is more advantageous to allow buyers to take the initiative in contacting sellers. Generally, contacts by salesmen are more costly than advertising messages but may prove the cheaper promotional method if it is hard to identify potential buyers or if the product has a small market. Magazines which cater to specialized groups such as hobbyists, professional associations, etc., contain advertising of products whose markets are limited to these groups. This explains why surf boards are not advertised in literary magazines and why *Measurement in Economics* is not advertised in *True Confessions*.

Industrial products are often less advertised than consumer products because there are fewer buyers and because the return from a given sale is high enough so that it pays to use salesmen. Some may explain the difference on the ground that industrial buyers are more "rational" than housewives and, therefore, less susceptible to advertising claims. Be that as it may, there are obvious reasons for not advertising locomotives on television.

Summing up, we see that changes in the stock of knowledge about

goods and services creates a demand for advertising messages that will keep consumers informed about changing conditions in the market. The demand for advertising varies by commodity because of differences in market size which makes one or another promotional technique the more efficient. The turnover of customers, the ease of obtaining direct information about goods, the frequency of purchase, the expense of trial purchases, and the satisfaction with existing goods —all play a role in the supply and demand for advertising messages.

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DISCUSSION

Harold J. Barnett: Dr. Telser has factored the relevant television markets out of his analysis, so that he has the sponsor selling a combination of goods and advertising information directly to consumers for dollars. Given this conception of the economic market and since ultimately consumers must pay the cost of advertising, Dr. Telser analyzes whether the advertising should be sold to the consumer separately from the goods. I suggest that Dr. Telser's simplification—deletion of the TV markets—is a serious error. Cancelling out the TV markets in an analysis of TV advertising demand and supply is like presenting Hamlet without cast, script, or theater—like asking an audience not to bother coming to the show but to send their checks to the backers directly.

For example, Dr. Telser thinks the TV advertising message is a desirable (that is, utility yielding) economic good to the viewer, which he would pay for if it were not available free. Contrariwise, I think TV advertising messages are negative economic goods to the viewer, which he must "consume" in order to obtain the entertainment program. Dr. Telser seems to conceive that the TV advertising message is simply for the benefit of the viewer, conveyed to him as in an information market, like consumer research reports. I conceive of the TV advertising message as only partly informational—as a combination of information, persuasion, enticement, fantasy, misinformation, color, duress, entertainment, monotony, distraction, irritation, and other elements, with great quality variation in each of these many components, presented by the prospective seller of soap, cereal, or cars solely for his benefit (profit). Dr. Telser views the TV advertising message as jointly supplied with the product, as (in his example) a carburetor is jointly supplied with the remainder of the automobile. As I see it and have written previously, the customary joint supply in TV is, rather, entertainment program and advertising message, which are jointly exchanged in a barter market for viewer time; in a separate dollar market the advertiser then pays the station for the viewer time, roughly in proportion to its volume and with relation to the number and duration of advertising messages.

Dr. Blank examines viewing habits in the three experiments with pay-TV: California, Toronto, and Hartford. In Hartford, the only surviving case, 4 percent of the populace subscribes to pay-TV; their viewing yields about two dollars per week per subscriber; and they view films and athletic events more than ballet, opera, drama, and concerts. Since present advertiser-commercial TV also presents more films and athletic events than ballet, opera, etc., Dr. Blank seems to conclude that advertiser-commercial TV is giving the public what it wants; that there is little indication of consumer dissatisfaction with advertiser-commercial TV programming; that there are not large dissatisfied minorities; and that the American public would not support pay-TV.

I do not think that these conclusions can be logically derived from his evidence. For example, Dr. Blank's evidence is very thin. The categories "films"

and "athletic events" are catch-alls. Minorities totaling in the millions are not trivial, etc. Only a much more modest conclusion can be supported. Dr. Blank's evidence of pay-TV viewing tends to refute emotional criticism that networks, stations, and advertisers deliberately and stupidly do not give the mass of people programs which they will watch en masse. He could also have used as evidence of mass-audience preferences comparisons of attendance or revenue figures for ball games and movies, on the one hand, with ballet, opera, and concerts, on the other. A merit of Dr. Blank's paper is that, interpreted as I suggest, it helps clear away grandiose, economically illiterate charges, so that more serious questions and problems of the present TV situation may be discussed.

Professor Coase and Dr. Blank have opened the discussion of these questions, and I shall now try usefully to extend the discourse.

The American television industry and economic markets are on the edge of possibly revolutionary change due to ripening technological and institutional impulses. It behooves us to be aware.

1. *UHF*. Several years ago the FCC maneuvered through Congress the all-channel bill, which requires that the tuners of all new TV sets be able to pick up signals of 70 UHF channels as well as the 12 VHF channels. This overcomes one major bottleneck to expansion in the number of programs available in each locality. But other major obstacles still stand in the way of such increase in TV services. As compared with existing VHF stations, the new UHF stations will tend to face higher investment costs for tower and transmitter, smaller population coverage and ratio of audience to coverage, and lesser access to program material and advertiser dollars. Nevertheless, there is a significant likelihood that gradually UHF will amplify service to the public. But this will depend on whether monopolistic restrictions and practices are prevented, particularly in supply of program material, and on development of additional networks or quasi-networks.

2. *CATV*. Today, in conventional VHF-TV, signals are moved over the country by multiple-channel microwave and land lines from the network origination point. The last stage of signal to viewer, however, is a local, single-channel, airway broadcast by a station; since the number of stations has been limited by availability of channels in the heavily populated areas and by high fixed cost per airway broadcast generally, the number of programs available to a community has been limited.

CATV overcomes the difficulty of local broadcast limitations by tying its subscribers onto a local, multiple-channel land line (usually carried on telephone poles, by arrangement with the phone company), so that anything which the community antenna can pick up is available to subscribers. Thus, once subscribers are connected to the local coaxial line, CATV can greatly multiply the number of programs available to a locality by picking up distant programs at distant points and relaying them by microwave, with appropriate amplification, to the local community antenna. The only additional cost of adding all the programs available from a major metropolis is the microwave relay expense. For example, there is no technical obstacle to prevent a commu-

nity antenna company from picking up local New York or Chicago or Los Angeles programs and relaying them to its subscribers in, say, Pennsylvania, Georgia, Missouri, or Oregon. But there are institutional and legal obstacles. The innovation is very complex in its economic, political, and legal characteristics and implications. The various self-interested industry and governmental groups are bringing great political power and influence to bear on the CATV phenomenon.

3. *Communication Satellite.* The prospect is that within a relatively few years communications satellites could provide multiple-channel service to communities the nation over. Original broadcasts could be bounced off the satellites directly to homes or to local TV suppliers (CATV, subscription TV, and local broadcast companies). The cost of service to the national advertiser and network would be markedly less than the present network system of distribution. The availability of programs in each locality could be much larger than at present.

4. *Non-mass Audiences.* The obstacle to our present very limited number of wide-coverage stations undertaking during part of each day or evening to serve non-mass or minority audiences is not necessarily that such service would be unprofitable. Rather, the obstacle to their doing so is that revenue from serving such minority tastes would be less than revenues foregone from serving the mass—stations would earn considerably smaller profits than at present.

I have just described the local availability of additional wire and broadcast channels for pay-TV or advertiser sponsors, via UHF, CATV, and satellites. The availability of such additional channels is very large. Some of the incremental services, as they entered, would aim for the mass audiences which the present stations now serve, thereby reducing the reviewers per station and the rates of profit in serving such mass audiences. Some of the incremental services, however, would attempt to serve minority tastes if the prospective revenues therefrom suitably exceeded the costs. That is, with very many more channels available, prospective new suppliers (who have no present TV mass-audience profits to forego) would serve non-mass audiences if this were profitable, as is normally the case in highly competitive markets.

There are two general possibilities for suppliers to visualize expanding the probability of prospective profit in serving minority audiences. One is to enlarge the audience by making his program region-wide or nationwide, through use of satellite and/or CATV.

The other possibility is to decrease the out-of-pocket cost of originating minority-taste programs. This would occur if local telephone companies in their common carrier, public utility role would wire up cities with coaxial (multiple-channel) lines to homes, with fixed monthly fees paid by the homes, as is true with phone wires. Then the marginal cost of services by multiple TV suppliers, broadcasting simultaneously, would be merely the costs to each of his program material—tapes, movies, or live photography—and playback equipment or cameras. Such suppliers would not each have to make a half-million dollar investment in a tower and transmitter to broadcast his single set of pro-

grams. Marginal cost so reduced might be suitably below marginal revenue from subscribers or advertisers—as is the case for FM stations specializing in classical music, for example—and thus minority tastes might be met under economically viable conditions.

In closing, I offer three brief comments concerning public policy. Of course scarce VHF channels should be auctioned off for reasons of economic equity and efficiency. But also—and more important, in my view—channels should be auctioned off to prevent the possible public immorality and political and governmental degrading which can occur when limited numbers of immensely valuable public properties are given away free to powerful and influential applicants. Second, while Professor Coase focuses his criticism on FCC, I find the federal communications statutes and congressional behavior (committees and individuals) to be greatly responsible for unsatisfactory government behavior concerning this industry, perhaps even more the performance deficiencies of the FCC. Finally, I suspect that some minority-taste audience groups may never be economical to serve, except by fee or by subsidy from foundations or governmental funds. The need for such financing should be honestly faced. As Professor Coase points out, it is rather too much to expect profit-seeking broadcasters not to seek profits or, I might add, nonprofit broadcasters to suffer unrecoverable deficits.

HYMAN H. GOLDIN: Dr. Blank's theme is that the preferences of the mass audience run to movies, sports, quiz shows, medicos, spies, situation comedy, and the rest of the "quantity" which makes up commercial TV programming. Conversely, only a fractional minority tunes to good drama, music, or discussion. This is evidenced by the audience ratings, by the growth of CATV which requires consumer payments for greater access to TV quantity, and by the pattern of consumer choices in the experimental subscription TV tests. These data, Dr. Blank suggests, are proof that the public chooses its television fare, not out of quiet desperation as some critics contend, but from an affirmative preference for television quantity.

On the basis of simple numbers I quite agree, but that has never been the name of the game. It is at the margin that the policy issues are centered. And by the "margin," I mean both the size and leadership significance of the minority audience and the minority of time that even the mass audience will accept quality program fare.

You will note that Dr. Blank has constructed his opposition models with some deliberateness. His second model, for example, is a proponent of the view that "while commercial television programming may satisfy a majority of the public, a *very large* minority of the American public is *vastly* dissatisfied." (Emphasis supplied.) His third straw model argues that "only a *very small* minority of television viewers are *really* getting what they *most* want." (Emphasis supplied.)

I propose more modest target models: The probability of a minority audience—in the approximate range of 8-10 percent of total families—who are actively, although not vastly, dissatisfied with the current insufficiency of

"quality" programming. Characteristically, this minority includes the better educated, the recipients of higher income, the more active citizens, the teachers, writers, executives—including television executives—and perhaps even economists. A second model is the existence of a larger segment, perhaps a quartile of the public, which from time to time can be attracted to quality programming, if sufficient resources are committed both to the programming and to the promotion. I believe that using substantially the same trade sources as Dr. Blank I can validate the objective reality of these models.

Nevertheless, even if I were successful I suspect that Dr. Blank and I would disagree on the policy consequences. I would contend that a disproportionate share of resources should go into instructional, educational, and quality entertainment programming to serve these models. By "disproportionate" I mean greater than would be warranted by short-term market demand, which is Dr. Blank's measuring rod. If we shift the basis of measurement from percentage or ratings to absolute numbers, television is still the largest concert hall, theater, opera house, and public forum ever devised. As such I cannot accept Dr. Blank's view of TV as merely a purveyor of TV quantity.

I believe that my approach is particularly defensible in light of the changing technology of the industry. In the future we shall not be bound as rigidly as now by a constricted, limited-channel broadcast system. Within the next decade we could have a 10-channel satellite-to-home broadcast system, and/or a 20-channel wired home system, in addition to low-cost home video recorders. As a matter of national policy we could then reshape our broadcast institutions to serve more effectively both mass and limited audiences and tastes.

As with a rising national income, we shall have the capability of spreading the benefits of an expanding communications service and thus partially resolve the present tensions of conflicting objectives.

Perhaps immodestly I recognize the personal insult in Professor Coase's statement: "The FCC is rather like a whale stranded on the seashore, waiting while the local inhabitants, ignorant of whale anatomy, try to show it the direction in which it should swim." May I return Dr. Coase's compliment by suggesting that I detect the voice and hands of Captain Ahab.

Despite the invidious comparisons Professor Coase draws between academic and nonacademic economists, he and I are not wholly in disagreement. We both favor greater use of the pricing mechanism in the broadcast structure and less reliance on the regulatory apparatus. But whereas he conceives of the frequency spectrum as a resource in the private sector that should be fully subject to market considerations, I tend to view the spectrum as more akin to public lands. Nevertheless, although our diagnoses differ substantially, our specific prescriptions have elements in common.

I agree with Professor Coase that a broadcaster should pay for the use of the spectrum, and that competitive bidding is a preferable alternative to the present, lawyer-subsidy hearing system. I also agree on the desirability of introducing some form of pay-television. On this latter point, I am reasonably optimistic that pay-television will be approved—I hesitate to say eventually—by the FCC. I am even hopeful that the broadcast industry may not oppose

pay-TV on broadcast channels as vigorously as it once did. As indicated in Dr. Blank's paper, the industry is now more skeptical of the economic viability of open-channel pay-TV. May I suggest to Dr. Coase, also, that CATV at least in part is a form of pay-TV in that it enables consumers to pay for three full network services and for a wide choice of movies and sports—the present television quantity.

However, whether television is supported by advertising or by a fee system, I believe that for the substantial majority of consumers the program choices will be basically similar. Like Dr. Coase, I would give the public the right to choose which system they prefer and I would extract as fully as possible the economic rent now enjoyed by the purveyors of television quantity. Unlike Dr. Coase, I would make such specific provision, outside of the market mechanism if necessary, for the educational, cultural, instructional, and experimental usage of television as is necessary for other public or quasi-public institutions —museums, libraries, parks, and symphony and opera organization.

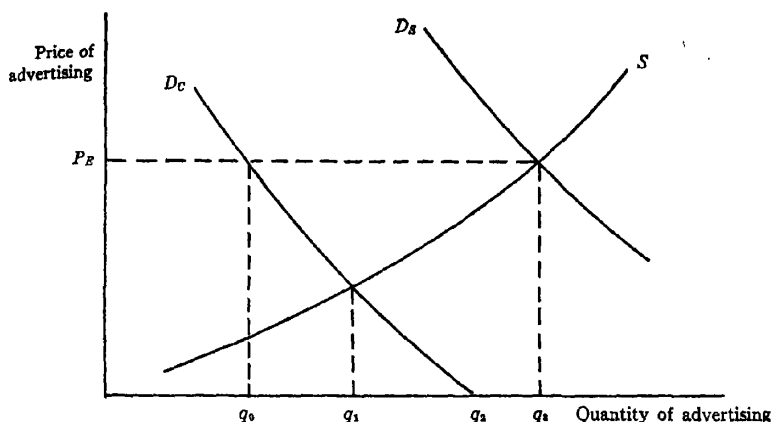
In summary, the changing technology in radio opens a range of choices broader than the either-or of Dr. Blank or of Dr. Coase. I invite the economists of the world—academic and nonacademic—to unite on that platform.

PETER O. STEINER: Both Professor Telser and Dr. Blank are engaged upon debunking missions in the mythology of consumer sovereignty. Telser questions the widely held view that consumers pay for more advertising than they want and are thereby unwilling accomplices in a waste of resources. Blank questions the criticism that broadcasters fail to give the viewers what they want. Each appeals to factual evidence, of a sort, drawn from observable market behavior.

As a true believer in confronting theory with evidence, I should like to take a brief look at some aspects of these exercises in empirical revealed preference. Because my time is limited I refrain from cataloguing the little quaintnesses in each paper—such as Telser's notion that the nonsurvival of newspapers without advertising is evidence of positive utility of advertising to readers or Blank's view that the New York Rangers are a major league hockey team—and look instead at the core of their remarks.

Let us accept Telser's postulate that the efficient way to provide advertising messages is as a joint product and that there is a single supply curve that relates the quantity of advertising to the price of advertising messages. This price may be viewed either as the price paid to advertising media by sellers of goods or as the advertising component of the price paid by consumers of goods for the advertised product. There are, however, two separate demand curves, D_c, consumers demand, and D_s, sellers demand. In the graph I draw the D_c curve to the left of D_s for the sake of argument.

One of Telser's arguments is that because there are economies of joint production there are really two supply curves as well. I accept this in principle, but will neglect it in order to focus on my major criticism. The supply curve I have drawn is assumed appropriate to both D_c and D_s.

Given these curves, Telser and I agree that q_1 represents the quantity of

advertising consumers would buy and pay for. This is however not the actual quantity provided. Nor as Telser believes is the quantity produced q_2—the amount of advertising people would buy at a zero price. Instead it is the quantity q_3, because it is the advertisers demand curve (D_s) that reflects their view of "the amount of advertising they should provide to maximize their profits." In my view the "excess advertising" issue concerns the difference q_1 q_3 not the irrelevant q_1q_2.[1] At the quantity q_3, the sellers can pass on to buyers of advertised products only the amount P_E q_0, and must absorb the rest. My criticism then rests upon his neglect of the difference between D_c and D_s.

Telser might argue that the case I have sketched is wrong on several bases:

1. He might argue that $D_c = D_s$ by necessity: that the demand of sellers is a purely derived demand. He does not so argue, for the good reason that except in a world of perfect competition it is not a valid prediction of theory. We are all aware of why sellers in noncompetitive markets might choose to advertise in order either to attempt to increase their share of a market or to raise barriers to entry of new firms. They may, in aggregate, spend more on advertising than they can pass on to consumers.

2. He might argue that the evidence shows this equality to be the case. He does not do this. Instead all his evidence is designed to show that D_c is not zero, a point I readily concede, but that does not do more than destroy a silly defense of the wastage argument.

3. He might argue that, since D_c is not zero, it might be less than, equal to, or greater than D_s. This is logically possible. I think it is at variance with both good theorizing and the evidence he presents. Theoretically it is unappealing because one can find no plausible reason for sellers to provide less advertising than customers want and are prepared to pay for, but can find

[1] It is irrelevant because, if D_c is not zero, advertisers can pass on part of the cost of advertising to customers.

lots of plausible reasons why they should be motivated to provide more advertising. These reasons are those mentioned earlier concerning one firm's share of a total market. By way of evidence: (1) The vast bulk of the advertising is done by industries—soap, cereals, cigarettes, automobiles, etc.—that are clearly oligopolistic. (2) The persistence of advertising on established brands—as in cigarettes before the cancer scare and the shift to filter cigarettes jumbled this up. (It is necessary for Telser's argument, not that advertising be higher on new brands than old ones, but that it falls toward zero on established brands.) 3. The fact that, contrary to Telser's prediction, an industry characterized by low plant-scale economies and multi-plant firms may engage in massive advertising—again cigarettes are the classic example. 4. The fact that rapid brand obsolescence (in toiletries, etc.) is a response to the needs of advertising, and not the other way round. (There is ample evidence that "New" and "Modern" are demonstrated crowd pleasers.)

In short, while there is much of interest in Telser's paper, I feel he has not assessed the validity of the argument he poses.

I find it helpful to think of Blank's paper as addressing three questions.

1. Is there a dominant preference for entertainment as distinct from edification? I agree with his conclusion and his evidence that the answer is yes.

2. Is here a viable unsatisfied minority audience that is not being adequately supplied? He says no. I have doubts about the adequacy of his evidence.

One criterion of unconstrained optimal programming would be that any program should be provided if potential revenue exceeded cost. My first point is that regional tests of pay-TV provide a poor test because they divide the revenues without dividing the production costs. (The exception, significantly, is the movie produced for initial theater showing, where the only relevant costs are marginal.) Suppose for the sake of argument that 10 percent of TV households would be prepared to spend one dollar each to see a program that cost $1 million to produce and distribute. This is roughly $5 million of potential revenue. But very little of it is in Hartford or Toronto or even California. The market mechanism may now be deficient in not providing a mechanism whereby CBS can collect this $5 million, but that does not demonstrate that the viable national audience does not exist.[2] I take it that the serious form of the charge of "unsatisfied minorities" is not that the existing networks fail to cater to those minorities that it is profitable to serve but that the present institutional set-up does not provide the mechanism that makes it profitable for someone to cater them.

Blank's own data suggest there is an elastic demand for pay-TV. If this is so, cost per program per viewer is of critical importance, and a regional market raises cost per viewer. Is it not possible that the success story of CATV is in major part the very low cost per program? My point here is merely that small regional pay-TV experiments show nothing decisive about the nonexis-

[2] If you are wondering why CBS does not provide the program anyway, it is that, perhaps, it can only sell these viewers to a sponsor at fifteen cents each. If it profit maximizes, it will seek a cheaper program that attracts the same size audience, or a possibly less intensely desired one that attracts more viewers.

tence of unsatisfied viable minority tastes. Further, there are some fragments of evidence the other way; to name just one, some classrooms of the air attracted significant numbers of viewers even at barbaric hours.

3. The third and most subtle of Blank's questions is whether TV provides adequate diversity within its selected sphere. Blank argues that the evidence shows there is a desire for more quantity but not for more diversity. In part this issue is semantic, but there is substance to semantics. He is not using the word "diversity" the way I do, but the implications of assigning the word "quantity" are to remove any criticism of existing programs and place it all on the availability of channels, etc.

Obviously no one needs more quantity if it is identical with the existing fare. If people want more programs in order to get more variety of choice, it may reflect either a cross-sectional dispersion of tastes of the population that is inherently unsatisfiable given limited channels or it may reflect dissatisfaction with the variety of fare that existing program producers provide. Obviously three networks can only provide three programs in any half hour, but they can provide eighteen in any three hours and ninety in fifteen hours. Do people want an extra signal because they want four choices in any half hour or because they want more choice in the prime time of any week? Blank's data which show that people will undertake direct costs to widen their alternatives are consistent with either. The evidence—empirical as well as theoretical—that there is a frightful sameness to the programming fare is ample. It is true that if we could get enough channels, the choice would increase, but everyone knows there are constraints, and much of the question concerns getting a reasonably efficient use of facilities given those constraints.

Finally, let me note that I am not suggesting nor do I think it correct to suggest that existing networks are failing to do what they are set up to do.[8] If the structural and institutional arrangements lead to a socially nonoptimal situation, the fault is with who ever structures the situation. The FCC for over thirty years has an all but unblemished record of misconceiving both the problems and the available remedies in broadcasting. That they have done so with the best of intentions and (except for a few sordid interludes) with high standards of integrity, is a real source of scandal.

[8] Indeed, as I have argued elsewhere, networks probably provide higher quality programming than pure profit maximizing would dictate.

THE ECONOMICS OF SCIENCE POLICY

NATIONAL SCIENCE POLICY: ISSUES AND PROBLEMS*

By EDWIN MANSFIELD
University of Pennsylvania

I. *Introduction*

In the past few years, there has been a significant increase in the amount of attention devoted to national science policy.[1] Congressional committees, scientific panels, and other groups have spent a considerable amount of time trying to determine the proper role of the federal government in the support and management of the nation's scientific enterprise, the strengthening of its basic science, and the evaluation and selection of substantive scientific programs. Several factors account for the increased concern. First, because of higher costs of scientific research and considerable growth in the amount of research carried out, federal R and D expenditures are now large enough to be politically visible. Second, the rapid expansion in the federal science budget seems to be coming to an end, and problems of choice are harder to avoid than in the past. Third, there is growing recognition of the importance of research and development in the process of economic growth.[2]

My assignment is to describe very briefly some of the major issues and problems in national science policy, the purpose being to present an introduction to the subject for economists, many of whom are still relatively unfamiliar with this area. The paper begins with a brief

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[1] For some recent literature on this subject, see Gilpin and Wright [10], Price [43] [44], OECD [38], National Academy of Sciences [29] [30] [31], Freeman, Poignant, and Svennilson [9], Dupree [8], House Committee on Government Operations [50], House Select Committee on Government Research [51], House Committee on Science and Astronautics [52], NIH Study Committee [37], Senate Select Committee on Small Business [53], and Senate Committee on Government Operations [54].

[2] For some interesting discussions of the meaning of "science policy" and the causes of the increased interest in the subject, see Gilpin and Wright [10], Study Number X of the House Select Committee on Government Research [51], Smith [46], and Weinberg [58]. We are primarily interested in Section V in the "policy for science" rather than the "science for policy" aspect of science policy. See *Business Week*, Dec. 4, 1965, for an example of current expectations regarding the 1970 and 1975 level of government R and D spending. According to these estimates, there will be about a 3 percent annual increase in such expenditures during the next decade, as contrasted with about a 13 percent annual increase during 1960-65.

summary of the difficulties that arise when a society's allocation of resources to the production of knowledge is left to competitive markets. Then, after discussing various issues concerning nondefense technology, it takes up the management and organization of military research and development. Finally, it discusses some issues concerning basic research and higher education, as well as the organization of scientific policy making in the federal government.

II. *Research and Development in a Competitive Economy*

To begin with, three elementary, but important, points should be noted regarding the allocation of resources to the production of knowledge in a perfectly competitive economy. First, unless some R and D is supported by the government or some other organization not motivated by profit, less R and D is likely to be generated than is socially desirable, the deficiency being particularly acute in the case of basic research. There are considerable discrepancies between the private and social benefits of R and D, the results of research often being of little direct value to the sponsoring firm but of great value to other firms. Moreover, if firms are risk averters, the investment in risky activities like R and D is likely to fall short of the social optimum.[3] Second, the incentive to keep research findings secret produces results that are economically inefficient in a static sense. Since knowledge, once produced, has a dissemination cost of essentially zero, it is socially optimal for it to be disseminated free of charge. However, if it is disseminated free of charge, there is no reward for the knowledge producer, even though the social benefits of the new knowledge are more than sufficient to cover his costs. Thus, the incentive for firms to create new knowledge is reduced, and there may be a conflict between static and dynamic efficiency. The patent system is an attempt to deal with this situation.[4] Third, although a free enterprise system may not result in an optimal allocation of resources to R and D, some aspects of the competitive process are still desirable. In particular, if the uncertainties involved in a project are very great initially and if they diminish substantially as a project proceeds, the optimal strategy may be to run in parallel several "competitive" projects designed to serve the same end. Although this may seem to involve needless and wasteful duplication, it is often the cheapest way to proceed.[5]

[3] For elaboration of these points, see Nelson [32] and Arrow [3]. Note that this argument assumes perfect competition and no support of R and D by private foundations and governments. If these assumptions do not hold, there is no assurance that the conclusion does either. See Sections III and V.

[4] See Leontief [20], Usher [55], Nelson [32], Arrow [3], and Mansfield [25].

[5] See Nelson [34] and Hitch [13]. Note, however, that important difficulties are encountered when one attempts to generalize the analysis to include more than one review point. See Marschak [27].

III. *Federal Support of Nondefense Technology*

To carry out existing programs, the federal government spends about $15 billions per year on research and development.[6] Excluding the Defense Department, over 80 percent of the expenditures are made by NASA, AEC, and NIH, each of which attempts to provide strong support for a particular area of technology: space exploration, atomic energy, and health. These areas tend to lie close to the public sector, atomic energy and space exploration having military and cold-war significance and requiring sums that would not possibly be forthcoming from the private sector. Besides these huge programs, there are smaller ones that support R and D in agricultural techniques, weather forecasting, coal utilization, desalinization methods, and many other areas.[7] In addition, there have been proposals to aid civilian industrial technology which, according to some, is hampered by an underinvestment by some industries in R and D.[8] In 1963, the Department of Commerce submitted a civilian industrial technology program to Congress, but met with little success. Industrial groups opposed the bill because they feared that government sponsorship of industrial R and D could upset existing competitive relationships. In 1965, one element of the program, the State Technical Services Act, was passed by Congress. It authorized for industry a program somewhat analogous to the agricultural extension service, a major purpose of the program being to increase the rate of diffusion of innovations.[9]

There are many important and difficult issues regarding the proper role of the federal government in supporting nondefense technology. For example, concern has been expressed that too much of our engineering and scientific talent has been absorbed by the defense and space programs, to the detriment of other high-priority applied fields as well as basic research and teaching.[10] Within the nondefense public sector, it is often alleged that such fields as mass transport, public housing, education, and various urban services have received too little R and D support.[11] Within the private sector, it is often claimed that

[6] According to *Federal Funds for Science, XII* (NSF, 1964), total federal expenditures for R and D and R and D Plant were $15 billion in 1964. See [26], Table 3.3. According to [50], these expenditures were estimated to be $16 billion in fiscal 1965.

[7] See *Federal Funds for Science, XII* (NSF, 1964).

[8] For example, see Freeman, Poignant, and Svennilson [9], p. 42, and Council of Economic Advisers [6], p. 105.

[9] See U.S. Department of Commerce, *The Civilian Industrial Technology Program*, Washington, 1963, Allison [2], and *Science*, Sept. 24, 1965, and Mansfield [23].

[10] For example, Arthur Ross thinks that too much of our scientific talent is being allocated to defense and space. See his "How Do We Use Our Engineers and Scientists?" in *Labor and the National Economy*, ed. by W. Bowen (Norton, 1965). For other discussions of manpower "shortages" and problems in this area, see Alchian, Arrow, and Capron [1] and the National Academy of Sciences [31]. For a study of the effects of disarmament, see Nelson [36].

[11] For example, according to Nelson [33], "aside from the fields of defense and space,

industries composed of many small firms have invested far too little in R and D, that the areas between "basic research" and "product development" have been neglected, and that too little has been spent on large "systems" development projects because of the narrowness of the technical interests of particular firms.[12] Also, there are questions regarding the patent system. Although no serious attempt is being made to abolish the system or to change it radically, there is considerable interest in such technical issues as whether or not it would be desirable to adopt a delayed examination system, like that instituted recently by the Dutch. Interest in changes of this sort helped lead to the creation this year of a Presidential Commission on the Patent System.[13]

Without pretending to answer these questions, I think that the following points should be noted. First, the arguments presented in Section II do not tell us whether current government R and D expenditures in particular areas are too large or too small. Although they suggest that the government or some other organization not motivated by profit should support some R and D, the optimal amount may be smaller or larger than that currently supported by the government. Since we cannot estimate the social returns from additional R and D of various sorts at all accurately, it seems sensible to view the relevant policy problems in the context of the theory of sequential decision making under uncertainty. To the extent possible, programs designed to change the amount of R and D in particular parts of the public or private sectors, if initiated at all, should be begun on a small scale and organized so as to provide data regarding the returns from a larger program. Of course, this approach does not eliminate the need for better measures of the returns from R and D. On the contrary, without such measures, the sequential approach will be of little use because there will be no reliable way of measuring the returns from small-scale programs. Until better measures are developed, the crude measures that are currently available will have to be used instead.[14]

Second, an alternative way in which the federal government might try to encourage innovation in civilian markets is through its procurement policies, one possible device being performance-based federal procurement. Its proponents claim that, by formulating performance criteria rather than physical specifications for the items it purchases, the federal government will free industry to innovate, limited only by

peacetime atomic energy, and perhaps public health, it is likely that we are relying too much on private incentives as stimulated by the market to generate R and D relevant to the public sector. . . ." In connection with urban transportation, note that the High Speed Ground Transportation Act of 1965 authorized $64 million for research on high-speed ground transportation.

[12] For example, see some of the opinions expressed in Allison [2], as well as Nelson [33].

[13] See the *Journal of the Patent Office Society*, May, 1965.

[14] See Nelson [35] and Mansfield [23]. For a discussion of the measurement of returns from R and D, see Griliches [12] and Mansfield [22].

the requirement that certain specified functions be performed, encourage cost reduction for the government itself, and serve as a pilot customer for technical innovations where it represents either a big enough market or one sufficiently free from local restrictions, codes, etc., to make innovating worth industry's while. Also, state and local governments may be stimulated to apply new technologies by demonstrations of their successful use in federal programs.[15]

Third, turning to the patent system, delayed examination has some important advantages. Under a delayed examination system, an application is first examined only as to formal matters. After a prior art search, the application together with the results of the search is published as a provisional patent. A further examination is made as to novelty and invention only if certain fees are paid by an interested party within a certain period of time; and if full examination is not requested within this period, the patent will lapse. Since the invention is published more rapidly than under the present system, the technical information embodied in the patent will be disseminated more quickly. This is socially desirable, though not always beneficial to the inventor. Moreover, since a provisional patent should be enough for defensive purposes, the delayed examination system should relieve the Patent Office of the burden of giving these applications a full examination, thus helping to reduce the long delay in issuing patents.[16]

IV. *Research and Development in the Department of Defense*

Consider next the Department of Defense, which accounts for over half of the federal R and D budget. To understand many of the issues regarding the organization and management of military R and D, one must recognize that the relationships between the federal government and the weapons producers differ considerably from those in an ordinary market. In many cases, the seller's products—aircraft, missiles, etc.—are sold almost entirely to the government and have no civilian markets, much of the seller's capital is provided by the government, and the government has agents involved in the managerial and operating structure of the seller's organization. Because of the great uncertainties involved in military R and D and the impossibility of competitive bidding for R and D contracts, the mechanism of the free market has been replaced largely by administrative procedure and negotiation. Moreover, the need to maintain an industrial mobilization base, to-

[15] See Mansfield [26] and the references cited there, as well as National Bureau of Standards, *Improving the National Climate for Innovation* (Washington, 1965). Of course, performance-based procurement would entail a great many extra costs, as well as possible advantages.

[16] See Kaysen [16] and Mansfield [25].

gether with political pressures, makes it likely that contracts will be awarded without strict attention to past performance.[17]

Economists have devoted considerable attention to at least four issues in this area. First, there has been a debate over military development strategy, Cherington [4] and others defending the so-called "systems approach" to development, Klein [18], Kaysen [16] and others attacking it. The systems approach, which in the past has been central to the way the services conducted the procurement of new weapons, is characterized by great emphasis on integrating initially the design of various components and on planning from the beginning the various steps that must be carried out and the sequencing of these steps which will minimize delay. Its critics argue that this approach is inefficient because, although something like the scheduled completion dates may be met, costs tend to be very high and performance characteristics tend to be sacrificed. They believe that requirements for systems should be stated initially in broad terms, that flexibility should be maintained, that parallel approaches should be taken to difficult problems, that components should be tested as soon as possible, and that the integration of the system should be postponed until the major uncertainties have been reduced substantially. There is evidence that the DOD and the services underestimated the relevant uncertainties during the 1950's. Recently they seem to have recognized more fully the dangers involved in proceeding with the full system design before the initial uncertainties are reduced.[18]

Second, there has been a debate over the organization of military R and D. Some have argued that, to a greater extent, research and development should be divorced from production and performed in nonprofit research institutes and government laboratories, which in their view are better suited than business firms to carry out this task. If development can be separated from production, it would be possible to reap the benefits from competition at the development stage and from a freer choice of suppliers and contract instruments at the production stage. A fundamental consideration in judging this proposal is the cost involved in separating development from production. It is sometimes argued that these costs are high, because there is considerable overlap and similarity between these two functions and because learning is transferred between them. We need much better extimates of these costs.[19]

[17] See Cherington [5], Dupre [7], Kaysen [16], Moore [28], and Peck and Scherer [40].

[18] Also, see Klein [19], Dupre [7], Glennan [11], and Peck and Scherer [40]. Note that both Klein and Kaysen favor more R and D that is outside the major weapons-systems programs.

[19] See Kaysen [16] and Cherington [4]. Some empirical studies that I carried out at RAND suggest that in the cases considered these costs may not be very great, but the results pertain to only a few types of weapons and are extremely tentative.

Third, there has been considerable discussion concerning the types of contractual arrangements used in military R and D. Until a few years ago, these contracts were cost plus fixed fee, this kind of contract being defended on the ground that the "product" was so unpredictable that the risks to the seller would require a very high fixed price if it were feasible at all. Recognizing that there is little incentive to reduce costs in CPFF contracts, the DOD has begun to switch to incentive contracts in the past few years. Since they reward cost reduction by giving the firm a certain percentage of the difference between its actual costs and the negotiated target costs, incentive contracts are likely to lead to greater efficiency than CPFF contracts, if the target costs are the same. However, many observers challenge the assumption that the target costs are the same, pointing out that the contractor, with considerable advantages in the negotiation of target costs, has more incentive to increase these costs under incentive contracts.[20]

Fourth, there has been considerable controversy over the extent of the "spillover" from military and space R and D to civilian technology, this topic being of considerable importance because of its implications regarding the extent to which civilian technology has been drained of scientists and engineers by the defense and space programs. Although it is very difficult to measure, there seems to be a widespread feeling that the spillover per dollar of R and D is unlikely to be as great as in the past, because the capabilities that are being developed and the environment that is being probed are less intimately connected with civilian activities than formerly was the case. Consequently, there has been some discussion of ways in which spillover can be increased, and NASA in particular has established a number of programs to try to match up its inventions with civilian industrial uses. The extent to which these programs have been successful is by no means clear.[21] Of course, this spillover question is related in various ways to the controversy over

[20] Note four things. First, the use of CPFF contracts tended to encourage the stockpiling of engineers and scientists by contractors. Second, the Secretary of Defense has claimed that at least ten cents is saved for each dollar shifted from CPFF to incentive contracts. (See DOD Cost Reduction Program—Second Annual Progress Report.) Third, at least one observer claims that incentive contracts may increase the likelihood of contracts being given to relatively inefficient firms. Fourth, for further discussion, see Scherer [45], Williamson [59], Mansfield [24], and C. Greer, "CPIF: The New Look in R and D Management," *Ind. Mgt. Rev.*, 1964.

[21] Programs have been started by NASA at the Midwest Research Institute, the University of Indiana, the University of Maryland, and elsewhere. The Midwest Research Institute receives information on technical developments that have been used in the space program, evaluates them in terms of industrial applicability and then holds industrial briefings to communicate the results to industry. The Aerospace Research Applications Center at the University of Indiana stores all technical reports of the Space Agency in a computer facility, and provides this computerized data to the thirty industrial firms participating in its program. Members of the Center's staff learn what a participant's technical needs are by having him provide a "profile of technical interests," then attempting to retrieve pertinent information. The University of Maryland sends a description of promising innovations to a relatively large number of companies in the relevant field. See Allison [2], Mansfield [23], Solo [47], and Welles, *et al.* [57]. The DOD has been less concerned with promoting spillover.

the treatment of patent rights to inventions made under government procurement contracts.[22]

V. *Basic Research and Higher Education*

Consider next the important area of basic research, where the National Science Foundation plays a key role, although its expenditures on basic research are far less than the major mission-oriented agencies. It is generally agreed that the government should support basic research, but there is considerable uncertainty in Congress and elsewhere regarding the optimal amount of this support and its allocation among scientific fields. These are perhaps the most difficult science-policy problems. Formally, the government should push its expenditures to the point where the marginal social benefits from various kinds of basic research equal the marginal social benefits from the relevant resources in alternative uses. However, this rule, which is hard enough to apply in the case of applied research and development, is practically useless in the case of basic research, because its benefits are so difficult to predict. Faced with this enormously complex problem, the House Committee on Science and Astronautics recently asked the advice of a distinguished panel of the National Academy of Sciences. As one would expect, the panel's recommendations generally are based on criteria, like scientific merit, that are no more quantifiable than marginal social benefits. Where they are quantifiable, they usually amount to a plea for the maintenance of the *status quo*—or a simple upward extrapolation of expenditures for basic research.[23]

A related set of issues is concerned with the effects of federal R and D expenditures on higher education. These expenditures, as well as other policies of federal agencies, clearly influence the allocation of scientific and engineering effort between teaching, on the one hand, and applied research and development, on the other.[24] Machlup [21] and others have pointed out that applied programs, like NASA's, compete with teaching for scarce scientific and engineering talent, and that increases in these applied programs can be dangerous if, by curtailing the supply of teachers, they reduce excessively the rate of increase of the supply of scientists and engineers. Studies have been made of the

[22] The controversy over the treatment of these patent rights has received considerable attention. What little evidence we have suggests that this issue may not be as important as some, like Senator Long, would have us believe, but it is not entirely negligible. See Leontief [20], Preston [42], and Watson, Bright, and Burns [56].

[23] See National Academy of Sciences [29], particularly the papers by Brooks, Johnson Kaysen, Kistiakowsky, and Weinberg. This volume, which reflects the thinking of some of the country's most distinguished scientists, contains a great deal of important and useful material. However, the assignment is close to an impossible one. For other relevant studies, see the Wooldridge report [37] and the recent National Academy study of chemistry.

[24] For simplicity, I lump basic research with teaching. Of course, there is a strong complementary between basic research and teaching, the importance of which is pointed out by Machlup [21] and the President's Science Advisory Committee [41].

distribution of scientists and engineers between teaching and other work, and simple models have been used to derive "optimal" allocation rules [49]. Unfortunately, as their authors are aware, these studies suffer from the fact that the available data completely overlook the crucial differences in quality among scientists and engineers, and that the models oversimplify the relationships between teaching and R and D.[25] Also, there has been a great deal of criticism leveled at the effects of federal research grants and contracts on the quality of undergraduate education. For example, the Reuss Subcommittee [50] claims that federal research programs "have harmed scientific higher education by excessively diverting scientific manpower from teaching, and by overemphasizing research to the detriment of teaching. . . ." (It claims, too, that an important imbalance has developed between the natural sciences, on the one hand, and the social sciences and humanities, on the other.[26]) Because it is so difficult to measure the quality of undergraduate education, it is difficult to know how seriously to take these criticisms. Although the Subcommittee seems to think that the adverse effects of government research are borne out by the published testimony of university professors and administrators, a close examination of this testimony shows that a great many of the respondents do not agree with this conclusion. The Subcommittee report seems to oversimplify the situation.[27]

Still another set of policy issues that has attracted considerable attention concerns the distribution of federal R and D spending. There are substantial disparities among regions and among universities in the volume of these expenditures, the differences being due largely to the fact that scientific strength is concentrated in certain regions and universities.[28] During the past few years, there has been growing pres-

[25] For one thing, the quality of teaching depends on the extent and quality of previous research.

[26] See the Reuss Subcommittee [50] and Machlup [21]. The National Foundation on the Arts and the Humanities was established recently to help remedy this imbalance. See *Science*, Oct. 1, 1965.

[27] Judging from the June, 1965, report, *Responses from the Academic and Other Interested Communities to an Inquiry by the Research and Technical Programs Subcommittee of the Committee on Government Operations*, the number of respondents that felt that federal research programs have not had a deleterious effect was about equal to the number that felt that they have. For some other discussions of the effects of government research grants and contracts on higher education, see the National Academy of Sciences [30], Orlans [39], and Kidd [17]. Of course, one important consideration in this area has been fear of federal control of education. At this point, we should also note that there has been considerable discussion of the adequacy of the allowances for the indirect costs arising from government research grants to universities. Many observers claim that the imposition of a flat percentage limitation on the payment of indirect costs has been inequitable and has resulted in a drain on university funds. See *Government and Science*, Study No. 5 of the House Committee on Science and Astronautics [52], as well as the corresponding hearings before the Committee. Finally, problems in government-university relationships have sometimes arisen because of loyalty requirements, security measures, and classified research on campus.

[28] See *Government and Science*, Study No. 4 of the House Committee on Science and Astronautics [52], as well as the hearings before the Daddario Subcommittee; i.e., the

sure in Congress and elsewhere for an equalization of these expenditures;[29] and in September, 1965, the President issued a major policy directive asking federal agencies to be more responsive to the have-nots in the competition for federal funds.[30] With regard to federal expenditures on academic research, how should we go about this process of equalization? For a number of reasons, it would seem unwise to allocate research funds on a quota system based on regional student enrollment (or something similar), or to spread most of the available funds over a large number of minor universities, or to move from a system emphasizing project grants to one emphasizing institutional development grants.[31] Contrary to the expectations of some, it simply is impossible to establish fifty or a hundred universities that are first-rate in most major scientific fields in the short run, and there is a danger that in trying to do so, we may seriously weaken the relatively few that presently are excellent in most such fields. The others should be encouraged to attempt to excel in only a few areas and to broaden their scope gradually.[32]

VI. *A Department of Science*

Finally, I turn to an issue regarding the organization of scientific policy making in the federal government. In recent years, there have been proposals of a cabinet-level Department of Science, which would include the AEC, NASA, NSF, the basic research activities of the DOD, the National Bureau of Standards, the Office of Technical Services, the Patent Office, and part of the Smithsonian Institution. Advocates of a Department of Science, led by then Senator Humphrey, advanced the following arguments in its favor. First, the secretary of such a department, because of his cabinet rank, would assure greater

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Subcommittee on Science, Research, and Development of the Committee on Science and Astronautics, U.S. House of Representatives, 82nd Cong., 2nd sess.

[29] In Congress, the Daddario Subcommittee, the Elliot Committee, and the Reuss Subcommittee have argued for a more even distribution of funds. See the House Committee on Science and Astronautics [52], the House Select Committee on Government Research [51], and the House Committee on Government Operations [50]. Previously, the President's Science Advisory Committee and the National Academy of Sciences recommended a policy of increasing the number of academic centers of excellence. See [41] and [30]. It has become customary for people interested in science policy to distinguish between "big science," where research is very expensive because of high equipment costs, and "little science," where support is given individual investigators and does not involve large capital costs. The political attention focused on the location of big science facilities is evidenced by the tremendous competition among states and cities for the proposed 200 BEV Accelerator.

[30] See *Science*, Sept. 24, 1965, pp. 1483-85.

[31] See Hornig [14]; the hearings before the Subcommittee on Science, Research, and Development of the Committee on Science and Astronautics, U.S. House of Representatives, 82nd Cong., 2nd sess.; this Subcommittee's report, *Higher Education in the United States*, 1965; and Leland Haworth's statement in the *Fourteenth Annual Report* of the NSF, 1965. Also, see C. Kerr's *The Uses of the University* (Harvard, 1963).

[32] See Wolfe [60] and Stigler [48], as well as the references cited in note 31. Greater interinstitutional cooperation and sharing of facilities and faculties might also be a useful development.

status for science. Second, the department would help to eliminate useless duplication and to promote a better allocation of scientific manpower. Third, it would constitute a policy link between Congress and the President.[33] The proposal has been opposed by the scientific community, for what seem to be good reasons. The 1958 Parliament of Science (and numerous scientists who testified) opposed the proposal because they feared further centralization and because it seemed administratively and politically unwise. Since the agencies that would be merged have great status and importance, the department might well be torn by intramural dissension. Moreover, the merger of agencies with practical missions with NSF might well result in the neglect of basic research programs. Finally, since the new department could not hope to include all government scientific activities (because Defense, Agriculture, etc., would have to maintain research establishments), the new department would have to compete with existing departments and could not act as a coordinator of all scientific activities.[34]

VII. *Conclusion*

In this paper, my assignment was to provide a general introduction to some of the major issues in national science policy. In attempting to fulfill this task, I have touched on a wide variety of questions concerning the allocation of R and D resources within the public sector, the support of civilian industrial technology, government procurement policies, the patent system, military development strategy and organization, the spillover from military and space R and D to civilian technology, the size, allocation, and effects of federal support of basic research, the geographical and institutional distribution of R and D expenditures, and the organization of federal science policy making.

These questions vary considerably with regard to the contribution that economics presently can make to their solution. With regard to many of the major issues concerning basic research, economics has very little to say. As one moves toward the development end of the R and D spectrum, economics becomes more useful, but it still has only a limited contribution to make. In another paper,[35] I have indicated the specific kinds of economic research that I believe are particularly needed. Broadly speaking, work is needed to develop better measures of the rate of technical change, to develop better models to characterize technical change, to obtain more meaningful breakdowns of R and D, to investigate the sources of invention,[36] to study the motivation

[33] For example, see Hubert Humphrey, "The Need for a Department of Science," *Annals of the Amer. Acad. of Polit. and Soc. Sci.*, 1960. Also, see Senate Committee on Government Operations [54].

[34] See Price [44].

[35] See Mansfield [23].

[36] A promising study in this area is Project Hindsight, a DOD study that attempts to measure the sources and value of research results in the military sphere. DOD scientists

and characteristics of inventors, to determine the extent of the economies of scale in various types of R and D, to incorporate R and D expenditures and related inputs into the production function, to extend the work on sequential decision making in R and D, to estimate the costliness of separating military development from production, to study the effects of sharing rules on target costs, and to determine the effect of various factors on the diffusion of information. Hopefully, this paper will help stimulate economists to turn their attention to these areas.

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and engineers identify important innovations in each of a number of weapons systems, and attempt to determine who was responsible for the research underlying the innovations. Using the resulting data as a measure of research output, a comparison is made of the productivity of various types of laboratories, and an attempt is made to identify the managerial, organizational, and environmental factors associated with high productivity. This study is just beginning. For a few of the preliminary results obtained to date, see Chalmers Sherwin's testimony before the Subcommittee on Science, Research and Development of the House Committee on Science and Astronautics, July 1, 1965.

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SCIENCE POLICY AND NATIONAL DEFENSE

By ADAM YARMOLINSKY
Department of Defense

It is a privilege for a lawyer gone wrong to be invited to appear before a group of economists, particularly since I recall the advice of my economics tutor who told me there were only two proper disciplines for someone who intended to make a career in government—one was economics and the other was law and, having tutored me in economics, he suggested that I choose the law.

The best evidence of concern with the economics of science policy in the Department of Defense is that while there are seven assistant secretaries of defense dealing with such complex and difficult areas as installations and logistics, systems analysis, and international security affairs, the director of defense research and engineering has the rank and pay of an undersecretary, one notch higher on the totem pole than his nonscientific colleagues. Similarly, his principal deputy is one step higher on the executive pay scale than his opposite numbers in other areas of responsibility, and the third layer in his organization has a comparable one-step advantage. There is also a more flexible professional pay scale for scientific personnel under a special Act of Congress: Public Law 313. And I note that the Office of the Director of Defense Research and Engineering enjoys some 180 square feet of office space per person in the Pentagon while other offices have 156 square feet or less.

Any economist will recognize these distinctions as in the nature of a policy for dealing with a scarce resource. Standing by themselves, they represent a simple approach to the problem, but at least a clear one. Beyond this point policy becomes more difficult to discern and, indeed, to develop.

In fact, it could be argued that, apart from salary differentials, the Department of Defense (which spends 55 percent of the government's research and development dollar) has, at least until very recently, been treating the supply of scientific resources as essentially unlimited.

Defense Department procurement policies permitted and even encouraged the stockpiling by contractors of scientific and technical manpower and the extensive use of that manpower for activities that have well earned the nickname of brochuremanship. Contractors too often expended their best scientific and engineering talent on packaging proposals, and talented people too often spun off their own new enterprises, finding it more profitable to spend their time being salesmen rather than

scientists. The sum total of productive scientific effort was thus reduced as a result of unnecessarily duplicative entrepreneurship.

On the in-house side, technical ability was too often wasted. In at least one instance, scientists employed in in-house laboratories of the Department had their minds taken off their complaints that the most challenging research tasks were assigned away from them, by the distraction of having to do the janitor work in their own laboratories—and their efforts to communicate with their colleagues in the outer world were frustrated by the fact that the local base commander would not deliver their mail unless they informed their correspondents in advance of the internal code numbers required by the base post office.

At the same time the Department was engaged in a largely theological argument with defense industry about the nature of certain nonprofit entities, created with substantial initial injections of public funds (not unlike their profit-making counterparts), and whether these entities were uniquely qualified to do certain kinds of research and development work, and disqualified from doing other kinds.

I do not mean to suggest by using the imperfect tense in the preceding paragraphs that all is now perfection, or that more than a fair beginning has been made in resolving these paradoxes in our official behavior.

But we have made a beginning. We have changed the ways we buy research and development services outside the Department and the ways we decide what we need to buy. We have broadened the range of outside organizations from which we can buy these services. And we are improving the quality of our own people on whom we depend for advice about what to buy and how to buy it.

An awareness of a scarcity of scientific and engineering manpower resources has been a major factor in revamping contract procedures for the procurement of new weapons systems. The program definition phase has been instituted, during which the rival claims of competing contractors can be tested by more rigorous means than an examination of their sales presentations. During this phase, competing contractors pursue alternative paths towards defining the development effort to be undertaken in the next phase and identifying more precisely the design specifications for the end product. Finally, they submit a bid containing cost estimates for development and procurement. The cost-plus incentive fee contract has been adapted for use even in exploratory contracts where assembly line production is a long way off. The weight given to a contractor's previous experience has been substantially increased in contract selection procedures, in part in order to discourage brochuremanship.

These changes and others like them have required a better under-

standing of what the Department is actually doing by way of research and development, and where along the spectrum from pure research through exploratory research, advanced development, engineering development, and test and evaluation, the dollars are being spent. This detailed breakdown is a further application of the functional budgeting and program packaging procedures introduced in the Department in 1961, and has been essential to the articulation of every aspect of science policy. The analysis that emerges emphasizes that by far the heaviest expenditures are incurred towards the development end of the research and development spectrum. In fact, out of the almost $16 billion budgeted by the federal government for research and development in fiscal 1966, almost $10 billion goes for development items, and the ratio is substantially the same for expenditures by the Department of Defense alone.

The detailed breakdown also points up the geometric increase in cost of any given program as it moves away from the drawing board into the shop. This analysis has in fact encouraged the development of devices like the program definition phase which consciously postpones final commitment to the procurement of a specific item, in order to minimize the cost of later stages. It is a good deal less expensive to explore blind alleys in the laboratory than in the field, and the program definition phase allows the Department to maintain its options in the choice of weapons at least until those uncertainties that can be resolved have been resolved. In this way we also maintain competition for as long as possible throughout the initial phases of a project.

Another major advance was embodied in the 1962 Report to the President on Government Contracting for Research and Development, familiarly known as the Bell Report, after its principal author, your colleague, David Bell. The Bell Report took off from the fact that more than 80 percent of federally financed research and development "is conducted today through nonfederal institutions rather than through direct federal operations," and urged the need to continue to rely heavily on the entire range of private institutions, universities, nonprofit entities and profit-making industrial corporations available to perform this work. It argued that the character of the private institution, whether profit or nonprofit, should not affect the choice of a particular contractor to perform a particular task. And it identified the troublesome conflict of interest problems that arise when a research and development contractor also undertakes production responsibilities, so that he may be preparing specifications for a proposal on which he will later want to bid for production. These problems, it concluded, were not solved by the expedient of creating new nonprofit entities, but rather required a new set of rules for profit and nonprofit organizations

alike to insure objectivity and fairness in particular situations where conflicts might otherwise arise.

Perhaps paradoxically, while these new rules have increased the area within which nonprofits and profits can compete, they have also reduced the government's need to rely on some of the large nonprofit entities like the Aero-Space and Mitre Corporations which it found it necessary to create in the last decade.

The Bell Report also recommended substantial improvement in the work environment for scientists and technicians within the government; and the Department of Defense has been actively concerned over the last three years with making this recommendation a reality. Apart from the salary increases embodied in the 1964 Federal Pay Act, we have shortened the chain of command from the top of the military department down to the individual laboratory, we have given laboratories more direct access to the resources they need, and we have increased the provision for discretionary research funds under the direct control of the laboratory director. The effects of the serious erosion of competence in our in-house laboratories which we faced in the 1950's have not yet been completely overcome. But the trend has been reversed.

It is not the expectation of the Defense Department planners that the in-house laboratories will occupy more than a minority role in doing the actual work of research and development for the Department. It is rather the view that they will provide continuing special competence in particularly military specialties.

It is also felt that contracting for research and development cannot be intelligently administered unless the administrators understand what they are contracting for. And this understanding in turn requires experience at the laboratory bench as well as at the office desk. To some extent this experience can be acquired during periods of service outside the government. But the opportunity needs to be available within the government as well.

All of the activities of the Department that I have been cataloguing might perhaps be lumped together under the general heading of consumer education, for the chief consumer of scarce scientific resources. Attracting and retaining people who know what we need, giving them the widest possible range of sources to choose from, and the most effective tools to make the choice, are all part of the effort to improve the quality of the selection process.

Nor is this effort limited to the procurement of research and development of weapons of war. The Defense Department buys blankets as well as bullets, and groceries as well as guns, and it builds housing developments as well as hangars. In all of these areas it is so large a

consumer that the opportunities for substantial economies by applying large-scale research and development are very considerable indeed, and, as a result of several new initiatives recently directed by the Secretary of Defense, they are just beginning to get attention.

The question remains whether what is good for the Department of Defense is also good for General Motors? And the question is particularly pertinent when the *Wall Street Journal* reports, as of last week, that the private share in U.S. research and development spending, though still less than 40 percent, has increased slightly over the last year. I submit that at least two major lessons of the economics of defense science policy apply to the economics of national science policy. Our scientific resources are now so great that we can modify our environment substantially in any way we choose—even destroy it completely. The range of modification is limited only by our own imagination. The first lesson, therefore, is that we cannot afford to leave the imagining to the scientists alone. We have to do it ourselves. Just as defense planners have allowed their imagination to range from weapons systems to family housing units, so nondefense planners must allow their imagination to range from nuclear power plants to better educational systems.

Second, we are accustomed to asking scientists whether they can perform particular tasks for us—to build an intercontinental ballistic missile or to land on the moon. Increasingly, the question now is not whether, but how much will it cost, and what else can we buy for the same amount of money. As soon as that question is asked, it seems to me economists are entitled to review the bidding. You are the experts on relative value. You may have to acquire a certain amount of technical, scientific and engineering knowledge in order to do the job properly, but it will be well worth it—not only for your own professional satisfaction, but for the advice that you can then give to the politicians.

SOME ASPECTS OF THE ALLOCATION OF SCIENTIFIC EFFORT BETWEEN TEACHING AND RESEARCH

By MICHAEL D. INTRILIGATOR, *University of California, Los Angeles* *and* BRUCE L. R. SMITH, *RAND Corporation*

I. *Introduction*

The allocation of scientific effort has recently become a subject of concern to both policy-makers and economists. For the policy-maker the substantial public expenditures on "big science" have led to difficult choices in the allocation of scientific effort.[1] For the economist it has become clear that there is an important relationship between scientific and technological progress and economic growth.[2] The result for the scientific community is that it can no longer be regarded as an autonomous enclave, insulated from the larger social and economic context, but must operate within a general framework of public accountability.

The allocation of resources to science presents the policy-maker and economist with a multitude of problems of choice, including the allocation of public funds among various fields of scientific activity and various projects and the choice of which institutions to support, in what ways, and with what regard for various patterns of interaction among different institutions. Of central importance, also, are questions of the allocation of trained scientists among different scientific careers and various projects and institutions.

This paper analyzes one aspect of the problem of scientific choice: the allocation of new scientists between teaching and research careers. In particular we are concerned with the "feedback" of science doctorates into higher education.[3] There may be a danger of either seriously weakening the educational process or "starving" nonacademic scientific endeavor by an improper allocation between scientific careers. The aim of this study is to provide a conceptual framework for analyzing this problem: by considering some simplified objectives of a science policy, developing a model for the allocation of new scientists, identifying preferred allocations in the light of the objectives and the

[1] Derek J. DeSolla Price, *Little Science, Big Science* (Columbia Univ. Press, 1963).

[2] Edward Denison, *The Sources of Economic Growth and the Alternatives Before Us* (Committee for Economic Development, 1962).

[3] F. Machlup, "Can There Be Too Much Research?" *Science*, Nov. 28, 1958, pp. 1320-25. V. Stoikov, "The Allocation of Scientific Effort: Some Important Aspects," *Q.J.E.*, May, 1964, pp. 307-23. Richard H. Bolt, Walter L. Koltun, Oscar H. Levine, "Doctoral Feedback Into Higher Education," *Science*, May 14, 1965, pp. 918-28. See Richard Stone, "A Model of the Educational System," *Minerva*, Winter, 1965, pp. 172-86, for a more general approach.

model, and discussing the possible implications for science policy.

The proportion of new scientists choosing teaching careers is used here as a measure of the allocation of new scientists between teaching and research. The remaining new scientists are assumed to become researchers. A crude measure of the allocation hence is the proportion of new science Ph.D.'s whose first employer is a college or university, as shown in Table 1. This measure is crude because the educational function does not correspond uniquely to the institutional sector called "colleges and universities": doctoral scientists in colleges and universities are not all engaged in full-time teaching[4] and conversely some educational endeavor is carried on in institutions other than colleges and universities. There are furthermore important complementarities between teaching and research.[5] But teaching and research are not only complementary; they may also be competitive at some point for the scientists' time and attention.[6]

TABLE 1

PROPORTION OF NEW PH.D.'S IN SCIENCE FIRST EMPLOYED BY A COLLEGE OR UNIVERSITY, 1957–62

Year	*Proportion*
1957	.46
1958	.49
1959	.51
1960	.44
1961	.44
1962	.44

SOURCES:
L. R. Harmon and H. Soldz, *Doctorate Production in United States Universities, 1920–1962* (Publication 1142 of National Academy of Sciences, National Research Council, Washington, D. C., 1963 for 1957–1961); Bolt, Koltun, and Levine, *op. cit.*, for 1962. For a breakdown among various fields of science (and engineering) see both sources. For a discussion of the variations in the proportion over time, see Bolt, Koltun, and Levine, *op. cit.*, p. 924, and below in Part V.

II. *Objectives of Science Policy*

A critical and most difficult aspect of analyzing science policy is the specification of operational policy objectives. The National Science

[4] In 1960 there were 130,000 scientists primarily employed by colleges and universities but only 62,000 full-time equivalent teaching scientists according to the National Science Foundation, *Profiles of Manpower in Science and Technology* (NSF 63-23, Washington, D.C., 1963), p. 25.

[5] As a recent picturesque formulation put it: "Teaching and research are like sin and confession—if you don't do some of the one you don't have anything to talk about in the other." Soviet planners seem to have learned this lesson the hard way. An extreme compartmentalization of teaching and research, instituted after the Bolshevik revolution, was gradually seen to be detrimental to both the quality of higher education and to the productivity of the research effort. See Alexander G. Korol, *Soviet Research and Development: Its Organization, Personnel, and Funds* (Massachusetts Inst. of Tech. Press, 1965), pp. 83-103.

[6] See, for example, *Conflict Between the Federal Research Programs and the Nation's Goals for Higher Education*, Eighteenth Report of the Committee on Government Operations, H.R. No. 1158, 89th Cong., 1st sess., Oct. 13, 1965.

Foundation experience illustrates this point. Although directed by the 1950 legislation which established the foundation to formulate the objectives of a national science policy, the NSF has made little progress toward this end. In general, studies dealing with science policy have encountered similar difficulties in defining policy objectives and have usually formulated only very broad and sometimes ambiguous objectives.[7] The present study attempts a different approach. We attempt to identify and treat systematically some explicit though simplified policy objectives as a way of clarifying some difficult aspects of science policy. This specification of objectives is based on several assumptions:

First, it is assumed that there are social benefits for both teaching and research. Research provides social value in various ways, including the social productivity of the research effort and its contribution to the stock of knowledge which contributes to the educational process. The social value of teaching is partly the social benefits of education itself but also the future teaching and research effort of students who become the next generation of scientists. The products of both research and teaching are therefore to some extent "public goods" consumed in part by society as a whole. Furthermore, while both teaching and research provide both current and future benefits, the benefits of teaching probably lie further in the future than the benefits of research. These considerations suggest that the private rewards for teachers relative to those for researchers may fail to represent the social benefit of teaching relative to research. Thus the free market may fail to allocate skills properly between teaching and research.[8]

Second, it is assumed that the amount of effort devoted to teaching and research can be measured by the number of (full-time equivalent) teaching and research scientists, respectively. Because of wide disparities in teaching and research abilities, this assumption must be viewed as no more than a first approximation.[9]

Third, it is assumed that the policy objective can be represented as the maximization of welfare and that the welfare function is composed of two terms, indicating its dependence both on the number of research and teaching scientists at some future date and on research and teaching scientists and on time during the intermediate period of time between the present and future date. Mathematically the welfare function is of the form:

[7] See B. L. R. Smith, *The Concept of Scientific Choice: A Brief Review of the Literature* (RAND Corp., P-3156, June, 1965). An abridged version of this paper will appear in *The American Behavioral Scientist*, May, 1966.

[8] That the free market fails to allocate appropriately does not, of course, mean that a government influenced allocation will be appropriate. Poor government policy—or lack of policy—may influence the allocation in an undesirable direction.

[9] Scientific output is measured via inputs just as in national income accounts government output is measured via inputs.

$$W = F[R(T), E(T)] + \int_0^T I[R(t), E(t), t]dt$$

where:

W = welfare, to be maximized
t = time
$t = 0$, initial (present) time
$t = T$, terminal (future) time, possibly infinite
$R(t)$ = research scientists at time t
$E(t)$ = teaching scientists at time t

and where the future component of welfare, at the end of the period ($t = T$) is summarized by the function $F[. , .]$ and the intermediate component of welfare, over the intermediate period ($0 \leq t < T$) is summarized by the function $I[. , . , .]$.

The components of welfare during the intermediate period are added (integrated) to obtain the contribution to welfare. An underlying assumption of the welfare function is therefore that intermediate components of welfare are independent and additive. This assumption must also be considered a crude first approximation. To the extent that research is basic research, the intermediate component of welfare depends not only on the number of research scientists at any particular time but also on the number of research scientists at previous times. Even excluding time discounting, ten research scientists working for one year do not produce the same results as one research scientist working ten years.

The welfare function as defined above is a very rich one in that it includes many possible objectives of science policy as special cases. Three cases in particular may be cited:

1. *Maximum Terminal Value.* Maximize the value of scientific effort at some future date, given the date and the value on this date of scientists as teachers relative to their value as researchers.[10]

2. *Minimum Time.* Minimize the time required to attain given terminal numbers of research and teaching scientists.

3. *Maximum Present Value.* Maximize the present value of teaching and research over the interval from the present to some future date, possibly infinite, given a constant discount rate and a constant value of scientists as teachers relative to their value as researchers during the time period.[11]

[10] The future values of scientists as teachers and as researchers must, of course, partly take account of the period beyond the terminal date. Planning should not aim at a terminal point with no concern for the state of affairs beyond that point.

[11] When there is no upper limit on the terminal date, the present value object, with an infinite time horizon ($T = \infty$), is perhaps most relevant since maximizing welfare within each period does not necessarily lead to the maximization of welfare over the entire period. There may be, however, some circumstances in which a particular terminal date may be

These three special cases as well as a more general case will be used below to obtain, within the framework of a model, preferred allocations of new scientists between teaching and research careers. No pretense is made that even the general welfare function is general enough in exhausting all important aspects of the problem of specifying science policy objectives or even treating all the relevant variables. The welfare function nevertheless does include some significant variables likely to have a bearing on important policy choices, and we believe it may serve a useful heuristic function in government planning. Furthermore, having a welfare function in the model also opens up the possibility of an analysis under uncertainty, since the expected utility approach applies.

III. *A Model for the Allocation of New Scientists*

The problem of allocating new scientists between teaching and research can be approached using the conceptual framework of a model. The model developed here is similar to those proposed by Stoikov and by Bolt, Koltun, and Levine.[12]

The number of scientists at time t, $S(t)$, equals the number of research scientists, $R(t)$, plus the number of teaching scientists, $E(t)$:

$$S(t) = R(t) + E(t).$$

The increase in the number of scientists at time t, the time rate of change of S, $\dot{S}(t)$, is hence the sum of the time rates of change of R and E:

$$S(t) = R(t) + E(t).$$

The increase in the number of scientists at any time depends, among other factors, on the number of teachers, the productivity of teachers, the number of students, and laboratory-classroom facilities. It will be assumed that of these factors the critical ones are the number and productivity of teachers. This assumption is probably justified for the United States at the present time and for the immediate future in view of the continued pressures toward greater graduate school enrollments.[13] Thus we hypothesize a production function for new

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important; for example, the U.S. commitment to land a man on the moon before 1970 and the goal of a developing nation to achieve a "critical mass" of scientific capability as rapidly as possible.

[12] V. Stoikov, *op. cit.*; Richard H. Bolt, Walter L. Koltun, and Oscar H. Levine, *op. cit.*

[13] See William V. Consolazio, "Sustaining Academic Science, 1965-75: A Science Re-

scientists dependent on the number of teaching scientists and time, $f[E(t), t]$, representing the gross increase in the number of scientists in year t. The dependence on time allows for productivity improvements due, for example, to technological aids such as programmed instruction. Presumably both partial derivatives of f are positive. The net increase in the number of scientists is the gross increase less depreciation. Assuming the number of scientists leaving the field of science because of death, retirement, or transfer to other occupations is proportional to the total number of scientists, the depreciation term is $dS(t)$.[14] The net increase in the number of scientists is then:

$$S(t) = f[E(t), t] - dS(t).$$

The measure of the allocation of new scientists between teaching and research will be the proportion of new scientists becoming teachers. The historical record of this measure is given in Table 1 for 1957-62. By the above assumption that scientists are either teachers or researchers, if b is the allocation proportion, then $(1 - b)$ represents the proportion of new scientists becoming researchers. Assuming the same depreciation rate d applies to both teaching and research scientists, the rates of increase in the number of teachers and number of researchers are, respectively:

$$E(t) = bf[E(t), t] - dE(t)$$
$$R(t) = (1 - b)f[E(t), t] - dR(t).$$

It is assumed that appropriate policies can affect initial career choices but only within certain narrow limits. Thus:

$$0 < b_o \leq b \leq b_1 < 1$$

where b_o and b_1 are the presumably distinct lower and upper limits on the allocation proportion. The case of compulsory choice of occupation,

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sources Planning Study," *Educa. Record*, Spring, 1964, pp. 210-29. In other countries, this assumption may not be justified. Cuba, for example, seems to have suffered recently from an excess supply of teachers and insufficient numbers of students. See A. R. Jolly, "Education," in Dudley Seers, ed., *Cuba: The Economic and Social Revolution* (Univ. of North Carolina Press, 1964). Further, this assumption may not be justified for the United States at some future point. There is clearly some upper limit to the supply of individuals who are capable of becoming productive scientists.

[14] Richard H. Bolt, Walter L. Koltun, and Oscar H. Levine, *op. cit.*, estimate d is 0.2; i.e., 2 percent of scientists leave the profession each year due to death, retirement or transfer.

for which $b_0 = 0$, $b_1 = 1$, is rejected as unacceptable in a democratic society.

There are several limitations to the model. First, the model does not explicitly incorporate real world conditions of supply and demand for scientific talent and the complex interrelationships between the two. A sharp decline in the amount of government funds channeled to the support of research and development, for example, could result in a decline in the attractiveness of a scientific career and a possible slacking off in student enrollments. A full analysis of the problem would have to devote much more careful attention to real supply and demand conditions and how the allocation of new scientists is thereby affected.

Second, the model does not consider changes in careers for established scientists, in particular, changes between teaching and research careers. Such midcareer changes may be of considerable importance for certain disciplines in which scientists reach their creative peaks at an early age, leading in some cases to initial careers in research followed by careers in teaching. These midcareer changes have been neglected partly for analytical convenience and partly because public policy probably has a greater influence over initial career choices than changes in established careers. The model can, hopefully, be extended to deal with this aspect of the problem.

A third limitation of the model is the fact that it considers only two broad sectors: teaching and research. It would be desirable to consider finer distinctions and other sectors, such as undergraduate versus graduate teaching, basic versus applied research, and administration. The crude two-sector model employed here, however, does provide some insights and hopefully can be extended to provide a richer representation of the complex reality of this class of allocation problems.

Finally—and most important—the model does not directly consider questions of quality. Such questions are of course crucial and are difficult to assess on the basis of quantitative indices. Quality is ignored here, partly because of the difficulty of measurement and partly because quantitative relationships are to some extent indicative of quality in both education and research.

IV. *Preferred Time Paths for the Allocation Proportion*

The problem of this section is to determine a preferred time path for the proportion of new scientists entering teaching $b^*(t)$. By "preferred" is meant the attainment of some policy objective as discussed in Part II within the context of the model developed in Part III. Special cases of this general problem are those of the three special cases of objectives, maximum terminal value, minimum time, and maximum

present value, combined with the assumption of constant average production of new scientists per teaching scientist. The latter assumption is:

$$f[E(t), t] = gE(t)$$

where g is the average production rate, assumed constant.[15]

The preferred time path for the allocation proportion in these special cases is:

$$b^*(t) = \begin{cases} \text{maximum} = b_1 & \text{if} \quad 0 \leq t \leq \bar{t} \\ \text{minimum} = b_o & \text{if} \quad \bar{t} < t \leq T \end{cases}$$

i.e., allocate the maximum proportion of new scientists to teaching during an initial period and then allocate the minimum proportion to teaching during a terminal period. At the switching time $\bar{t}$, which depends on the parameters of the problem, the allocation proportion switches from a maximum value.[16] The result is often referred to in capital theory as a turnpike theorem since the best path is the indirect turnpike which allocates initially a maximum proportion to teaching and finally a minimum proportion to teaching.[17]

An example of the minimum time problem is one for which there are initially 100 teachers and 80 researchers and the cutoff date is defined by 200 teachers and 240 researchers. Each teacher produces .14 scientists each year, scientists exit their profession at the rate of 2 percent per year and through appropriate policies the percent of new scientists allocated to teaching can vary from 10 percent to 60 percent. The solution is then to allocate the maximum (60) percent of new scientists to teaching in the first 11.2 years and then to allocate the minimum (10) percent of new scientists to teaching in the remaining 4.3 years, attaining the desired terminal numbers of researchers and teachers in 15.5 years.

A more general case is that of the general objective function of Part II combined with the general production function of Part III. Assuming the intermediate component of welfare is summarized by a linear function, the preferred time path for the allocation proportion in this more general case is still one in which the allocation proportion at any time is typically at its maximum or minimum values, but in this case

[15] Richard H. Bolt, Walker L. Koltun, and Oscar H. Levine, *op. cit.*, estimate g as .14; i.e., seven doctorate educators on the average produce one new doctorate each year.

[16] The switching solution is obtained from the technique of control theory. See L. S. Pontryagin *et al.*, *The Mathematical Theory of Optimal Processes* (Interscience, 1962). It is assumed that $b_o < d/g < b_1$.

[17] See T. C. Koopmans, "Economic Growth at a Maximal Rate," *Q.J.E.*, Aug., 1964.

there may be more than one switch between these extreme values. The allocation proportion is at its maximum (minimum) value if the marginal social benefit of teachers exceeds (is less than) the marginal social benefit of researchers.[18]

V. *Some Possible Implications for Science Policy*

The Switching Character of the Analytic Results. A first point that merits reflection is the switching nature of the analytic results. The desired objectives are best achieved by allocating either a maximum or a minimum proportion of new scientists to teaching, possibly switching one or more times between extreme values. This notion will hardly be surprising to economists familiar with unbalanced growth and turnpike theorems. It might, however, appear rather novel to policy-makers and observers of science policy who have often considered balance and gradualism important components of science policy. But the notion of pronounced shifts in policy actually has some intuitive appeal. Science, like national economies, often advances in an unbalanced pattern, making rapid strides first in some sectors and then in others. A high degree of flexibility and responsiveness to changing needs may therefore be an important requirement for science policy.

In particular, we may need a high degree of flexibility among the institutions chiefly responsible for the nation's scientific effort. Administrative agencies must be flexible enough to "shift gears" quickly and to make appropriate changes in their policies and procedures. A research organization established for a specific mission, for example, might have to be disestablished promptly once its mission was fulfilled. Such a flexibility does not generally exist in the conduct and management of the nation's scientific effort: organizations and programs once established tenaciously cling to life even though their original mission may have been completed; institutional policies and procedures tend to generate an inertia which makes innovation difficult; and institutional policies make career patterns in many fields of science somewhat inflexible. Of course, there are costs to be incurred in achieving greater flexibility that doubtless place certain realistic limits on the scope for

[18] The time paths of these marginal social benefits (or shadow prices) are obtained from control theory. If e is the marginal social benefit of teaching and r is the marginal social benefit of research then:

$$\dot{e} = -\frac{\partial I}{\partial E} - [be + (1-b)r]\frac{\partial f}{\partial E} + ed \qquad e(T) = \frac{\partial F}{\partial E}$$

$$\dot{r} = -\frac{\partial I}{\partial R} + rd \qquad r(T) = \frac{\partial F}{\partial R}$$

See L. S. Pontryagin, *op. cit.*

innovation. One cannot normally envisage a total description of prevailing institutional or career patterns. Institutions devoted to scientific activities are difficult to create *de novo* and their fate cannot be made wholly dependent on short-term variations in assessments of national needs. But marginal adjustments in the system from time to time can and probably should be made with the objective of making the system more adaptive to changing needs.

The Importance of the Educational Function. Another interesting implication of the analysis is the importance to be inferred for the educational function. Since teaching and research in some ways represent incommensurable values, it is difficult or impossible to specify unambiguous objectives for science policy. For a number of reasonable assumptions concerning objectives, however, the model imputes a high social benefit to teaching. The best route to attain certain goals is through the indirect path of first strengthening the educational sector. This result has some intuitive appeal and appears to be supported to some extent by historical evidence. Ben-David, in accounting for German primacy in the field of medical research at the turn of the century, has pointed to the importance of a reserve of scientific capabilities in academic institutions that enabled Germany to overtake the previous leaders in medical research.[19] Kaysen has also argued the importance of a reserve of scientific capabilities, especially in academic basic science, that can be drawn on for various purposes in times of extreme national need.[20] A weakened educational capability might place a strain on growth potential by not providing the "slack" in the system necessary to exploit new opportunities. Thus there may be some good sense in emphasizing the strengthening of the educational sector.[21]

In recent years there has been a growing concern that education has suffered in the great expansion of federal research support programs.[22]

[19] Joseph Ben-David, "Scientific Productivity and Academic Organization in Nineteenth Century Medicine," in Bernard Barber and Walter Hirsch, eds., *The Sociology of Science* (The Free Press, 1962), pp. 305-28.

[20] Carl Kaysen, "Federal Support of Basic Research," in *Basic Research and National Goals* (National Academy of Sciences, 1965), pp. 147-68, esp. pp. 150-51. The scientific effort in World War II provides an example of the effective use of academic scientists in time of extreme national need.

[21] This conclusion, perhaps surprisingly, may have considerable relevance for the developing nations. These nations face understandable pressures for early results from their investments in science and technology. Frequently they are advised to take the "natural" route of applying the fruits of science and technology of the advanced nations to their immediate problems. The ease of scientific and technological "transfer," however, has probably been greatly overestimated. In actuality, a developing nation may need a flourishing science and higher education if it is to adapt effectively the technology of other nations. See Edward Shils, "Scientific Development in the New States," in Ruth Gruber, ed., *Science and the New Nations* (Basic Books, 1961), pp. 217-26, and H. J. Bhabba, "Science and the Problems of Development," *Science*, Feb. 4, 1966, pp. 541-48.

[22] *Conflicts Between the Federal Research Programs and the Nation's Goals for Higher Education*, report of the Committee on Government Operations cited above; *Federal*

Common arguments are that professors have been motivated to neglect teaching in view of the easy availability of research funds and that promising scientists have been lured away from teaching by lucrative opportunities in industrial or government research institutions. The evidence is not sufficiently clear to permit a very satisfactory evaluation of such arguments. Extensive government support of research in some ways has undoubtedly strengthened higher education. Federal research funds to colleges and universities have likely enhanced the attractiveness of a teaching career and in many cases probably have improved the quality of graduate education by providing graduate students an opportunity to gain research experience.

Yet there may be some reason to fear the long-term consequences of present policies. As the moon venture comes to maturity, for example, a strain may be put on the supply of scientific talent available for employment in colleges and universities.[23] While the prestigious national university centers will probably have little difficulty in attracting promising new scientists as teachers, the liberal arts colleges and the lesser-known universities, especially those in certain sections of the country, could encounter substantial difficulties in attracting teaching scientists. This could have an adverse effect on the educational process on the theory that the national university centers depend on infusions of talent from the secondary institutions, and a weakening of any part of the system could ultimately offset the health of the system as a whole. Of course, it is not only federal research support programs that might have an importance influence on the quality of education. Some universities, in an effort to upgrade their academic status, may overemphasize research relative to teaching. These factors, in any case, point toward the need to consider seriously how our society can assure the continued vitality of the educational system at a time when ever increasing demands are placed on scarce scientific manpower resources.

Policy Initiatives. Present policies of federal agencies primarily responsible for science policy clearly have some influence over both the initial career choices of scientists and the distribution of scientific effort between teaching and research in academic institutions. For example, the increased national interest shown in graduate education and the increased availability of federal funds for research in colleges and universities probably contributed to the rise in the feedback of new scien-

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Support of Basic Research in Institutions of Higher Learning (National Academy of Sciences, Publication No. 1185, 1964); and Harold Orlans, *The Effects of Federal Programs on Higher Education* (Brookings Institution, 1962).

[23] See *Towards a Better Utilization of Scientific and Engineering Talent: A Program for Action*, A Report of the Committee on Utilization of Scientific and Engineering Manpower (National Academy of Sciences Publication 1191, Washington, D.C., 1964).

tists into higher education between 1957 and 1959, as reported in Table 1. Similarly the decline in the feedback into higher education after 1959 probably arose in part from the expansion of research programs in industry and the federal government resulting from the space program. It would seem desirable for the federal science agencies to exercise this influence in a more systematic fashion, paying closer attention to the interrelations of their separate policies and to the long-term consequences of their policies. There are a number of policy adjustments which could contribute to this end without either basically altering the pluralist pattern of science support, disrupting the intricate doctorate producing system, or leading to unacceptable government controls over science.

One possibility is a program of teaching and research fellowships which would either supplement or supplant present postdoctoral fellowship programs.[24] The relative numbers of teaching and research awards could be varied according to an assessment of the desirability of increasing the number of teaching or research scientists at that time.[25] Such a program could enable science policy-makers to stimulate science doctorates toward critical sectors of the nation's scientific effort.

A second set of policy initiatives would involve closer attention to the subtle incentives in government contracts. Adjustments of seemingly minor contractual details could have a considerable impact on the allocation of new scientists and the distribution of scientific effort between teaching and research. Some government contracts with universities, for example, currently do not allow time spent on teaching as a legitimate overhead cost, and thus probably serve as an incentive to channel university resources toward research and away from teaching.[26] These provisions could be amended to allow some portion

[24] The recent report of the Committee on Government Operations, *op cit.*, p. 26, recommended a plan similar to that suggested here for a program of science teaching fellowships. The dangers of present postdoctoral fellowships introducing disruptive effects on university teaching programs are discussed in *Federal Support of Basic Research in Institutions of Higher Learning*, *op. cit.*, pp. 92-93, and in E. C. Pollard, "How to Remain in the Laboratory Though Head of a Department," *Science*, Sept. 4, 1964, pp. 1018-21.

[25] To assist disadvantaged regions, a regional formula might be introduced in this fellowship program.

[26] The point may be illustrated concretely by reference to certain policies of the National Institutes of Health (NIH). The NIH considers capital expenditures on research equipment, for example, as allowable direct costs. But only that proportion of the investigator's salary which is equivalent to the proportion of time spent working on the research project is considered an allowable direct cost. The effect of this requirement is offset to some extent by the fact that many graduate students are employed as research assistants on NIH-supported research projects and the portion of the investigator's time devoted to directing the efforts of these students is chargeable as a research cost. Nevertheless, it is difficult to escape the conclusion that this provision may act as a subtle incentive for the recipient of NIH research funds to be concerned with the research project and psychologically to be less sensitive to teaching responsibilities (especially undergraduate

of an investigator's time spent on teaching as a legitimate overhead cost and thereby influence the allocation of effort between instruction-oriented and noninstruction-oriented activities, especially for universities heavily supported by federal funds.

Another policy initiative that invites consideration is the prospect of obtaining teaching services from scientists engaged primarily in research occupations. Qualified research scientists working for the government or for a government contractor could be allowed and encouraged to spend some portion of their working time teaching either within their own institutions or at approved nearby academic institutions.[27] Such a proposal could stimulate researchers to become researcher-teachers just as many teachers have become teacher-researchers. Such a plan would offer several advantages: First, it would serve to facilitate recruitment for certain kinds of research institutions that might wish to offer an opportunity for an individual to retain an academic tie. Second, such an arrangement might help strengthen the role of the applied sciences in graduate curricula.[28] Third, the plan could be used to encourage a greater geographical dispersion in the distribution of scientific talent and a greater mobility in the scientific career. By co-locating a federal research establishment near a cluster of universities in one of the scientifically less privileged regions of the country, for example, it might be possible to elevate the quality of science instruction in these universities. In certain cases both the quality of graduate instruction and the productivity in mission-oriented research could be enhanced by a closer relationship between research and academic institutions.

We conclude by cautioning against an unrealistically rigid distinction between "teaching" and "research." Although we believe that it may be desirable from time to time to influence the career choices of scientists toward either teaching or research, the interrelations between the two classes of activity (and between different science insti-

teaching). It can probably be assumed that, by allowing some portion of the investigator's time spent on teaching as an allowable cost, there might be subtle but important shifts in incentives that could serve to strengthen the teaching role.

[27] Alvin M. Weinberg proposed such a plan in "The Federal Laboratories and Science Education," *Science*, Apr. 6, 1962, and has started such a system at Oak Ridge under an arrangement with the University of Tennessee. See John W. Finney, "U.S. Will Examine Its Laboratories," *New York Times*, Sept. 6, 1964, p. 37. For some empirical findings on the willingness of industrial scientists to teach part-time, see J. W. Nystrom, "Can Industrial Laboratories Supply Part-Time College Teachers?" *College and University*, Winter, 1964, pp. 135–46.

[28] In the recent National Academy of Science report, *Basic Research and National Goals*, *op. cit.*, the papers by Arthur Kantrowitz and Edward Teller strongly urge such a plan. They argue that the United States has slipped badly in the applied science fields and that cooperative arrangements between academic institutions and government and industrial laboratories could strengthen graduate enducation in the applied sciences.

tutions) are of critical importance. The quest should be to promote fruitful patterns of interaction among different science institutions while retaining the special competence of each. The policy initiatives suggested here, while far from being fully developed programs for government action, seem to offer promise of strengthening the ability to manage the nation's scientific resources. As such we believe they merit further examination by observers of science affairs and responsible officials.

DISCUSSION

William Capron: In commenting on these two papers, I have chosen to concentrate on only a few of the points I would like to discuss. The excellent review paper by Mansfield in particular invites extended discussion but time does not permit this.

It is by now well recognized that a competitive economy will underinvest in R and D judged from the standpoint of society. Mansfield makes this point and I accept it as far as it goes. But it may not go far enough to meet reality in the U.S. in the 1960's. For the theoretical underpinnings of this theorem as it has been developed by Arrow *et al.* require competition. It is not clear that the monopoly elements—and particularly the importance of oligopoly—which characterize the U.S. economy do not render the theorem inapplicable. It has been argued that in at least some market situations firms may be investing too much in R and D from society's standpoint. It is not clear, for example, that the drug industry is not overdoing R and D.

My own view on this is that we can say nothing with much confidence on theoretical grounds about the social adequacy of our total R and D effort—though my hunch is that as a nation we are underinvesting R and D over all. However, I think we can say with assurance that the existence of noncompetitive elements and sectors of the economy produces a misallocation of resources within the R and D total. Indeed, this really falls out of the basic theorem: if firms in markets which conform reasonably well to the competitive model underinvest in R and D, and if we have reason to suspect "excessive" R and D in noncompetitive markets (at least under certain conditions), then the balance of total private R and D is thrown out of whack. It might be argued that the total number of dollars invested in R and D turn out to be about right from a social optimality viewpoint in oligopolistic markets. But it is very unlikely that the particular R and D program in such industries will correspond to the socially desirable. The reason for this hypothesis is clear: a major motive for R and D in oligopolistic situations is to maintain—or, if lucky, enhance—the firm's market share. A number of economists who have examined industrial R and D in some detail have found a significant emphasis on producing relatively certain, modest improvements in products and processes. They have also found an indication that many firms design their R and D efforts in part, at least, as "defensive," and it is unlikely that R and D aimed at "protecting a firm's flanks" is socially optimal in its specific use of resources.

There is another noncompetitive element in our system which leads to higher levels of R and D than analysis of the purely competitive model suggests: eleemosynary nonprofit support of research. Private individuals and foundations support R and D—with heavy emphasis on basic research—for many noneconomic motives, most notably perhaps in health-related fields. Moreover, private "charitable" contributions support research in connection with support of higher educational institutions. Finally, individuals undertake

"extracurricular" research for many noneconomic reasons: thirst for knowledge, desire for fame, as excuse to get away from their families, etc.

In summary, we can identify departures from perfect competition which create at least a presumption that R and D will be supported at a level above that indicated by consideration of the competitive model. And we must therefore be a bit careful in a flat assertion that without government action and support, too little R and D will be done to be socially optimal. Nonetheless, let me join Mansfield in suggesting that our system will underinvest in R and D in the absence of public action. More significant and interesting from the standpoint of policy, I am convinced that a proper balance and emphasis in our total R and D efforts can only be achieved by active federal support and related policy initiative. Not only is this necessary to achieve appropriate emphasis at each point along the spectrum from very basic research to final product development; it is also required if different disciplines and problem areas —and technical possibilities—are to receive appropriate attention.

Shouldn't we be putting federal R and D money and the scientific and technical talent that money buys into learning how to control pollution, desalinate water, move people on the ground from Washington to Boston in one or two hours, etc., rather than insisting on getting a man on the moon by 1970? This query or variants thereon have, as Mansfield says, occurred with growing frequency in the last few years. For example, Congressman Reuss and his Government Operations Subcommittee open hearings on January 7 which are prompted by concern over an alleged serious misallocation of R and D resources.

This is certainly an important policy issue—and since it is a resource allocation question, an issue in which economists should presumably have an interest. But I suggest that it is not primarily an issue which revolves around the level and composition of government R and D expenditures. Rather at issue is the evaluation one places on the relative priority and significance of various public programs—and not on the desirability of R and D per se. R and D is a means, not an end—it may help to improve the manner in which we move towards the objective, but it is an input, not an output. The question is whether one views the social benefits of getting to the moon as being more or less important than improving the quality of the environment of our cities, the purity of our water, or the speed and comfort of ground transportation. At least the way statements on this subject have frequently been made suggests this is the issue rather than an argument about R and D expenditures as such —although the dialogue is very confused. For one thing, those who argue that we should not be putting so much into the Apollo program but instead should be doing more on more mundane and earth-bound problems rarely go on to examine in specific terms the payoff that could be got from a significant increase in, say, pollution research. It may very well be that we should as a nation be doing more about pollution control, but it is by no means clear, as far as I am aware on the basis of available evidence, that this "more" should consist exclusively, or even primarily, of more federally-sponsored R and D. In short, one cannot sensibly evaluate the desirability of increasing R and D di-

rected at a particular problem area without some indication of the probability of achieving improvements in that area and the costs of that R and D. The fact that one may disagree with the implied social utility function used by the Congress and Administration in setting the relative importance of various public programs does not necessarily and inevitably lead to a conclusion that the pattern and level of R and D expenditures are not socially useful.

On spillover, I have only a brief comment: discussion of this topic is greatly improved if we distinguish clearly between product spillover and what I will call "technology" spillover. There seems to be a consensus that military and space products have considerably less civilian application potential than was the case up through World War II. But it is not nearly so clear that the spillover potential of military and space-related technology has been sharply reduced. As a single example, I point to computer technology. It is true that many of the most exotic miniaturized computers developed for military and space purposes have no place in the civilian market. I have heard no one suggest that civilian-use computers of the variety, capability, and relatively low cost which are on the market today would have been available nearly so soon had not defense needs prompted a major federal push in the 1950's.

Turning finally to the Intriligator-Smith paper and the research versus education conflict, I have one major point and a few quick comments. The major point is a basic problem with the authors' model which assumes teaching and research to be substitutes and therefore competitive resource-using activities. Intriligator and Smith recognize verbally that there is some complementarity but their model ignores it. While I realize how imperfectly we understand the teaching-research relationship in higher education, I am of the view that the complementarity is strong enough to throw serious doubt on the real-world applicability of the model in its present form. More specifically, I suggest that on the definitions used by the authors, we might well increase basic research by increasing the number of scientists who were teaching (as measured in the paper)—or vice versa.

Let me emphasize my appreciation of the attempt made by Intriligator and Smith to build a specific model to tackle this issue. As they modestly indicate, they do not feel they have said the last word on this subject and will perhaps not object if I add to the above point one or two suggestions. First, I am not sure, from a policy standpoint, how useful it is to focus on "science" (and science teaching and scientific R and D) as a single aggregate. Rather, it seems to me that the policy-maker and administrator are typically concerned with particular disciplines, e.g., biophysics, solid-state physics, etc., and with particular types of R and D.

Second, if one accepts the suggestion that the model be adapted to deal with particular disciplines, this throws in question the notion that the supply of students is never a limiting factor—an assumption which bothers me even where all science is treated as one. But if one is looking at, say, physics, the supply of students qualified both by ability and by previous training cannot be assumed unlimited. The competitive attraction of the various disciplines changes over time—and is a very illusive factor. But at any given time I do

not believe it to be the case that any increase in the capacity to teach physics will automatically attract a sufficient number of qualified students to be fully utilized—the assumption underlying the Intriligator-Smith analysis.

Third, I fully understand the authors' inability to use the third objective function they suggest as a possible "special case": maximum present value of teaching and research over time; yet conceptually this seems to me clearly the correct objective. But I am troubled with the other two "special cases" they suggest, "maximum terminal values" and "minimum time," since I find it hard to imagine a situation when a policy-maker should employ either one of these objective functions. With regard to influencing career choice, I think the real policy issue is the extent to which government tries to do this overtly, and the extent to which instead we rely on the market. I would not depart from the conclusion Arrow and I reached several years ago: the market for scientists and engineers and the way this scarce resource is allocated works remarkably well considering the "special" character of this market.

Finally, I agree (of course!) that administrators should be more flexible and that we should be willing and able to switch policy from time to time. Some argue we have done too much "switching" in federal programs—the charge of "on-again, off-again" R and D support is familiar. But I am somewhat concerned at the implications of following a switching policy for educational institutions, in part because of the social costs implied by the periodic excess capacity in the physical plant of our colleges and universities. Most important, I do not know how, given our basic ideology, we can persuade students who happen to graduate from high school just when we are switching to a heavy research, light teaching mix that they cannot be scientists because many of the teachers are needed elsewhere. In short, I think it very tricky to implement a switching policy on an "open," overt, national basis.

Let me close by applauding the program committee for scheduling this session. There are a host of intriguing and vital problems in the area of science policy and federal R and D support and I speak from experience when I say that there is a critical need for the profession to provide the bureaucrat with better tools for decision.

MERTON J. PECK: This session is part of what I call the "new new economics." This school is characterized not so much by the novelty of its intellectual content as by its willingness to apply economics to sectors that in my graduate school days were considered as noneconomic, and the proper concern only of political scientists and sociologists. Now all this is changed and, as this year's meeting illustrates, economists have become active in health, education, welfare, defense and, as our two papers illustrate, science policy—all sectors with a blending of private and public institutions and lacking markets as economists ordinarily visualize them.

While this expansion of scope can claim some remarkable successes, it has also had its frustrations. Economists are perhaps too ambitious, wanting to move immediately to the question of social returns and costs. Yet, as Mansfield reports, "we cannot estimate the social returns from R and D of

various sorts at all accurately" and he counsels sequential experimentalism. But I would like to amplify this statement, pointing to certain analytical and political problems of such an approach, drawing in the process upon a forthcoming manuscript by Richard Nelson, Edward Kalachek, and myself.

At present, governmental responses seem excessively dominated by lagged and massive adjustment, moving from delay and inaction to massive programs when the problem becomes acute. There is little of the continuous probing of the environment that characterizes the activities of large firms. This may be because of the threshold costs in new programs; the energies to fight bureaucratic inertia and obtain congressional approval are too great to be dissipated on small-scale experimental programs. Perhaps the way to surmount such obstacles is to give each federal agency some lump-sum no-year money for demonstration projects in its general mission area and allow great flexibility in its spending. This is the standard method of budgeting for basic research in both large firms and government agencies. This approach has been applied with some success in the Housing and Home Finance Agency and in the Poverty and Retraining Programs.

In choosing the areas in which governmental agencies might experiment, considerable importance ordinarily ought to be attached to the presence or absence of market failure. By this I mean starting from the presumption that economically worth-while activities will be carried out by private enterprise. Hence a proposed governmental program bears a burden of proof of establishing that, despite absence of private activity, the proposal is economically worth while. The gap between private and social returns in R and D, that Mansfield mentions, is the most general case of market failure but its very generality makes it a poor guide. A more detailed examination of market failure may yield some clues to its relative importance in different sectors of the R and D spectrum. The approach would be that of market structure and behavior rather than that of measuring directly private and social returns. While this industrial organization approach can hardly claim any startling successes in antitrust policy, where it has been applied, it has still been useful there and it may also be for science policy.

Once an experimental policy is launched, it is important, as Mansfield says, to have evaluation built in early into the experimental programs. All too often the program is launched and an evaluation is conducted afterwards, and in the interim, crucial data collection possibilities are forever lost. Yet, even under the best of circumstances, the program or program evaluation is difficult. It seems most successful under three conditions: (1) when applied to "final product" programs such as high speed passenger trains or strategic missiles rather than when the output is an intermediate product such as information for a whole spectrum of uncertain applications; (2) when ranking a narrow range of alternatives rather than determining the rate of returns for comparisons across wide ranges of activities; (3) when mixed strategies rather than all-or-none choices are possible. Unfortunately, a good many policy instruments in science policy such as the patent system appear to have an all-or-none character. Yet, even here, one can consider less drastic modifications, as,

for example, Mansfield's very interesting suggestion of a delayed examination in issuing patents.

This still leaves the central question of the value of public investment in R and D relative to that in education, in health, and in other governmental expenditures directed at growth. Such decisions will, of course, be made largely by bureaucratic bargaining. Lindblom termed such bargaining the invisible hand of government. Bargaining certainly applies to science policy; after all, not all political scientists are in departments so named. But the bargaining can also be likened to another decision process—that of the courts. Each agency can be viewed as a litigant prosecuting its case for more resources before a higher tribunal. The analogy is not completely novel. President Conant advocated making decisions of scientific policy through a judicial format with advocates and written opinions in his book, *Science and Common Sense* (1951).

I think such formalization undesirable and in an informal sense we now have such a process. Yet the process is now being institutionalized in a different dimension through the introduction of systems analysis, cost-benefit analysis, and all the other tools of the new economics. These were originally advanced as procedures for yielding definitive answers. Yet such approaches when transported from Santa Monica to Washington have failed to produce answers; they have made more controversy rather than less. Yet if these approaches have failed in that respect, they have produced another benefit. The systems analysis or cost-benefit framework has provided a procedure to delineate the relevant from the irrelevant and served to shape the issues in dispute. Thus they have become the rules of evidence and relevancy for bureaucratic bargaining and so have created a sort of public administration jurisprudence. As the analysis of markets shows, given the right set of rules, rivalry can be productive. Perhaps systems analysis may provide the right set of rules for bureaucratic rivalry.

I have one further suggestion concerning Mansfield's paper: why limit the examination of the costs and benefits of separating development and production to military projects? One of the peculiarities of R and D as an economic activity is that it is mostly carried on in organizations whose central purpose is other than R and D. It occurs mostly in a manufacturing firm or an educational institution, or in a mission-oriented government agency. Hence R and D finds itself usually in an environment where the major organization is other than its own activity, whether it is producing A.B.'s or automobiles. The scale of the organization, its procedures and its policies are set in large part by the requirement for these other functions. While one can argue that the present arrangement—the joint location of R and D with some other function in a common organization—work relatively well, the question of whether a greater separation might be more efficient has only been raised in connection with military development. It might be fun, as well as fruitful, to visualize a quite different organization of R and D as a separate activity, servicing business firms on some sort of contract basis.

The paper by Intriligator and Smith has a direct bearing on this question of R and D in the setting of multi-objective organization. It also serves to remind

us of the value of flexibility of such arrangements, particularly when, as with teaching and research, the various functions use a common scarce resource—in this case the scientists.

The particular solution that Intriligator and Smith set forth has considerable heuristic value. It is that of unbalanced growth and switching between extreme values for the two activities. As they point out, it appears novel to policy-makers and observers of science policy who have "often considered balance and gradualism important components of science policy." The attachment to balance, I am afraid, is often because it serves to moderate interorganizational disputes. I think, however, there are two important aspects of the production function in both education and research that explain why gradualism is so beloved.

The first is the question of the supply of students. Using their examples, prior to the switching time the student population must grow proportionately with the 60 percent maximum proportion of new scientists going into teaching. After switching, the growth in student population will fall to one-sixth of previous amount, as only 10 percent of the new scientists go into teaching. If we assume constant quality of potential students before and after the switching times, the change in the rate of growth of the student population will require a reduction in their quality.

The second aspect is the general problem of start-up and termination costs as the two activities change scale. These can be considerable in research and teaching where new organizations and traditions take years to develop. The experience of universities in building first-rate departments suggests a rule-of-thumb of a decade to locate the faculty, to develop working relationships, and to attract quality students. Such costs, combined with the uncertainties of which fields will be the most fruitful and who are, in fact, high quality researchers and teachers have led to gradualism.

Start-up and termination costs can be minimized by the high degree of flexibility that Intriligator and Smith mention in the closing parts of their paper. The now famed phrase "multi-university" reflects the fact that the educational conglomerates Americans have created begin to fit Intriligator and Smith's requirement for flexibility. Yet, as single institutions become larger and more diverse, a price is paid. Rigidities and conflicts that before were between institutions now become internalized to a single institution—at least that is one economic interpretation of the strains in the contemporary university scene.

EUGENE G. FUBINI: The fact that political scientists, economists, technologists, university professors, and administrators are gathered here is perhaps more important than the different subjects which have been and are going to be discussed.

It seems to me that the fundamental issue which underlies this and many other symposia, this and many other speeches, this and many other discussions, is the fact that in the last ten years people across the country have come to the realization that the merger of physical sciences with technology which occurred in the last fifty years has changed the character of science from that

of a factor which could be handled by a group of specialists to one which requires national consideration. This is an important point, this is a new factor, this is a change.

I do not believe that increases in the federal budget or any other factors which have been reported today have as much of an impact on public life as the fact that science and technology problems are now a part of politics. And since they are part of politics, they are part of economics. In other words, the increased cost, the increased budgets, the increased attention are causes, not effects. Once science joined forces with technology, the impact of the combination had a revolutionary effect on our lives that, to quote Mesthene, of Harvard, exceeded that of Marx. I am not referring to the political impact of the internal combustion engine in creating the suburbs, the supermarket, and the urban traffic congestions. I am referring to the test ban, the antiballistic missile defense problem, the November 8 blackout in the northeast, the impact of automation, the dams that move water from one ocean to another, the removal of mountains, the establishment of new ionospheres. So gentlemen, here is a problem you can no longer leave to a few specialists: the task of organizing technology, carrying on science, bringing about a relationship between the two. You can no longer worry only about trade, balance of payments, cost of money, depressions, economic cycles, and so on. For the first time, science and technology have become partly your problem.

I believe it is particularly fascinating to examine the technical factors as they apply to politics and as they are being handled today. The reason for my fascination is one that I have discussed in the past and that I will summarize today by simply stating that in my mind one of the greatest inventions of the human mind is a democratic government—a government whereby people learn to do things that are greater than themselves. The democratic process that used to be applied in the years gone by, and still is, to economic problems such as whether silver should be monetized or not, tariffs high or low, currency hard or soft, the Federal Reserve independent of the executive or not, this process is now applied in its full force to the problems of science and technology.

Truly, the process of government is not as clearly defined as the processes that we are accustomed to recognize in other fields which have a much longer history of political involvement. Yet, the processes are similar. The parliaments are often appointed rather than elected and one of them is what is loosely called the "scientific community." Elements of the Congress participate in and with the scientific community in arriving at these decisions.

We see in the government documents circulating from office to office, agency to agency, to obtain "concurrence." This is in my mind nothing else than the well-established process of obtaining a consensus to get yes votes. Sometimes I am amazed to hear people complaining in the area of technology about the fact that they can never find the person who makes a positive decision. Yet these same people, when questioned, would recognize that one of the characteristics of the democratic process is precisely not to have a single person who can say yes. I see no reason to complain when the same political methods are

applied to technological problems. I am left to wonder why people seem to protest the generalization of a principle which has been proven sound. I believe that some of the speakers who have discussed the problems of science and technology in this gathering may have failed to make the point that the same process which used to be applied to economics is now being applied to technology as well. I believe that we should welcome this process and try to apply the rules that we have learned with suitable modifications.

Dr. Mansfield's list of issues and problems of national science policy are indicative of the point of view of an outside observer who is not deeply and personally involved in these problems as an actor, but rather as a monitor. Some of the observations he has made are extremely cogent; others should be questioned.

As I just stated, it is my impression that if we significantly increase the amount of attention devoted to national science policy, it is due less to the increased expenditures than to the importance of the results. To put it another way, the results have been so important they are gathering increasing expenditures and increasing attention. The second observation I would like to make relates to the point of view of the author that less research and development is likely to be generated than would be socially desirable in a free economy—at least but not exclusively in the field of basic research. I maintain that the private sector is willing to take risks and that the government does not have to do it all.

I must take issue with the point of view that knowledge should be disseminated free of charge in order to be socially optimal. I believe in security classification, in royalties for authors, and patents for inventors. Dr. Mansfield concedes my point, however, in the next phrase. If we assume, as I do, that industry is responsible for and should be encouraged to carry on a substantial amount of research, it is socially not only optimal but necessary that in a few cases the industry be given the chance of acquiring a patent, not necessarily to make money out of the discovery made by its own research department, but rather to defend itself against attacks from the outside. To summarize, then, my disagreement with Dr. Mansfield is that I do not believe that industry is unwilling to take risks, provided a certain amount, although not necessarily large, of compensation is made available.

Dr. Mansfield repeats the often made remark that in the nondefense sector, research and development have not been supported with enough enthusiasm by the government, and that in those industries where many small firms predominate rather than a few giant ones, basic research and product development have been neglected.

I used to agree with this statement. When I was in the government I attempted to generate a meaningful research program in one of the so-called "neglected" areas, and I must say that I have found there are many good reasons for the neglect. The primary reason is that the results of the initial research and development work in these areas have been very disappointing. It

is not always easy to give increasing support to areas where the initial effort failed. Yet, I guess that continuing effort in the areas which represent large industrial expenditures with relatively small research is necessary. The building and transportation industries have a percentage of research and development which is too far below that of the communication industry. Despite the fact that I disagree with the reasons given by Dr. Mansfield for this fact, I do agree with him that new attempts should be made to determine whether the original disappointment represented only a higher price of admission than we had any reason to believe initially.

Dr. Mansfield made another point with which I take issue. In his original text he stated, "moreover, sellers sometimes exert significant political power in an attempt to influence the choice [by the Department of Defense] of weapon systems and the need to maintain an industrial mobilization base together with political pressures make it likely that contracts will be awarded without strict attention to past performance." I must agree that the past performance of a contractor had not been given in the past sufficient weight. This is why a program aimed at gathering information upon the performance of its contractors was initiated in the middle years of the previous administration. But it does take time before the data are gathered. Today we do not have a satisfactory and unbiased evaluation of the performance of each contractor sufficient to justify its use in the case of an award.

I have often heard people ask "why such and such a company did such and such a bad job and in fact got themselves another contract." When this remark was analyzed, it turned out that it was not clear that such and such a company had done such a bad job, and in fact in some cases the contract had been awarded to a completely different division of the same company where the relationship between the two was very slim. I particularly protest Dr. Mansfield's implication that political pressures and the power of major suppliers have been able to swing awards in the direction of companies whose past performance was less than satisfactory.

Dr. Mansfield at some point argues that research and development should be divorced from production and performed by not-for-profit research institutes and government laboratories. Dr. Mansfield supports this statement and I have studied it for a long time. I have found that the idea, if applied to the industries where technological content is high (and this is the only case where sufficient R and D is available in nonindustrial entities), is obsolete and not worth debating. It will fail the first time anyone seriously tries it. I have recently had the opportunity to discuss a particular situation in my company, where the development and production are separated. I believe that, despite the fact that we are one company, we will have no choice but to unite them if we want to make the combination an effective competitive tool.

The last item to which I will disagree with Dr. Mansfield relates to the statement that a contractor has considerable advantage in the negotiating of target costs and that CPIF target costs may be higher than CPFF target costs.

Clearly, Dr. Mansfield has not had the opportunity to experience the competitive environment of present DOD, AEC, or NASA awards. I can assure him with experience on both sides of the fence that he is incorrect. My concern has always been that the contractor may tend to set his targets too low, so as to obtain a contract and lull himself and the government in the belief that the job can be done at such a price even if, with increased costs, his fee will be decreased. By and large the average defense contractor is more interested in backlog than in profit, especially when dealing with cost reimbursement contracts.

THE PRODUCTION AND USE OF ECONOMIC KNOWLEDGE

ECONOMIC RESEARCH SPONSORED BY PRIVATE FOUNDATIONS

By RALPH L. NELSON
Queens College, City University of New York

The importance of private foundations in the production of economic knowledge is generally conceded. Examples abound of foundations that have played Maecenas to neoclassical and Keynesian theory builders, to linear programmers and games players, to data-makers and data-takers, and to other laborers in the economic vineyards. Whether given to research institutes for large-scale, long-range projects, to groups of university scholars, or to individuals for the leisure to think, foundation grants often have meant the difference between unfinished exploration and discovery.

I make this assertion with confidence, not only because I believe it to be correct, but also because it presently can be neither precisely refuted nor confirmed. As the paper will repeatedly demonstrate, we as yet do not have the data, much less the tools of analysis, for precise measurement of effects. I therefore have taken as my assignment the marshaling and presentation of readily available data which, hopefully, might permit at least some rough measurement of the magnitude of foundation spending on economic research. To begin, I shall briefly summarize the level and trend of total foundation giving.

Most recipients of foundation grants may be pardoned if they are unclear as to the effects of foundations on their fields of interest. Large-scale foundation spending is so recent that its accumulating effects are still very much in the process of being realized. In the ten years since 1955, foundations spent between $8 and $9 billion on their programs; in dollar value this probably includes between two-thirds and three-fourths of all the grants ever made by foundations.

The recency of foundation spending derives from two developments. The first is the "population explosion" in foundations. Most of the explosion relates to the thousands of small foundations spawned in the 1950's and 1960's. However, there has also been a considerable increase in the number of the large ones that account for the bulk of grants. The history of the Ford Foundation is typical. Established in 1936, it received most of its endowment in the 1940's and started

spending at high levels in the 1950's. About twenty of the fifty largest foundations received their endowments more recently than did Ford.

The second reason for the rapid increase in grants is the growth in the large foundations' investment income. About four-fifths of the combined portfolios of the fifty largest foundations is common stock and of companies which, in the 1950's and 1960's, have usually done better than the general stock indexes. Prohibited from an unreasonable accumulation of income, the growth in dividends has been translated into proportionate or more than proportionate growth in grants. The Treasury has estimated that, in 1962, all foundations made grants and related outlays of $1 billion. This compares with probably $175-$200 million in 1950 and $70 million in 1930. Investment income for the Ford Foundation alone rose from $32 million in 1951 to $147 million in 1964; its expenditures rose from $32 to $216 million.

To what degree has economics participated in the increasing affluence of foundation grantees? The systematic collection and classification of foundation grants is still in its early stages and incomplete and tabulations go back only through the 1960's. The most comprehensive continuing tabulations are those of grants indexed by the Foundation Library Center. These indicate that for the period 1962-65 economics and business received about 1 percent of total indexed grants (Table 1). The grants index understates the flow of money into all fields, and the understatement for business-economics is probably higher than average. If so, Table 1 probably understates business-economics' share of total grants. It is doubtful, however, that business-economics has captured more than, say, 2 percent of recent grants.

While every red-blooded economist would contend that this is less than its rightful share, business-economics appear to have done about as well as the other social sciences. Indexed grants to government-law in this period were slightly more than those to business-economics while grants to anthropology-sociology-psychology were about the same. In the face of massive support from various government foundations and agencies as well as from broad public donations, the physical and life sciences attracted two and one-half times the private foundation grants that went to the social sciences. Surprisingly, indexed grants to the humanities were more than twice those to the social sciences.

It is probable that business and economics would have fared even less well in total grants had it not been for the special interest of the Ford Foundation. In the four-year period 1962-65, 3.8 percent of the Foundation's total grants were to economics and business, and the percentage was higher in the period preceding it. Most of these were listed under Economic Development and Administration (hereafter referred to as EDA)—that part of the Foundation's annual catalogue so closely studied by economics professors and business school deans.

TABLE 1

PRIVATE FOUNDATION GRANTS INDEXED IN *Foundation News*, 1962–65, BY FIELD OF ACTIVITY
(Dollar Values in Millions)

Field	Amount	Percent of Total	Percent of Total Social Sciences
All fields	$1,846	100.0	
Education	578	31.3	
International activities	336	18.2	
Health	299	16.2	
Welfare	192	10.4	
Physical and life sciences	151	8.2	
Humanities	142	7.7	
Religion	87	4.7	
Social sciences	61	3.3	100.0
Business and labor	9.9	0.54	16.4
Economics	8.2	0.45	13.6
Government	11.4	0.62	18.9
Law	9.5	0.51	15.7
Psychology	1.5	0.01	2.5
Sociology-anthropology	16.6	0.90	27.4
General	3.4	0.02	5.6

SOURCE: 1962–64: Foundation Library Center *Annual Reports*, 1963–65; 1965: Tabulation of 1965 issues of *Foundation News* prepared by Mrs. Lee Noe, Grants Editor, who generously supplied copies of her worksheets. The grants listings include the following explanation: "The records cannot be complete, for we can list only those grants about which we receive information either from donor foundation, public records, or from news reports. Grants of less than $10,000 and renewal grants will generally not be listed. Conditional grants or pledges will be omitted until we have knowledge of a payment."

A review of grants indexed by the Foundation Library Center also attests to the importance of the Ford Foundation in business-economics. Of $19.4 million of these grants reported in *Foundation News* through 1965, $13.4 million, or two-thirds, were made by Ford. It is likely, moreover, that Ford's actual share is somewhat higher. A sampling of the index suggests a larger understatement for Ford grants than for those of other foundations.

Because of Ford's importance, the EDA grant listings in its Annual Reports were examined in detail. As many of you know, the exercise can be fascinating. In the 1962 report, for example, one notes a payment of $600,000 to Brookings Institution classified, it seemed reasonable, under Research on Economic Problems. The payment was made from the unpaid balance of an earlier grant.

When and under what program was the original grant made? The investigation led back through the reports of 1961, 1960, and 1959 to the 1958 report, where mention of the grant disappeared from EDA!

But according to the 1959 report, there was a balance of $6.2 million owing Brookings carried over from 1958.

A number of intriguing conjectures came to mind; however, a more searching inspection of the 1958 report uncovered the wayward grant. Eight pages removed from the EDA listings and nestled between Education on the one side and Humanities and the Arts on the other was a category labeled National Institutions. Therein were listed three large grants to, respectively, Brookings, Resources for the Future, and the Lincoln Center for the Performing Arts. The association tempts me to ask Dr. Calkins whether Brookings has discovered an appeal to Ford that the rest of us might profit from knowing about.

TABLE 2

GRANTS FOR ECONOMIC DEVELOPMENT AND ADMINISTRATION AS PERCENT OF TOTAL FORD FOUNDATION GRANTS, 1954–65
(Dollar Values in Millions)

Period	Total EDA Grants*	Total FF Grants	EDA Grants as % of Total	EDA, Excluding Six Largest Grants‡	
				Amount	% of FF Total
1965 only	$ 6.5	$282	2.3	$ 6.5	2.3
1963–65	26.9	719	3.7	19.9	2.8
1960–62	25.5	529	4.8	20.0	3.8
1957–59	35.0	332	10.5	20.9	6.3
1954–56	18.0	196†	9.2	§	§

* Adjusted to exclude noneconomics grants included in EDA and two large economics grants listed elsewhere. See footnotes to Table 3.
† Excludes $476 million in 1956 capital grants to colleges, hospitals, and medical schools.
‡ The six grants are listed in footnotes to Table 3.
§ Not included in comparison.

Ford's EDA program got under way on a large scale in 1954 and 1955; the trend from then to the present is described in Table 2. Interpretation is complicated by the nonperiodic appearance of very large grants, such as the $7 million Resources for the Future grant of 1963. However, whether the large grants are included or excluded, the time pattern is essentially one of a flat trend in dollar value.

Flatness in trend is not usually reassuring and becomes positively disturbing when it spans a period over which the Foundation's total grants increased fourfold. Measured with or without the large single grants, the decline in EDA's share has been sharp—some might say precipitous. In 1964 the Foundation reported its grants under ten major subject areas; among the ten EDA came in a weak tenth.

The decline in EDA's share of total grants raises the question of Ford's future plans for this program. The annual reports are largely noncommittal on this point, aside from recent ones that warn that sup-

port of business education is in its terminal phase. However, two basic policy reports issued by the Foundation may provide some clues.

In the Report of the Trustees issued in 1950 the first detailed statement of basic policies and programs was made. Five major areas for action were blocked out and economic problems was one of them. The approach in economics was described as follows: "The Ford Foundation will support activities designed to advance the economic well-being of people everywhere and to improve economic institutions for the better realization of democratic goals." The details of this major program were spelled out and their economic content was substantial. Seven specific kinds of activity were itemized, and were inclusive enough to warm the heart of almost any economist. These were (paraphrased for brevity): (1) achieving a growing economy with high output and employment and a minimum of destructive instability; (2) achieving greater equality of economic opportunity; (3) improving our economic organizations, such as business firms, industries, and labor unions; (4) more satisfactory labor-management relations; (5) attaining an effective balance between freedom and control in economic life; (6) improving living standards throughout the world; (7) raising the level of public economic understanding in the United States.

In 1961 the trustees undertook their first comprehensive review of the Ford program since 1950 and in July, 1962, issued a report entitled, "The Ford Foundation in the 1960's." The report recognized "emerging forces since the 1950 report" that have influenced the Board's reflections on the Foundation's future course. Economic problems were neither specifically identified as an "emerging force," nor, as I read the report, were they very prominent by implication. Instead, the forces leading to changes in the Foundation's future course were (again paraphrased for brevity): growing demands on the American education system; greater recognition of need for higher standards of individual behavior and concern for the quality of life; increasing urbanization of American population; impact of modern weapons, space exploration, and science on domestic and international affairs; growth of federal government in research, foreign aid, education, health, and the arts; rising aspirations and energy of less developed countries; rapid growth of world population; growth of Soviet and Communist Chinese power; unification of Europe and movement toward Atlantic partnership; knowledge explosion and acceleration in the process of change.

The report goes on to say that, in the 1960's, the Foundation will act in five principal areas. These are: Educational Affairs, International Affairs, Public and Economic Affairs, Overseas Development, and Arts and Sciences. Economics is no longer separately identified as a

principal area; rather it shares its category with public affairs. Moreover, within this category the report mentions six fields of activity, only one of which has predominantly economic content. The six are Democratic Institutions, Problems of Urban Growth, Youth Development, Economic Growth, Population Problems, and Private Philanthropy.

Finally, there appears to be some change in the direction of interest within its economics program. The 1962 statement is less detailed and specific than that of 1950, with no repeat mention made of specific objectives except the one of stable economic growth with high employment. Two specific new objectives are mentioned, however:

> *Economic Growth:* The Foundation will help foster the achievement of a growing economy characterized by high output, the highest possible level of constructive employment, and a minimum of economic instability. It will help advance fuller utilization of human resources and the adaptation of technical education and retraining programs to meet the requirements of automation, structural unemployment, migration, and other change. It will seek to assist effort designed to keep the United States economy competitive and in a position to provide worldwide leadership. It will continue to support research in economics and business administration.

The statement suggests that the emphasis might be somewhat more on applied and policy research than in the past.

Ford's clear de-emphasis of business and economics in words and in deeds raises questions to which economists might give serious thought. Some consolation may be derived from the indications that support for basic research and fellowships is to be sustained in some fashion and that the change in policy represents mainly a shaking-down of the EDA program. However, the Foundation's statements go well beyond this, and appear to reflect a major demotion of economics in its scale of priorities. At the very least this development should stimulate some stocktaking by the profession, to which additional clarification by the Foundation could constructively contribute.

Whatever the degree to which there will be a Ford in our future, the Foundation's support for business and economics in the recent past has been unprecedented and in some areas possibly decisive. To provide some indication of its direction and effect, I shall examine the content of Ford's EDA program from 1957 to the present. The annual reports did not classify grants by subject area until 1957, and time did not permit a grant-by-grant classification for earlier years. However, the nine years 1957-65 include $87 of the $107 million in grants from 1951 through 1965 and cover the most recent nine of the Foundation's fifteen years of activity in these areas.

The trend in the distribution of EDA grants is given in Table 3. The categories used are those under which grants are listed in the annual reports. A grant is classified by the category under which it appeared in the year the grant was approved and first reported. This is not nec-

FORD FOUNDATION GRANTS TO ECONOMIC DEVELOPMENT AND ADMINISTRATION, ADJUSTED, 1957–65
(Dollar Values in Thousands)

Year Ending September 30	Total Adjusted EDA Grants*	EDA Grants as % of Total FF Grants	Economic Development, Human, Natural Resources†	Research on Economic Problems‡	Education for Business§	Fellowships¶	Training in Economics**	Other††
1957–65	$87,345	5.5	$26,555	$21,647	$21,387	$10,331	$5,109	$2,386
1965	6,454	2.3	2,361‡‡	1,105	613	1,429	300	716
1964	5,589	2.5	307	1,266	2,267	1,450	134	165
1963	14,845	7.0	10,027§§	0	2,731	1,438	394	255
1962	8,843	4.0	1,420	1,874	3,076	1,474	999	0
1961	6,859	4.8	0	3,272†††	900	1,436	1,251	0
1960	9,769	6.1	3,870¶¶	838	3,376	935	¶¶¶	750
1959	11,907	10.9	2,457	4,391‡‡‡	3,630	745	684	0
1958	17,982	22.8	6,113***	7,103§§§	2,905	721	640	500
1957	5,097	3.3	0	1,798	1,889	703	707	0

* Excludes grants in EDA classified under Population Studies, Population Problems, and Problems of the Aging. Includes two large 1958 grants (Brookings, RFF) classified under National Institutions and not in EDA. Does not include refunds, which were about ⅕ of 1 percent of total grants.

† Includes grants listed under the following categories: International Economic and Business Training, Comparative and Development Economics, Problems of Less Developed Countries, Training and Research on Less Developed Economies, Overseas Development and Economics, Economic Growth and Manpower Resources, Human Resources, Development of Human Resources, Careers For Minorities, and Economic Growth and Resources.

‡ Variously called Research on Economic Problems, Research and Training in Economics, and Research in Economics.

§ Variously called Education for Business and Business Education.

¶ Almost entirely predoctoral, dissertation, and faculty research fellowships.

** Variously called Graduate Training in Economics, Economics Education and Training, and Economic Education. Includes grants totaling $2,025,000 to Joint Council on Economic Education to advance economic understanding and $822,000 to National Educational Television and Radio Center for nationwide television course in economics.

†† Includes National Monetary Policy, Public Monetary and Fiscal Policy, Health Economics, and International Economics.

‡‡ Grants for 1965 are listed under Careers for Minorities and are directed toward the advancement and attraction of Negroes to business, managerial and executive training for Negroes, and the preparation of Negroes for labor union apprenticeship tests.

§§ Includes $7 million grant to RFF.

¶¶ Includes $3.5 million grant to Stanford for training of business leaders from less developed countries.

*** Includes $5.4 million grant to RFF.

††† Includes $2 million grant to Yale for center for quantitative study of national economies.

‡‡‡ Includes $2.5 million grant to NBER for general support of research program.

§§§ Includes $6.2 million grant to Brookings for research, education, and information on major public policy issues and problems.

¶¶¶ Included in Research on Economic Problems in 1960.

SOURCE: Ford Foundation Annual Reports. The Foundation's Office of Reports generously supplied back issues of annual reports and an advance

essarily the same as the categories under which payments on the grants are reported, in cases where payments are scheduled over a number of years. Categories have changed from one year to the next, the changes usually reflecting the particular focus of the current year's grants. New classifications frequently were established and the combination of old ones was common. An example is provided by the five grants, totaling $723,000 to Chicago, M.I.T., Princeton, RAND, and Yale for the analysis of economic growth and technology. In 1963, the year the grants were made, they were listed under Economic Growth and Resources. The 1964 and 1965 payments on the grants, however, were reported under Research on Economic Problems. One might speculate that the bumper crop of production functions that these grants nourished caused the Foundation to reclassify the grants. A more likely explanation is that new grants in 1964 and 1965 had little economic growth content and so this was not made a separate category.

In another direction, the EDA section of the report does not include many significant grants for foreign economic research and reported under Overseas Development. Two examples are the 1960 grant of $740,000 to the National Council of Applied Economic Research of India for "applied research on economics and management," and the 1958 grant of $269,500 to the University of Bombay for "teaching and research in monetary and international economics." Unfortunately time did not permit me to examine the extent to which the Overseas Development programs may have replaced the "domestic" EDA program in the Foundation's total support of economics.

For these and other reasons the trends in the broad categories at best provide only a very general indication of the changing content of the EDA program. The classification of grants for the year in which they were made was consistent, however, and so Table 3 can be used to summarize trends in the major components of the program.

To get more directly at the economic research content of the EDA program, the lists of grants were examined in some detail and an attempt was made to classify grants by economic research content. Time did not permit a grant-by-grant examination. However, even with more time many of the classifications would have been equivocal. Grant descriptions in the reports are usually brief and provide only a general and often conflicting impression of the economic-vs.-noneconomic, basic-vs.-applied, empirical-vs.-theoretical, and research-vs.-action program content of the grant.

The range of objectives in the EDA program is wide, however, and this permitted broad classifications of grants on a "wholesale-lot" basis. The results of this classification are presented in Table 4, which I hasten to label as tentative. The main categories are necessarily (and deliberately) qualitative; if he so desires, the reader may assign per-

TABLE 4

ECONOMIC RESEARCH CONTENT OF FORD FOUNDATION EDA GRANTS, 1957–65

(Dollar Values in Millions)

	Amount	%
Total	$87.34	100.0
Grants with No Significant Proportion of Economic Research		
Graduate Business School Development	7.53	8.6
Stanford Training of Business Teachers from Less Developed Countries	3.50	4.0
General Business Training Not Listed Elsewhere	2.67	3.1
Business and Vocational Careers for Negroes	2.30	2.6
Joint Council on Economic Education—Economic Understanding	2.03	2.3
Economic Education for Labor Union Members, Clergy, Foreign Businessmen, and others	0.88	1.0
Nationwide Radio and Television Course in Economics	0.83	1.0
	19.74	22.6
Grants with Moderate Proportion of Economic Research		
Resources for the Future—Research and Education in Natural Resources	12.40	14.2
Brookings—Research, Education, and Information on Public Policy Issues	6.31	7.2
General Business Research Not Listed Elsewhere	2.43	2.8
Training of Economists from Less Developed Countries	2.40	2.7
	23.54	27.0
Grants with Substantial Proportion of Economic Research		
Research on Finance, Management, Marketing, and Organizational Behavior	5.65	6.5
Business Predoctoral, Dissertation, and Faculty Research Fellowships	5.60	6.4
Western Management Science Center	1.30	1.5
Commission on Money and Credit	1.30	1.5
Resources for the Future—Research and Training in Urban Economics	0.90	1.0
Application of Mathematics to Business	0.88	1.0
Research Seminars for Economics Teachers from Liberal Arts Colleges	0.88	1.0
	16.51	18.9
Grants with High Proportion of Economic Research		
University Based Economic Research Not Listed Elsewhere	9.54	10.9
Economics Predoctoral, Dissertation, and Faculty Research Fellowships	3.30	3.8
National Bureau of Economic Research	2.87	3.3
Brookings—National Research Program in Taxation and Public Expenditures	2.40	2.7
Economic Research Institutes Not Listed Elsewhere	2.21	2.5
Yale Center for Quantitative Study of National Economies	2.00	2.3
Graduate Workshops and Research Seminars in Economics	1.33	1.5
Rotating Research Professorship in Economics	0.50	0.6
	24.15	27.7
Not Allocated	3.40	3.9

centages of research content in each case. Further, I fully admit that my ratings must reflect my own preconceptions. I have tried to avoid defining economic research as being "that which researching economists do." Beyond this, however, the classification has been guided by no very consistent application of taxonomic principles.

The table is largely self-explanatory and suggests that only a minority of grants went to the direct support of "hard-core economic research," however one may wish to define it. Even on a relatively broad definition, however, I would not be inclined to put the economic research content at more than 30-35 percent.

Rather than summarize the table in detail, I shall discuss my reasons for making several of what were in my mind the more uncertain assignments. In this I hope that my biases may be made more apparent.

First, the assignment of the $12.4 million in grants to Resources for the Future for "Research and Education in Natural Resources" to the "moderate proportion" group. While economics provides its central orientation RFF is necessarily heavily engaged with noneconomic research, particularly with natural resource technology, geology, forestry, water resource development, and the like. A separate large grant for research and training in urban economics was assigned to the "substantial proportion" group.

The assignment of the $6.3 million in Brookings grants for "research, education, and information on public policy issues" to the "moderate proportion" group was equally uncertain. In addition to economic studies, Brookings engages in large-scale government and foreign policy studies and has a large educational program (albeit one with heavy research orientation). An admittedly hasty review of its annual reports left me with the impression that a significant part of its economic studies program was supported by foundations other than Ford. The large Ford grant for its studies of government finance was assigned to the "high proportion" group.

The assignment of "Research on Finance, Management, Marketing, and Organizational Behavior" to the "substantial proportion" group involved two unequally offsetting judgments. The first and weaker one was that business research only partly carries over into economics. The second and stronger was based on the fact that the grants were large grants for long-range research to Berkeley, Carnegie Tech, Chicago, Harvard, M.I.T., and Stanford, averaging about $900,000 per school. The recipients are among the leaders in the application of mathematics, economics, and the other behavioral sciences to business. It was felt, therefore, that the grants could have a significant influence on the development of economics.

Finally, the table provides little indication of the subject area content of the "hard-core economic research," wherever it is present in the tabulation. Additional refinement of categories is desirable and would be particularly fruitful in the $9.54 million category described as "university-based economic resarch not listed elsewhere."

The paper should not omit at least passing mention of the importance of other foundations in the support of economic research. *Foundation News* through 1965 records sixty-one foundations that made at least one grant to economics or business. Of these each of eleven made grants totaling $100,000 or more. This is probably a minimum estimate because, as shown above, grant listings are not complete.

While I won't attempt to demonstrate it empirically, I would predict that grants from the other large foundations that support economics and business have relatively greater research content than was found for Ford. If so, then their share of the support of economic research is higher than their one-third or lower share of general economics and business grants reported earlier.

The distribution of large foundation support for the National Bureau of Economic Research tends to support this view. In the period 1957-64 it received $4.92 million in grants from seven large foundations. Of this grants from the six foundations other than Ford amounted to $2.05 million, or 42 percent of the total. Were a parallel examination made of Brookings' economic studies I suspect that one might find a similar pattern.

I suppose that if this paper has shown anything, it is that we as yet have only the most imprecise notions of the size and direction of foundation support for economic research. It has also shown that decisions have been made and will continue to be made which cannot help but have significant effects on foundation spending on economics in a period of rapid growth in foundation activities in all fields. The challenge and the opportunities in the situation should be clear to all.

THE PRODUCTION AND USE OF ECONOMIC KNOWLEDGE

By ROBERT D. CALKINS
Brookings Institution

This paper is intended to discuss only a fragment of the sweeping subject suggested by the assigned title. It is limited to some observations on the use of economic knowledge in the conduct of public affairs.

In his presidential address last year, Stigler advanced rather incidentally the thesis that the influence of economists and theory upon the formulation of policy had usually been small [1, p. 12]. Certainly few of us can be satisfied with the knowledge we possess for policy guidance, or with the record of explicit policy achievements over the years and across the wide range of policy problems confronting the nation. Yet somehow the ideas of economists have had a vast and increasing influence on public affairs. That influence, often with a notable lag, has been felt on central banking, monetary policy, fiscal policy, social security, tax policies, wartime price and production controls, trade policy, international monetary policies, defense policies, defense management, foreign aid, economic development, and countless other programs. Both economists and their theory have gained in influence since the 1930's, especially since 1941, and even more since 1961. The enormous improvement of economic data available has been both a result and a means of extending that influence. Advances in economic knowledge have not only placed economists in positions to influence policy, but, because of it, the goals and methods of policy in many areas are far more precise and ambitious than they were a generation ago. What then accounts for the seeming contradiction?

The fields I have mentioned are among those in which economists have worked extensively. Although their studies may seldom have been designed directly for policy formulation, they have nevertheless influenced policy. Moreover, in these areas economists have exerted an influence in the councils shaping policy by taking an active part as advisers or officials. Stigler refers particularly to policy areas in which economists have done comparatively little work or work that is inadequate to guide important policy judgments. In addition he may be looking for a more direct line of influence than is ordinarily visible in the intricate process of policy formation—a process which has been little understood and often oversimplified.

Economists have not been notably successful in defining either the relationship of economics to policy or their own conduct in making policy recommendations. For a century and a half the belabored discussions of the application of economics to policy have been devoted more to a clarification of economics as a positive science and its logical relation to policy proposals than to a clarification of the process of application or of policy formation. Smith was scientist, analyst, and policy advocate before these roles were distinguished. Malthus and Say wanted a political economy yielding rules for practical action, while McCulloch wanted it to specify the most effective means of production, accumulation, and consumption [2, pp. 38-42].

Senior and J. S. Mill more perceptively distinguished the science and the art of political economy. Senior looked to the science to ascertain the institutions most favorable to the production and distribution of wealth, but changed his mind a decade later and held that the application depended on so many other branches of knowledge that it belonged to the art of government, and that the political economist should not "recommend or dissuade," or add a "single syllable of advice" [2, pp. 48-49]. But this conclusion exercised no visible restraint on Senior in giving advice, and a decade later he returned to a modified version of his original position.

Mill elaborated the role of the science and the art in his 1836 essay [3, pp. 120ff.] and in the last chapter of his *Logic* (1843). Science was the study of what is, while art is practice based on rules and precepts provided as theorems by the corresponding science. In brief, art proposes an end and looks to science to specify the precept for most effectively achieving it. But the art depends on many sciences, and the ends have to be specified according to some general premise as to the proper objects of approbation and precedence—which, in his view, should be the happiness of mankind. It is not clear how seriously Mill expected a general body of rules, precepts, maxims, and principles to be developed for application. In any event the task he assigned to the future was not carried out. Nor in his *Principles* (1848) did Mill follow his own prescriptions, for he passed from science to application with little regard to the differences he had enunciated.

In his *Scope and Method* of 1890 J. N. Keynes emphasized the separation of the positive science from the art based on precepts for the attainment of given ends, and the normative science which was concerned with ideals and criteria of what ought to be. He urged the new less normative term "economics" for the science. He considered the precepts for the art to be so conditional under varying economic, ethical, social, and noneconomic considerations that he regarded the development of a body of precepts or doctrine for practice to be unat-

tainable. Instead he proposed an "applied economics" that would make the significance of economic theorems known to the legislator and social reformer. He doubted whether the normative science of ideals could properly be called even the "ethics of political economy," because it drew so heavily on other fields of knowledge [4, pp. 50-83].

As Hutchison indicates, Keynes's concept of a positive, neutral science clearly distinguished from policy recommendations requiring ethical or political judgments was widely, though by no means wholly, accepted by English and American economists [5, p. 38]. In suggesting the application of economic theorems to particular problems, rather than try to formulate a body of economic precepts, Keynes was perhaps anticipating more than influencing the course of development.

The increasing specialization of economics with the growth of literature in the so-called "applied" fields—a literature describing institutions, practices, policy, and history, along with a review of theory and practical problems—doubtless contributed toward a more specialized approach to policy. Specific problems and reforms—in antitrust, banking, taxation, and tariffs, for example—came to be studied more than the big issues such as the institutions most conducive to the production and distribution of wealth and those concerned with the scope and role of government action.

Meanwhile the quest for economic criteria for policy purposes continued. From utility theory and welfare economics came concepts of optimum utility, the social optimum, maximum social welfare, and economic welfare as ideals or criteria. These concepts, in spite of their imperfections for broad political and social decisions serving diverse social objectives, have had a major influence on the criteria of choice among economists [5, pp. 40-50, 143-70]. They have yielded operational concepts of optimizing in management situations and stimulated the analysis of alternatives and methods of choice based on cost effectiveness. Such methods have been improved by operations research and systems analysis to which statistics and mathematics were important contributors. For the larger problems of social choice the "new" welfare economics of more recent years proposes a social welfare function as the criterion for choice among alternatives. But although the approach provides useful concepts, it is still far from operationally feasible for most social policy decisions.

The recent dramatic changes in defense policy and management, however, are a direct outcome of applying economic concepts and methods to defense problems. The application initially was developed largely by RAND [6] [7], and the transfer of such economists as Hitch and Enthoven to the Department of Defense was important to the outcome. The concepts of optimizing, efficiency, cost effectiveness, comparative advantage, marginal utility and marginal costs, systems

analysis, trade-offs, spillover effects, and other tools of economics go to the heart of the reforms in program planning, performance budgeting, and decision making [8]. In those areas Hitch sees economic choice as a way of looking at such problems. He comments that:

> . . . the choice of a particular military strategy or military objective cannot be divorced from the cost of achieving it. Systems analysis at the national level, therefore, involves a continuous cycle of defining military objectives, designing alternative systems to achieve these objectives, evaluating these alternatives in terms of their effectiveness and cost, questioning the objectives and other assumptions underlying the analysis, opening new alternatives and establishing new military objectives, ...
>
> Thus, the problem of allocating resources within the Department of Defense itself involves the choosing of doctrines, weapons, equipment, and so forth, so as to get the most defense out of any given level of available resources or, what is logically equivalent, to achieve a given level of defense at the least cost. Approaching the problem from the first point of view—getting the most defense from a given level of resources—we work in terms of marginal rates of transformation and substitution. Approaching the problem from the second point of view—achieving a given level of defense at the least cost, . . . —we work in terms of marginal products and marginal costs in order to help the top decision-maker choose the appropriate level of resources [8, p. 52].

And these methods are not limited to the Defense Department. With economists more prevalent in the Bureau of the Budget in the past few years, such methods are spreading, and cost benefit analysis, though still ill-developed and roughly applied, is becoming a useful tool in the appraisal of programs [9].

The process of decision making according to Simon generally involves defining the problem, clarifying objectives, developing alternative courses of action, analyzing their consequences, and appraising them in light of the objectives and constraints [10]. Imperfect though the method is, and though "satisficing" more than maximizing may be the outcome, the approach has the merit of introducing more deliberate analysis and appraisal than the customary intuitive methods. Lindblom is critical of such synoptic approaches, including the more formal social welfare function approach of Tinbergen [11] on the ground that they cannot logically achieve the rational ideal they aspire to [11, pp. 27-57]. Among other defects they cannot consider all alternatives and consequences or provide an evaluative method either from a rational deductive system or a welfare function [11, pp. 48-51]. Nevertheless, with all its limitations in a world of imperfect knowledge, economic, political and social change, and uncertainty both with respect to the present and the future, the method, in less pretentious form than the description of it might suggest, has acquired substantial use, and it allows the economist to employ a number of his analytical tools.

Lindblom finds a wide discrepancy between what social scientists and policy analysts say they do and what they do in fact under the foregoing conditions [12]. He concludes that most policy making is incremental. The decisions introduce modest changes, that are not final

or irrevocable, but adaptive and subject to change with experience or new conditions [12, pp. 61-79]. Problems are often only an ill-defined discontent with an existing or prospective state of affairs. Ends are not given; in fact both the ends and means of policy must be devised and offered for acceptance. He describes a strategy of disjointed incrementalism as one way in which policy evaluations and decisions are in fact made. This is a procedure whereby only incremental differences in conditions or consequences among a limited number of alternative policies are made the basis of choice. The effort is to improve, not necessarily to maximize. Policy proposals originate in many centers and are subject to public criticism and modification. Political choice is, as always, a compromise [12, pp. 81-142]. It is an untidy procedure, but, like democracy, it works after a fashion.

The political process itself involves not only the formulation of policy recommendations but the achievement of legislative approval, and this requires both support from organizations and individuals and campaigns to inform and enlist public backing. There are probably more middlemen between those who initiate a public policy proposal and the enactment of a vaguely similar policy than in any other productive endeavor. Bailey, in his revealing account of the passage of the employment act of 1946, describes the process as "a kaleidoscopic and largely irresponsible interplay of idea, interests, institutions, and individuals" [13, p. 240]. The passage of the tax bill of 1964 offers no notable improvement upon that record. As Flash has shown [14], the Council of Economic Advisers not only offers advice, but to make that advice effective it plays both an educational and a political role.

Most uses of economic knowledge have some eventual and indirect influence on policy. Through education the economist perpetuates the flow of professional economists for teaching, research, and practice who in time exercise their influence. He also exposes a number of nonprofessional students to some familiarity with economic concepts and methods. How well this fragmentary knowledge permits students in later life to comprehend economic developments, policy issues, and the operation of the system is questionable, especially with the erosion and obsolescence of knowledge in later years. But some knowledge is doubtless better for the nation than none, though it is increasingly apparent that some substantial form of adult instruction is necessary as a means of reducing the social lag.

Through research the economist plays a vital role by contributing new concepts, methods, and theories. He also plays an important role in providing empirical research and policy studies. The increasing attention being given to social costs and benefits, welfare criteria [15] [16], growth and development, model building, the refinements of

theory, and techniques of analysis promises to have more usefulness for policy making than many now suppose. The economists outside government can often contribute such research more effectively than those in government can under the pressures of daily duties. In fact, economists in government look to the profession for these contributions, for the critical appraisal policies, for alternative policy proposals, and for creative ideas. The range of policy problems on which research is needed has grown steadily: (1) in policy areas for which more refined knowledge is needed, for example; growth, unemployment, cost-price relationships, etc.; and (2) large new areas such as poverty, education, health, and scientific research, where the analysis of the economist is urgently required.

The notion that the literature is not read or used is largely an illusion. If it has relevance, it is considered by the practitioners who can use it. The fact that the professional literature is not read by the public or large numbers of influential people has little more significance than that the professional literature of doctors, lawyers, or many other professional groups is not read by a mass audience. The number of economic practitioners has grown sufficiently to provide a link between research and policy. To these practitioners falls the growing task of translating technical knowledge for the decision-makers.

The economist outside of government is not limited, however, to a "come and get it" approach. Increasingly those with contributions to make in policy matters serve on committees and task forces and as advisers. Some of the most effective have been those who have served in government where they could exercise an influence directly on policy formation.

Just as there are types of research that can better be performed outside of government, so there are other functions which must be carried on in government, often with help of outside advisers. The first of these functions is interpretation of conditions and events. By interpretation is meant more than an explanation of how or why a situation developed or an event occurred. To the decision-maker it is equally or more important to know the import for the future—what is likely to follow as a consequence, and what could occur under one set of contingencies or another. Information on the balance of payments, price increases, the threatened independence of Rhodesia, and contemplated countermoves, for example, raise questions not as explicit as what will happen, but what is probable and what is possible under various feasible contingencies? This sort of interpretation is far more an activity for oral discussion or internal memoranda than for public print. The economist, along with those familiar with the relevant business, political, and social factors, is being drawn increasingly into this form of

analysis of possible consequences in prevailing or contingent situ tions. For the decision-maker these interpretations are essential appraising probabilities and contingencies to which action must b adapted and timed.

Finally, in the formulation of economic policy the economist pla his role as interpreter, as analyst, and as advocate. He defin problems, clarifies objectives, develops alternatives, analyzes the effects and side effects, and seeks ways to provide for contingencie He deals with multiple objectives, constraints, and uncertainties. H confers and considers the reactions and interests of other agencies, an seeks support. Again much of this work is conducted orally or throug staff papers and memoranda. But throughout, the economic practition is using his knowledge of economics.

It has been indicated that economists and economics have come play a far more important role in public policy making than they did generation or more ago. The probability is that this influence will in crease rather than diminish with the expanding influence of govern ment on economic affairs and the quest for more effective policies. Th problems of stability and growth, poverty, education, health, urban ism, social investment, foreign aid, taxation, resource use, foreign ec nomic policy, international finance, agriculture, for example, will co tinue to require the attention of economists, government, and the pub lic. The worldwide problems of economic development, population, foo supply, and institution building will continue to tax the economist knowledge and his ingenuity in policy formation.

The need for research and education has never been greater, and y there are in this author's view a number of improvements that woul facilitate both the advance of economic knowledge and its more effe tive use. Clearly we need to know more about the economic process well as more about the process of applying economic knowledge bot in practice and as a rational ideal. There is need for more rigorou standards of conduct among practitioners offering professional analys and advice. The practice of analyzing alternatives and consequenc would benefit from more rigor and less influence from personal valu judgments. More graduate seminars devoted to policy analysis woul advance these objectives. But it seems clear that additional centers fo economic research, including policy research, must be developed if th effort is to keep abreast of the expanding need for knowledge. The soci sciences now expend only about 3 percent of the funds for basic re search, and the figure is probably no higher if applied research is in cluded. The fraction for economics is approximately 1 percent. On must question whether this is a prudent allocation of resources in ligh

of the growing usefulness of economics and the multiplication of pro lems requiring economic analysis.

Finally, there is a serious problem of public understanding. As ec nomic problems are dealt with more technically, can the economist a tain the position of the doctor or lawyer whose advice is accepted faith? If not, by what means can policy issues and alternatives more effectively presented to the public and by what means can t public be given a more adequate knowledge on which to understa and react? This is one of the issues that seems likely to plague t profession and policy-makers for the foreseeable future. The prof sion must at least strive to warrant the confidence that its growi influence requires.

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TRENDS, CYCLES, AND FADS IN ECONOMIC WRITING*

By MARTIN BRONFENBRENNER
Carnegie Institute of Technology

I

Since my subject has been divided into "trends, cycles, and fads," I shall divide my own remarks in the same way. This opening section accordingly deals with trends in professional economic writing since Adam Smith's *Wealth of Nations,* now nearly two hundred years old.

In terms of subject matter, I cannot make out any trend less trivial than the horizontal. Neither can Professor Stigler, who has recently written [17, p. 16, running quotation]:

> The full range of subjects and problems which have attracted economists' attention throughout our history has been both extraordinarily wide and extraordinarily stable. True, some minor areas have been yielded up to younger sciences. True, some minor additions have been made to our present-day agenda. But in the broad, the boundaries of our discipline have not varied much.

Contrasting with this "all quiet" on the substantive front are two trends in methodology. The first trend has been toward emphasis on the formally "scientific" emphasis of what used to be called "political economy" and is now called "economics"—the change is significant. These scientific aspects include the abstract mathematical treatment of approximations or models, the empirical testing of economic propositions and their implications, and the accumulation of public and private data useful for model building and theory testing. I would not expect anyone in this audience to doubt the existence of this trend, dating rather from Cournot (1838) than from Smith himself. For statistical evidence, however, see Stigler's Table 10 (*ibid.,* p. 48). The question is whether certain of our fellow economists may not have elevated mathematical and statistical virtuosity to the status of ends in themselves. Some of us, even semimathematical types, are also concerned with the chance that our literary-humanistic-philosophical traditions may be headed for the ash can—meaning elementary textbooks, public documents, and the popular press. On this delicate point, I take an optimistic position. The formula, statistical table, and computer program have not yet displaced the written word from any of the natural

* Numerous perceptive comments were received on my original draft. Not all of them could be incorporated in revisions. Among individual commentators, I cite W. W. Cooper, Mark Perlman, Eugene Rotwein, and Leonard Silk.

sciences, even physics, astronomy, or, for that matter, mathematics itself. Why should economics be different?

A second trend which we all perceive is toward specialization. In Smith's day, political economy was a branch of "moral philosophy"—a field too interdisciplinary for even the Ford Foundation. Its twin bases were ethics and psychology. It also included political, sociological, administrative, and anthropological problems along with economic ones. Witness Smith's own *Lectures on Police, Justice, Revenue, and Arms*. (How many of us, I wonder, regret the demise of this venerable mish-mash?) Over the last two centuries, economics has not only achieved autonomy as a discipline, but it has become increasingly fragmented into subdisciplines. We have microeconomics, macroeoncomics, labor economics, international economics, and so on ad infinitum. The general economist may be traveling the Dodo or Passenger Pigeon Highway to extinction. He survives—and I am a survivor—as dilettante, journalist, or elementary teacher, or as an exotic variety of multiple-specialist whose several specialisms fall in more than one of our ordinary classificatory boxes. For two examples I might mention Abram Bergson's combination of welfare economics and the Soviet system, and George Stigler's combination of industrial economics, statistical economics, and doctrinal history. The general economist, however, is being replaced to some extent by the general theorist, transferable between specialties at short notice. His expertise includes aspects of microeconomics, macroeconomics, and often mathematical economics or econometrics as well. If the general economist corresponds to the medical G.P., the general theorist corresponds to the internal-medicine man.

II

Passing from trends to cycles, including long swings, I have been struck primarily by the flow and ebb of the strength of a changing something which is called "orthodoxy," and secondarily, by the flows and ebbs of the popularities of our various specialisms: "In 1830 no general work on economics would omit a discussion of population and in 1930, hardly any general work said anything about population The problem of economic growth was at the forefront of discussion in 1825, it was almost ignored in 1900, and today it is again *haute mode*" (*ibid.*, p. 16 f.). Both these ebb-and-flow phenomena, the appeal of orthodoxy and the popularity of specialities, deserve some attempt at explanation.

For a long time, I have found amateur Hegelian dialectic to be a useful hindsight in the history of economic thought [1, p. 167 f.] What seems to happen is this. As a result of a few great men and great issues, a consensus develops on a number of interrelated problems

After some years, this consensus ossifies into an orthodoxy, and progress decelerates under its dead hand. (In the Anglo-Saxon world, at least, 1850-70 and 1900-25 seem to have been two such periods.) This consensus or orthodoxy plays the role of a Hegelian thesis. Its antitheses may arise in three ways. They may arise, first, from policy problems to which the reigning thesis seems irrelevant or in which it points to unpalatable answers. (Major depressions and development problems have been relatively recent examples.) Antitheses may arise, second, from flaws and imprecisions in the reigning thesis, overconfidently formulated. (The "imperfect competition" revolution is a case in point here, and likewise the revolt against the conventional versions of Say's Law or Identity.) A third cause of antitheses may be technological and methodological developments, which the dominant thesis cannot utilize or assimilate to best advantage. (I have in mind input-output analysis and electronic computation as tools for forecasting and policy evaluation.)

Antitheses, in their early stages, are often emotional, often imprecise, and often more intolerant than the theses they are combating. If indeed "the ratio of cliché to analytical creativity in the literature of economic development is awesome to contemplate" [17, p. 26], that is only par for the course. What happens normally to antitheses is that they wither away at least temporarily, or else improve in quality to the point of either replacing or supplementing the original thesis in some new synthesis. This becomes in due course the thesis of the next generation and often also the orthodoxy of the generation after that.

As of the 1960's, the reigning economic thesis in the United States combines in uneasy peace elements of the previous Marshallian synthesis of the cost and utility theories of value and competitive price (with a variable adornment of imperfect competition), elements of the Keynesian theory of income and employment (embellished in varying degrees by the "neoclassical synthesis," alias "anemic counterrevolution") and elements of a neoclassical theory of economic growth and development. The ideological impact of this smorgasbord or chop suey is generally favorable to a mixed economy, primarily based on competitive private enterprise.

Two antitheses wait in the wings, however, and we may be on the verge of ferment at least comparable to the 1930's. The more threatening antithesis is the "linear" or "structural" economics of the Leontief input-output table [7], of Sraffa's schema of *Production of Commodities by Means of Commodities* [16], of a great deal of operations research, and of large-scale economic models, soluble with the aid of electronic computers, aimed sometimes at forecasting and sometimes at a degree of central economic planning which goes beyond the macroeconomic level. The ideological background to linear economics

may include strong Marxist infusions, particularly in socialist countries. In America, there are echoes of the technocratic institutionalism of Veblen and Ayres. Often, however, one observes no ideology more partisan than a practical engineer's desire for a system which promises to help him do his job and get his answers in a hurry.

It is more tempting than wise to laugh off some of the earlier manifestations of linear economics—technically or structurally fixed "laws" of this or that. Samuelson, for example, has commented ([15, p. 336], running quotation):

> [T]he fact that counter-trends have about canceled each other out for half a century can be regarded as a coincidence, and provides no guarantee of repetition in the future. [I have] learned how treacherous are economic "laws" in economic life: e.g., Bowley's Law of constant relative wage share; Long's Law of constant participation in the labor force; Pareto's Law of unchangeable inequality of income; Denison's Law of constant private saving ratio; Colin Clark's Law of a 25 percent ceiling on government expenditure and taxation; Modigliani's Law of constant wealth-income ratio; Marx's Law of the falling rate of real wage and/or the falling rate of profit; Everybody's Law of a constant capital-output ratio. If these be laws, Mother Nature is a criminal by nature. Experience has also taught me not to be necessarily suspicious of coincidence; in many cases, even if they do not explain the facts, they do describe the facts, up until they cease to describe the facts.

Two points at least can be raised in rebuttal. Even when deceptive in the long run, some or all of these "laws" are useful approximations for short-term problems. Furthermore, the long-term errors may be lessened by further work along essentially similar lines, just as Samuelson's own path-breaking statistical consumption function for 1921-35 [6, pp. 250-60] has been improved but not invalidated by a generation of further work.

So much for an unfairly brief comment on linear economics in its various forms, as an antithesis to our current orthodoxy. Another antithesis worthy of extended discussion comprises a number of developments I should like to force into a single "behavioral economics" category. In the theory of the firm, for example, I am referring to simulations of the way individuals or organizations behave, and in particular how they make decisions under conditions involving less knowledge than is postulated by orthodox theory. Many schools of business administration have become centers of this heresy. Carnegie Tech, my own institution, claims to be a leader among them [2, Part III] [3] [13, Part III, and Chap. 13] [14]. Moderate behavioralists, including my colleagues when nobody is baiting them, see in their work a desirable supplement to "conventional economics," involving more attention than most of us pay to current work on psychology and sociology. A few extremists, including my colleagues when you or I are nasty to them, propose to displace "conventional economics" by "economic sociology" or "behavioral economics" the day after tomorrow.

Another form of the behavioralist antithesis is technically quite

different. It concentrates on the growth and change of economies and economic institutions. It supplements (or replaces) "economic" growth theory by rival analyses based on the history, sociology, and psychology of individuals, small groups, classes, and nations. Let me cite the economic historian W. W. Rostow's *Stages of Economic Growth* [12], the social psychologist David McClelland's studies of "achievement motivation" in economic development, and the converted economist Everett Hagen's reliance on sociological and psychological factors in his *Theory of Social Change* [8] [5] and pass on. By stretching the term "behavioralist" somewhat, I can also put the great economic best-seller of the 1950's, Galbraith's *Affluent Society* [4], into this category, as well as much of the recent work of Gunnar Myrdal [10] [11], not to mention still more controversial figures like Robert Theobald and his fellow Triple Revolutionists [18].

How important will either or both of these antitheses, the "linear" and the "behavioral," become? How will they be synthesized with—or alternatively, what is their chance of replacing—economics as we know it today? With no basis for a priori judgment, let me meet these questions by posing as a pragmatist. Insofar as one or another of these antitheses "work" better than conventional economics, either qualitatively or quantitatively, in forecasting, in policy formation, or in explaining past history, we shall have to make peace with them, at the cost of discarding as irrelevant, if not wrong, some of today's most treasured nuggets of eternal verity. Insofar as the antitheses disappoint their advocates, or require slave-camp institutions to achieve their targets, let them "pass and be forgotten with the rest." (We can in any case expect periodic revivals under new names and forms in future generations.)

Perhaps one less than pragmatic warning may also be in point, against ignoring some useful bit of innovation or antithesis as "not economics." If it is not economics, so much the worse for economics, if it works better for economic problems! Conventional economists, including myself, can learn from events in the U.S.S.R., where progress in economic analysis was confined for a generation to engineers, mathematicians, and statisticians, because the professional economists down-graded themselves, by their own excessive orthodoxy, to clerks and propagandists.

III

Cycles of varying length and regularity can be discerned in the division of economists' interests among the several subdivisions of their subject, although nothing like Schumpeter's Kondratieff-Juglar-Kitchin tripartite classification meets the eye. Evidence from English-language professional publications is available since 1886 from the Ameri-

can Economic Association's *Index of Economic Journals* [9] in six volumes. A separate annual series, covering American doctoral dissertations only, can be assembled from the September issues of the *American Economic Review;* I have compiled it only for 1960-65, inclusive. Finally, Chapter 3 of Stigler's *Essays in the History of Economic Thought,* analyzes (pp. 44-50) the contents of four leading American scholarly journals (plus *Econometrica,* of international coverage) over a sample of academic years chosen decennially from 1892-93 to 1962-63.

It is easy for the statistical purist to discount this evidence. Of the *Index,* Stigler complains [17, p. 48] that one cannot easily "trace out, for example, the cycle of interest in monopolistic competition in the 1930's, the sweep of linear programming through the journals in the 1950's, and similar episodes," since "the categories are too broad, and the index itself recognizes only the categories . . . fashionable in 1960." Shorter movements are harder to trace than longer ones, and the *Index* classification of portmanteau or bridge contributions is inevitably arbitrary. How firm can a line be which separates, for example, the "Monetary Theory" of category 2.32 from the "Monetary Policy" of category 9.9? On the other hand, a more industrious content analyst than myself could subdivide the broader *Index* categories into the narrower ones which Stigler desires, and likewise into shorter periods of time.

The annual dissertation-subject breakdown, as has been said, covers only the United States. It is less purist than the *Index* in excluding "business administration" topics. Its 15-category classification is not easily made comparable with the *Index's* 23-category one. As a barometer of economist's interests, it is biased both by concentrating on younger scholars and on "manageable thesis subjects"—often paper-thin slices of, and virtuoso foot- and toe-notes to, the vast projects of academic entrepreneurs—such as provide the earliest and plushiest retirement to Santa Euphoria or the outposts of Pentagonia.

Stigler's tentative studies are intended primarily to illustrate the "potentially large scale for the statistical method in intellectual history" [17, p. 49]. Their most distinctive aspects are attempts to quantify the common generalization about the increasing level of technical proficiency required in economic publications, and the increasing independence of American economics from foreign-language sources. (*Ibid.,* Table 9, p. 47.) The first of these developments I have diagnosed as a trend, with the pious hope that it may have passed its inflection point and may even be approaching an asymptote. The second may possibly represent only a long swing, which may soon be reversed in some degree. (National and ideological considerations seem to

dissuade some foreigners from publishing in English. Our own professional interests are expanding geographically, to encompass increasing numbers of non-English-language areas, and our linguistic competence is beginning to keep pace. Existing translation processes, furthermore, are lagging increasingly behind our expanding needs.)

Let us return to the *Index of Economic Journals*. Table 1 and Chart I give a percentage breakdown of its citations by volume and major category from 1886 through 1963. Rather than counting articles, I have economized time by "measuring" them in column-centimeters, at the cost of overstressing the longer-winded titles. (The actual measurements were made by Paul Lavell and Yoichi Niwata.) This methodological difference explains, I assume, the discrepancies between my results and Stigler's (*ibid.*, p. 49).

Considering the *Index's* 23 categories without finer breakdowns, there are significant uptrends in 6. The most pronounced in absolute terms is in economic theory (category 2), which includes macroeconomics and dynamics along with the traditional range of topics in microstatics. What has happened, one would expect, has been successive waves of interest in the theory of the firm and the consumer, the theory of imperfect competition, the theory of income and employment, and the theory of economic growth. By the time one wave has ebbed, the next has overtaken it. There is a similar upturn in economic history (category 5), with some decline since 1960, and in two sets of tools: mathematics and statistics (category 7) and social accounting and statistical data (category 8). Business organization and managerial economics (category 14) and regional economics and housing (category 22) follow the upward path of economic history, including some slight decline since 1960, which can hardly be more than temporary.

Offsetting these steady, trend-like, increases, three categories show downward drifts of professional interest. Two are "theoretical" fields, methodology (category 1) and doctrinal history (category 4), at least since 1940. A shift away from humanistic and philosophical concerns among economists may be responsible here. Of the "substantive" fields, the sharpest decline is in industrial organization and public policy (category 15), which had furthest to fall.

There is a parabolic trend (or long swing) in monetary economics (category 9). Interest fell to a trough during and immediately after World War II and rose thereafter with postwar inflation and the "rediscovery of money." Health, education, and welfare (category 19) and population economics (category 21) show similar patterns, with the troughs coming later. Revivals can I think be traced in each case to "the real world," meaning expanded education and social-welfare programs in one case and the "population explosion" in the other. Parabolic trends (or long swings) in the opposite direction appear in pub-

TABLE 1

SUBJECTS OF PROFESSIONAL PAPERS IN ENGLISH-LANGUAGE ECONOMIC JOURNALS, 1886–1963
(Percentage Distribution*)

Category Number and Short Title	Vol. I 1886–1924	Vol. II 1925–39	Vol. III 1940–49	Vol. IV 1950–54	Vol. V 1955–59	Vol. VI 1960–63
1. Scope and method	2.84	1.49	1.96	1.78	1.43	1.29
2. Economic theory	8.42	10.90	10.74	15.04	16.58	16.77
3. Economic systems and planning	1.31	1.15	1.51	1.13	1.33	2.57
4. History of economic thought	5.10	3.81	4.47	3.80	3.61	2.78
5. Economic history	1.21	1.49	1.49	1.44	1.64	1.47
6. Contemporary economic conditions, policy, and planning	1.70	1.47	4.17	2.65	3.56	2.90
7. Mathematical and statistical tools	1.25	2.36	2.48	2.65	2.54	2.87
8. Social accounting and statistical data	2.16	2.13	2.23	2.50	2.99	3.55
9. Money, credit, and banking	10.11	8.45	5.30	5.38	5.82	5.89
10. Public finance	4.76	4.62	5.03	5.34	5.20	4.53
11. International economics	7.50	6.98	6.79	10.46	8.01	9.64
12. Economic fluctuations; stabilization policy	1.29	8.45	3.17	1.91	1.99	1.69
13. War and defense economics	4.26	0.40	9.18	2.12	0.53	0.51
14. Business organization; managerial economics	3.26	3.61	3.91	5.08	6.53	6.27
15. Industrial organization and public policy	17.75	16.03	11.86	11.99	12.78	11.73
16. Economics of agriculture	7.12	12.31	10.40	9.44	10.86	8.90
17. Natural resources; land economics	2.11	3.64	3.48	2.99	2.34	2.29
18. Population	1.42	1.41	0.75	0.93	1.23	0.95
19. Labor economics	11.55	5.10	5.86	8.92	6.82	8.05
20. Consumer economics	1.06	0.91	1.72	1.24	1.51	1.36
21. Health, education, and welfare	2.61	1.45	1.36	1.34	0.85	1.11
22. Regional economics; housing	0.81	1.75	2.09	1.77	1.78	2.78
23. Miscellaneous	0.42	0.08	0.04	0.08	0.06	0.10

* Percentages rounded; may not add to 100.00.
SOURCE: John Perry Miller *et al.*, eds., *Index of Economic Journals* (6 vols.).

lic finance (category 10) and land economics (category 17), including industrial location. Interest rises to peaks during the Great Depression and World War II, and, generally, when New Dealers are in power.

Shorter cycles are possibilities in half a dozen other categories. In comparative systems and economic planning (category 3), for example, there is a peak during and immediately after World War II, and then a larger one corresponding to the contemporary proliferation of development plans. The catch-all field of contemporary economic conditions and policies (category 6) also has two peaks, the larger one during the war and immediate postwar periods, the secondary one in the later 1950's. International economics (category 11) has three peaks, one including World War I and its aftermath, the second the

early 1950's, and the third the contemporary exchange and liquidity difficulties. Agricultural economics (category 16) has two peaks, one spanning the Great Depression and the other the late 1950's. Labor economics (category 19) has a generally falling trend of relative interest with two troughs covering the depression and the later 1950's. This may be largely a statistical artifact, reflecting a turn by labor economists to legal and industrial-relations journals. Consumer economics (category 20), on the other hand, accentuates a slightly rising trend with peaks during the price control period and, after data accumulation, in the later 1950's.

Finally, one can point to two sudden, and poorly maintained, spurts of interest, of the type identifiable as fads. The earliest in time is the drastic rise of interest in industrial fluctuations (category 12) during the Great Depression. The second is the similar spurt in war and defense economics (category 13) during the period of World War II, although a finer temporal breakdown might show a similar bulge during the period of World War I.

Searching for shorter cycles during the early 1960's, my assistants and I applied similar analysis to the American completed-dissertation topics listed in September issues of the *American Economic Review* for the years 1960-65, inclusive. Here we merely counted titles; Table 2 and Chart II are based upon this count.

As can be seen from our two tables, the *American Economic Review's* 15-category typology overlaps with the *Index's* 23-category one, which was adopted only later. The *Review's* categories also seem less coherent, less homogeneous, less readily defensible. The dissertation topics appear generally less theoretical, more commonly "applied" or "descriptive," than the published essays. Furthermore, a few short cycles, or parts thereof, are suggested by the figures in Table 2.

The most marked cycle is in the economic history and development area (category 3). It suggests that theses in Pidgin English on "Pidginian Devilopement Potiental Cooperation Agoricultural" may be on their way out, after peaking in 1962. This same year seems in retrospect a minor intellectual watershed, if one examines the table. There are peaks in economic theory (category 2) and the "population-welfare-consumption" catch-all (category 15). There are six correlative troughs in the same year, in statistics and social accounting (category 4), business fluctuations (category 6), business finance (category 10), business organization and managerial economics (category 11), industrial organization (category 12), and the land-agricultural economics combination (category 13). Monetary and labor economics (categories 7 and 14) have their troughs in the following year (1963). Still shorter cycles, approximating intellectual Kitchin or inventory cycles, are suggested by public finance and international economics (cat-

TABLE 2

SUBJECTS OF COMPLETED DOCTORAL DISSERTATIONS IN AMERICAN ECONOMICS DEPARTMENTS, 1960–65

(Percentage Distribution*)

Category Number and Short Title	1960	1961	1962	1963	1964	1965
1. General economics; methodology	0.15	0.38	0.12	0.32	0.35	0.20
2. Economic theory; doctrinal history	7.21	6.77	9.32	7.02	6.97	7.67
3. Economic history; economic development; national economies	9.66	10.98	17.64	16.41	5.84	6.54
4. Statistical methods; econometrics; social accounting	1.53	2.43	0.99	2.05	2.53	2.25
5. Economic systems; planning and reform; cooperation	1.07	1.28	0.37	0.65	2.18	2.05
6. Business fluctuations; forecasting	1.84	1.15	0.62	1.30	1.92	2.04
7. Money and banking; monetary policy	9.51	8.56	8.07	7.67	8.28	9.82
8. Public finance; fiscal policy	6.90	5.62	6.09	4.43	6.45	4.91
9. International economics	8.44	6.77	7.45	7.25	8.28	7.57
10. Business finance; investments; insurance	6.13	5.75	5.09	6.26	6.79	7.16
11. Business organization; managerial economics; marketing; accounting	10.58	10.98	10.93	12.10	13.59	9.41
12. Industrial organization; government and business; industry studies	10.74	10.22	9.94	11.23	12.46	10.63
13. Land economics; agricultural economics; economic geography; housing	12.88	16.22	10.93	12.85	12.72	16.56
14. Labor economics	9.82	10.60	7.70	7.45	9.23	9.41
15. Population; economic welfare; consumer economics	3.53	2.30	4.72	3.02	2.44	3.89

* Percentages rounded and may not add to 100.00.
SOURCE: *A.E.R.*, Sept. issues, 1960–65.

egories 8 and 9), with their saw-tooth but slightly falling pattern throughout the six-year period.

IV

It is difficult to say anything new about economic fads. There are plenty in evidence—probably more than usual, because more public or private money is pushing one fad or another, and more academic departments are financing the rocky road to excellence or high professional "Hooper rating" with "fad money." For the nonce, fad money has sponsored a minor academic gilded age, the campus equivalent of the "cuspidor capitalism" of a hundred years ago.

A fad is a fashion of which one disapproves, or at least considers overdone. By applying the name of "fad" to such a fashion, one is asserting or hoping that this fashion will be shorter-lived than normal—

random disturbance, in statistical parlance, rather than a cyclical one. He may even hope to exorcise the fashion out of fashion by the black magic of the "fad" label.

There is nothing shameful about fads or about admitting their existence. We have fads in food, dress, social customs, and all the other arts and sciences. Why should economics hope for immunity? What many of us want, and what I wish I could supply, is a method of distinguishing *ex ante* the fad from the "wave of the future," and so protecting oneself and one's juniors from betting their limited intellectual capitals on the wrong horses.

Here again, I think I have found useful material in Stigler's new book. In the passage I am using for my text, he is not talking about fads but about "environmental" interpretations of long-term trends, so I propose to turn his views upside down, inside out, and backwards, before commenting upon them. The relevant running quotation [17, pp. 20-22] is longer than most:

> [I]t is a sign of the maturity of a discipline that its main problems are not from immediate, changing events. [P]ersistent separation of scientific study from the real world leads to sterility, but sensitive response to current events stultifies the deepening and widening of principles and techniques. The leading theoretical chemists are not working on headache remedies, and the leading economic theorists need not be concerned with urban renewal.
>
> The vast majority of current social economic problems are routine from the standpoint of economic theory. It is more remarkable that most [events of major economic significance] leave economic theory unaffected. Minor changes hardly seem adequate to great new problems, and extensive reconstructions are usually the result of the systematic elaboration of a basic idea which previously had been ignored or given only *ad hoc* recognition. This suggests the third and influential type of economic problem: that which is pervasive. It is not enough that a problem be of vast importance, if that importance is momentary; it is not enough that the problem be persistent, if it is local.
>
> The dominant influence upon the working range of economic theorists is the set of internal values and pressures of the discipline. The subjects for study are posed by the unfolding course of scientific development. This is not to say that the environment is without influence, for every great economist injects some portion of it into the developing theoretical corpus. This element of realism, however, need have no simple or direct connection with the contemporary scene.

If Stigler is basically right about the economic mainstream, what is he implying about economic fads? Let us stand his argument on its head and hazard a guess. Are fads not primarily the short-term consequences of those environmental influences which Stigler had discounted for the long run? Are they not predominantly the more or less routine applications of economics—orthodox or unorthodox, good or bad, developed or undeveloped, rational or emotional—to problems in which interest is either general but short-lived, or alternatively, long-term but localized? These seem to me the most important and deceptive of the fads.

The other sort of fad includes literature "feeding upon itself," along with epidemic applications of this or that novel technique or formula to routine "empirical" testings by "Schumpeterian imitators." The

rash of expositions, critiques, and counter-critiques of Keynes's *General Theory* in the 1930's exemplify literature feeding upon itself; more recently, "simultaneous-equations approaches," "permanent" transformations of this or that economic variable, and homohypellagic production functions illustrate the rush for technical novelty. But these fads are generally easier to diagnose than the others, and therefore less likely to overstep reasonable bounds.

Let us return to the major fads: the temporary or localized kernels of general economic interest. This interest, it sometimes seems, can be more apparent than real, when it needs the support of massive public or private financing, novel hardware, or both. It also has in its favor attractive prospects of visiting the real world, even doing good to grateful people in a pleasant climate with sanitary drinking water. These appeals are obvious; few among us can claim immunity throughout their working lives, or even discount fad following as an error of their misspent youth.

Another point in defense of fad following: it is usually less risky than its name perhaps implies, and so the risk-averter need not concern himself too greatly for the safety of his professional capital. To follow a fad up the blindest of alleys gives the fad-follower n years of respite from competition with the higher I.Q.'s of his profession, who tend to concentrate near main streams and principal tributaries. If the fad-follower has passable personal relations and administrative talents, and if his n fad-following years encompass one presidential administration or long-term foundation grant, year $(n + 1)$ will find him far above the battle—an academic distinguished professor, a public sector bureau chief, a corporate vice-president, or equivalent. "He has ten telephones at once, so who can guess he is a dunce?"

These envious and splenetic interjections cannot substantiate my suspicion that, for example, much of the burgeoning "regional economics" of the Great Society will prove a fad, and likewise the behavioral simulations of janitors selecting broom-pushing routes under uncertainty. Neither can they substantiate my parallel confidence that some economic aspects of the poverty program will prove substantially more than fads. These arguments involve "pervasiveness" in its two meanings of continuity over time and place, and likewise the need for something more than routine applications of accepted analysis.

The economics of poverty has already involved us—under other labels, it is true—with the rival policy implications of micro- and macroeconomics, issues we have "ignored, or given only *ad hoc* recognition" for at least a generation. (Should we operate primarily on the level of aggregate demand, or on wage rates for unskilled labor, or on some *tertium quid?*) The economics of poverty is concerned with the effects of alternative antipoverty strategies upon the formation of human cap-
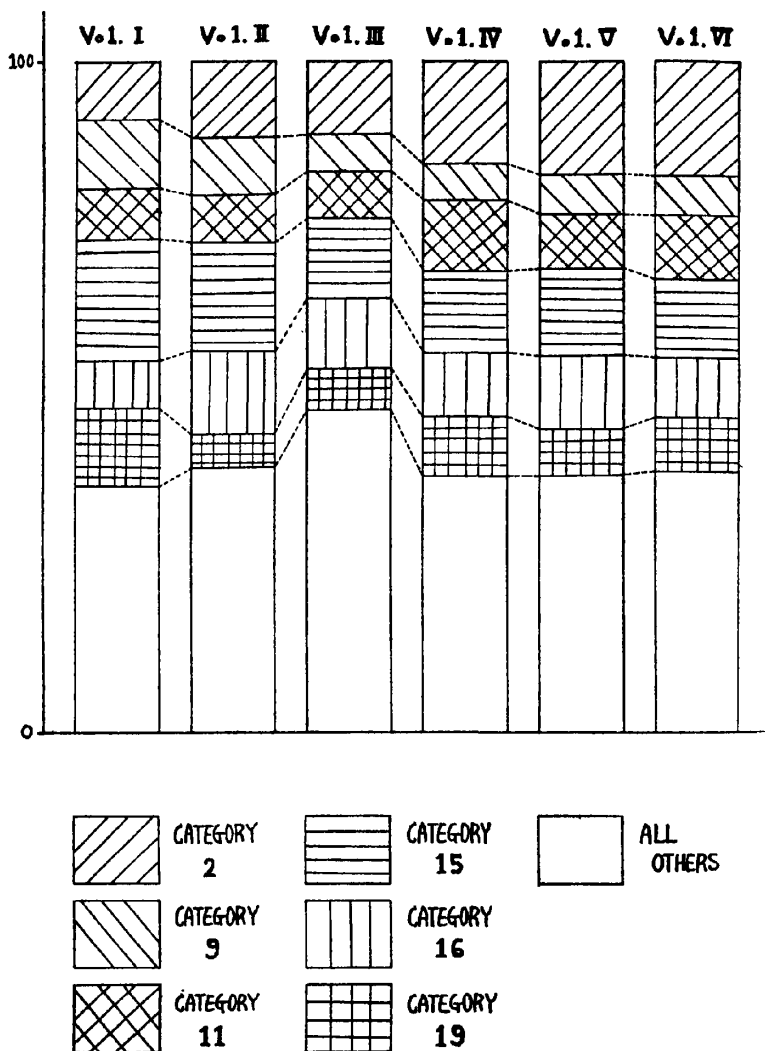
CHART I

ital, as well as upon incentives of the conventional sorts. The Triple Revolutionaries believe these latter, especially the incentive to work, to be largely obsolete. If we are only Single or Double Revolutionaries, conflicts remain between avoiding the disincentive effects of guaranteed minimum incomes and permitting consumer sovereignty in the
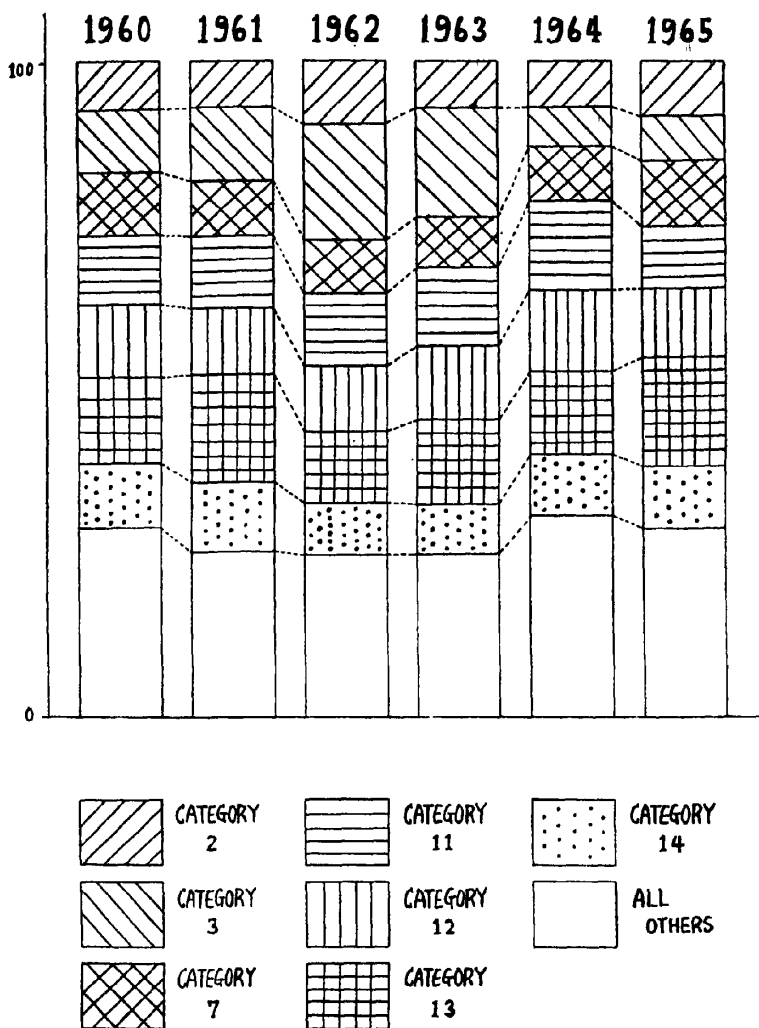
CHART II

work-leisure choices of the poor. We should also consider the financial and other costs, and the alternative distributional effects, of rival poverty programs, from the viewpoint of the rest of society. Some of us, for example, would use the War on Poverty in part as an engine for "soaking the rich" and moving in the direction of sumptuary legisla-

tion; others of us would use it as an engine for breaking up labor aristocracies in skilled trades, both blue- and white-collar.

These conflicts and implications, I think, make the poverty problem pervasive, and transcendent of routine, in ways that the regional problem is not, at least in societies with relatively cheap and open internal migration.

Finally, as for simulations, I hear rumors that graduate schools may shut off the flow of advanced degrees to computer programs, unless the auxiliary human candidate has contributed finitely to their devising, improvement, or interpretation. This, if true, I should myself consider a blessing, but surely my critics will take up the cudgels against such reactionary sabotage of the profession's Car of Juggernaut.

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DISCUSSION

HENRY H. VILLARD: The question on which I wish to concentrate is whether the allocation of scarce resources to economic research is optimal. Though the matter is rarely discussed explicitly, I have the impression that economists believe that every "reputable" economist should be supported in doing whatever he wants to do. I submit this does not make much sense, partly because I have yet to meet anyone who considers himself a "disreputable" economist unworthy of support, but primarily because all other academic disciplines feel exactly the same way.

To put the matter in extreme form, would it be desirable to have all college teachers teach only three hours a week in order to do more research? I submit that this question can only be answered on the basis of an estimate of the value of the research that would result. But in practice economists, who in every other connection stress the importance of returns in relation to the cost constraints imposed by the possibility of alternative uses, tend to argue that all additions to economic knowledge are worth while. Obviously this can only be true if the marginal contributions of economists in alternative uses are considered worthless—a proposition which I, at least, am not prepared to defend!

Let me clear away certain obvious difficulties that may confuse the matter. First of all, the value of additions to knowledge is hard to anticipate, so that the unforeseeable value of additions to knowledge may be far greater than those which can be foreseen. Hence research should, it seems to me, be encouraged to a point at which its foreseen return is far smaller than its cost. But unless there are foreseen reasons why the ratio of foreseen to unforeseen results should differ among alternatives—which I submit is rather a contradiction in terms—it will still be necessary to allocate whatever level of support we are prepared to provide on the basis of foreseen results.

Second, I shall leave to those more concerned than I am with the importance of catering primarily to market demand to worry about how market demand can be used to finance even the foreseen results of additions to knowledge, to say nothing of those which cannot be foreseen.

This brings me to my first main point: at present we support research without making any estimates whatsoever of foreseen value. This is so primarily because—though it may come as a surprise to some of you—most academic research is still supported by our relatively light teaching loads. But when teaching schedules are uniform, we in effect provide research support in proportion to what students need or want to be taught. I submit that, were we rational about the matter, the amount of time free of teaching and committee assignments should be related as precisely as possible to the foreseen results of the research thereby made possible.

I make this point not to advocate differences in teaching loads among disciplines; in a world where every academician is equal, I am not that stupid. Rather what seems to me important is that we have come to regard the time

provided us for research as "free" time which is ours to do with as we choose when in fact it is very expensive time made available to us by prevailing social and educational arrangements. It is, I submit, because we consider research time as "free" that we tend to treat all additions to economic knowledge as worth while—and to argue that foundations "owe" us the same "undirected" support of research as we presently receive from educational institutions.

Even when we recognize that research involves an allocation of resources and so requires an evaluation of foreseen results, we still face the question of how the value of such results can be determined. I freely admit that a money measure of the value of additions to knowledge is not the whole story, but I am willing to follow in many illustrious footsteps by taking it as a first approximation—which is to say, again following in illustrious footsteps, I shall pay no further attention to other aspects of the matter.

One might offhand have thought that economists would be particularly concerned with the probable contribution of their activities to increases in money value—or, more precisely, to appropriately discounted increases in real income over time, which I believe is what we mean by "money value." Instead, I submit that, as a general rule, we could not care less. On this you do not have to take my word but can rely on Paul Samuelson's, who concluded his presidential address "the economic scholar works for the only coin worth having—our own applause"—which means, if I understand him correctly, that we are prepared to measure pretty much everything by the coin of the market place except what we do ourselves!

What do we applaud? I submit primarily analytical sophistication. We do, after all, want to be "scientists"—so that we can look down on sociologists in the same way that we fear physical scientists look down on us. It was because we had some quite sophisticated insights into the way in which free enterprise solves the allocation problem—and almost none regarding either fluctuations in income or differences in what Smith called "wealth" and we today would call "real income"—that Lionel Robbins felt it correct in 1930 to define economics as "the allocation of scarce means among alternative uses." In that year the leading text—the Samuelson of its day—devoted a total of 16 out of 1200 pages to economic fluctuations and none whatsoever to efforts to explain differences in living levels.

Obviously there have been some revolutionary changes since then stemming mainly from the work of a man named Keynes. But Cambridge did not feel that merely revolutionizing economics was scholarly enough to make Keynes a worthy successor to Pigou, so that the professorship on Pigou's retirement went to Robertson. When, incidentally, did you last read Pigou—or, for that matter, Robertson? And how many do you think will a generation hence be avidly reading the current outpouring of econometric models?

What I have done thus far is to agree with Paul Samuelson that economists do much elaborate research to win the applause of other economists. The most important point I want to make is that I do not see what basis we have for asking for support for our research so long as we are concerned primarily with applause and remain unwilling to subject our work to the criterion—at least

as a first approximation—of its probable appropriately discounted contribution to foreseen increases in real income.

Does this mean that much economic research is likely to be wasted as economists vie for applause? Would that it were merely wasted! If economists did nothing but economic research, then the worst they could do would be to waste their time. But unfortunately economists are also teachers, and, because they are, I submit that much economic research serverely retards the growth of real income. This is so because, if real income is to increase, there must be broad comprehension of what must be done to increase incomes. But, as "economics is what economists talk about and economists are people who talk about economics," what we teach is directly related to what we research. The more, therefore, we engage in research unrelated to the factors responsible for increasing real income, the more we will clutter up our courses with irrelevant material and reduce the broad comprehension needed if appropriate policies are to be politically acceptable.

A single example of what I have in mind will have to suffice. We typically teach that the point at which a perfectly elastic demand curve is tangent to the minimum point of a U-shaped cost curve is the point at which total output is produced most efficiently, and, when a realistically minded student asks for an example, we typically mention agriculture, which suggests that the organization of agriculture is in some sense ideal. Instead I offer four propositions: (1) the last time any agricultural demand curve was tangent to a cost curve in peacetime was at least two hundred years ago; (2) 30 percent of the present labor force in agriculture, amounting to 2 percent of our total labor force, is almost certainly unnecessary; (3) productivity has grown rapidly in agriculture only because government, outside the free enterprise framework, has been responsible for 80 percent of all research; and (4) Professor Schultz believes that, despite this massive effort, "the resources committed annually [to agricultural research] would have to be increased very substantially before the rate of return from this stream of inputs would not exceed that obtained from production activities generally."[1] In short, I submit that, instead of agriculture being in any sense ideal, its organization and performance are grossly inadequate. But if this is true of agriculture where the government has accepted responsibility for research to the extent of bearing 80 percent of its cost, what is the rate of return from spending on research likely to be in the approximately 50 percent of American industry where spending on research is trivial, yet the government has accepted no responsibility whatsoever? And where in any standard text are matters of this sort discussed?

In my opinion, the inadequacy of the typical introductory text is directly related to its impact on high school teachers, who are after all responsible for 90 percent of the comprehension of economic matters that we achieve. Recall that Lee Bach reported in the June *American Economic Review* that high school teachers had to take three courses in economics before they knew measurably more about the subject than when they started. Undoubtedly this rate of achievement has something to do with the quality of our high school teach-

[1] Theodore W. Schultz, *The Economic Organization of Agriculture*, p. 120.

ers, but I submit that the dozen diagrams Samuelson feels are essential for a comprehension of income determination cause potential high school teachers to avoid economics in droves—and goes far in explaining why it is that in New York City every high school student is required to take a course in economics, but those who teach the courses are not themselves required to have taken any economics whatsoever!

Am I fair in blaming these sins—if sins they be—on economic research? Not if by research we mean that which is supported by outside funds: there isn't that much support. But I think I am fair if one means by research what economists do with their spare time.

In short, I do not see how economists can ask for support of the research they wish to do unless they are willing to have the cost of such research compared with the best possible estimate of its foreseen economic, and ultimately social, value—which is, I suppose, another way of saying that the direction economic research should take is altogether too important a matter to be left to the applause of the economists!

What of the question with which we started: is or is not the present allocation of resources to economic research optimal? Given the severely adverse effect of much current economic research on economic understanding, I submit that no easy answer is possible. But I am prepared to argue that research on the contribution of economic research is worth far more support than it currently receives—if only because I am convinced that, if we face up to the need for evaluating explicitly the results of our work, we will significantly increase the relevance of our research performance.

JACOB L. MOSAK: This session may be regarded as a testimonial to the 1964 President of the American Economic Association. Professor Bronfenbrenner has presented his paper as a gloss on Stigler's *Essays in the History of Economics* with which he finds himself in close agreement, and Dr. Calkins' paper on the use of economic knowledge in the conduct of public affairs uses Stigler's presidential address on the "Economist and the State" as its springboard. Like Dr. Calkins, I am inclined to believe that the influence of the economist on policy formulation has been considerably greater in recent years than Stigler seems willing to concede. Surely, one cannot complain about the lack of influence of the economist on policy if in the course of a single generation one economist can succeed in establishing his "heresy" as the orthodoxy of government and the business community. If anything, Keynes may have succeeded only too well, since for a time there was some danger that his theories might be applied too mechanically, even in countries where they were not directly relevant; to wit, in underdeveloped countries suffering from inadequate productive and absorptive capacity.

Nor, as Dr. Calkins points out, has the role of the economist in policy been limited to that of formal studies. No one familiar with the process of policy formulation in recent times in the United States or abroad could fail to be impressed with the active role of the economist as policy adviser to the government, whether as a regular government official or as an outside expert called in

for special assignment. Dr. Calkins has given a number of examples in his paper, and these could be multiplied many times over in one country after another and in the field of international as well as of national policy.

I would concede that the expanding role of the economist in influencing major economic policy is of fairly recent origin. But surely that role has been unmistakable in the United States since the New Deal period, in Western Europe since the Marshall Plan and the formation of the Common Market, and in the last five to ten years it has also become clearly evident in the centrally planned economies.

My second comment concerns a point in Bronfenbrenner's paper in which I find myself in very strong agreement with the main theme of Stigler's presidential address. In his paper, Bronfenbrenner calls attention to the trend towards "economic science," as distinct from what used to be called "political economy." This trend, it seems to me, is composed of two elements. For most of the period the trend was essentially towards greater formalization and abstraction and away from policy examination. This part of the trend I would regard as a mixed blessing. Insofar as it led to greater rigor and precision of analysis, it was, of course, all to the good. But insofar as it led to the piling up of more and more "empty boxes," I do not believe it represented very much of an advance over the old political economy. Here I can only express my admiration for the very effective criticism of both classical political economy and neoclassical economics made by such a leading exponent and historian of this field of study as Professor Stigler.

The second component of this trend towards economic science is a much more recent one, and it will, I believe, prove of far greater significance. I refer to the trend in this generation towards the measurement of economic phenomena and the empirical testing of economic theories. It is a trend to which the institutes that are represented here have contributed in considerable measure. I agree with Stigler that this represents a scientific revolution of the very first order of importance.

Many factors would have to be considered in explanation of this trend. In part, the measurement of economic magnitudes and the empirical testing of theories is a natural and, one might almost say, inevitable outcome of the greater precision reached in the mathematical reformulation of economic theory. In part it is the fruit of the pioneering endeavors of men such as Kuznets, Leontief, and Tinbergen, who combine an extraordinary curiosity about economic magnitudes with almost infinite patience to assemble the necessary information. In large part, however, it is due to the shattering confrontation between classical and neoclassical economic theories and the hard economic realities, first of the depression of the 1930's and then of the postwar worldwide drive for economic growth and development.

The medieval scholastic, we have been told, looked for his answers in Aristotle and Plato rather than in the laboratory. For over a century the elite of the economic profession tended to look for its answers in the logic of simple algebra and elementary calculus, forgetting that mathematical theorems are only tautologies—highly useful and even indispensable for clear thinking—

but, nevertheless, tautologies which cannot yield any new information that is not already contained in the initial premises. So long as the economist was the prisoner of a priori mathematical laws, of immutable and eternal truths, he had little motivation to get at the facts. This was equally true of the classical period when economics was a dismal science with iron laws of wages and subsistence levels of living, and of the neoclassical period when it was a happy science with golden laws demonstrating that the free market provided maximum social welfare.

The great Keynesian contribution to this scientific revolution was, I believe, that it freed the economic profession from the shackles of such inevitable laws. With the displacement of Say's law, for example, it became possible for economic theorists in good conscience and in good standing in the profession rather than as schizophrenic personalities to search for answers to problems of employment and unemployment in the data and not in a priori theorems. It was the Keynesian analysis of problems of unemployment in developed countries which produced such widespread recognition, both in the profession and in government, of the need for social-accounting data to deal with economic policy questions. In the postwar period, recognition of the urgency of the problem of economic underdevelopment has contributed to the revolutionary development of economic information in much of the rest of the world. And, as Dr. Calkins rightly points out, the enormous improvement of economic data and the increasing resort to empirical testing has been both a result and a means of extending the influence of the economist in policy formulation.

LABOR ECONOMICS: EFFECTS OF MORE KNOWLEDGE

INFORMATION NETWORKS IN LABOR MARKETS

By ALBERT REES
University of Chicago

This paper is not concerned with the information about the labor market provided by labor statistics. Rather it deals with the information that participants in the market have about one another—with the ways in which job seekers find jobs and employers find employees. I shall draw heavily on a study of the Chicago labor market now in progress in which my associates are George P. Shultz, Joseph C. Ullman, David P. Taylor, and Mary Hamilton.[1] The focus of the paper is accordingly on local rather than national markets.

We may divide information networks in the labor market into two groups: formal and informal. The formal networks include the state employment services, private fee-charging employment agencies, newspaper advertisements, union hiring halls, and school or college placement bureaus. The informal sources include referrals from employees, other employers, and miscellaneous sources, and walk-ins or hiring at the gate.

The literature stresses the great importance of the informal channels, and our study of the Chicago labor market offers additional support for this emphasis. In the four white-collar occupations under study, informal sources account for about half of all hires; in the eight blue-collar occupations, informal sources account for more than four-fifths of all hires.

Economists have traditionally taken a dim view of informal networks of labor market information. The typical discussion of channels of employment begins with an analogy between the public employment service and stock or commodity exchanges. To be sure, various reasons are given why the analogy is imperfect and a "grain exchange for labor" cannot be established. But in the end, the disorganization of the labor market is deplored and suggestions are made for the improvement of the employment service.

For example, a recent textbook in labor economics starts a discussion of the effectiveness of the labor market by using the New York

[1] I am indebted to these associates and to George J. Stigler and Arnold R. Weber for helpful comments on a draft of this paper. I am also indebted to the Ford Foundation for its generous support of this study.

Stock Exchange as a model of efficiency. It notes that formal inter
mediaries in the labor market are not widely used, and concludes tha
"the worker who sets out to find employment very likely goes throug
a process of chasing down vague rumors or leads." "All too frequent
ly," it adds, "the buyers and sellers, blindfolded by a lack of knowl
edge, simply grope about until they bump into each other."[2] I shal
argue here that the analogies with commodity and security markets
even when qualified, are mischievous and misleading and that th
effectiveness and advantages of informal networks of information hav
been too little appreciated.

The search for information in any market has both an extensive an
an intensive margin. A buyer can search at the extensive margin b
getting a quotation from one more seller. He can search at the inten
sive margin by getting additional information concerning an offer al
ready received. Where the goods and services sold are highly standard
ized, the extensive margin is the more important; when there is grea
variation in quality, the intensive margin moves to the forefront. Thi
point can be illustrated by considering the markets for new and use
cars. Since there is relatively little variation in the quality of new car
of the same make and model and since the costs of variation are re
duced by factory guarantees, the extensive margin of search is the im
portant one. A rational buyer will get quotations from additional deal
ers until the probable reduction in price from one additional quotatio
is less than the cost of obtaining it.[3]

In used cars of the same make, model, and year much of the varia
tion in asking prices reflects differences in the condition of the cars
and this calls for a substantial change in the strategy of the rationa
buyer. He will invest less in obtaining large numbers of offers an
much more in examining each car. For example, he may have each ca
he seriously considers inspected by a mechanic. He may want informa
tion on the history of the car as a substitute for the direct assessmen
of condition and will pass up a used taxi in favor of the car owned b
the proverbial little old lady who drives only to church. It will not b
irrational for him to pay a relatively high price for a car owned by a
friend if he has favorable information about his friend's habits as a ca
owner.

Organized commodity and security exchanges deal in highly stan
dardized or perfectly uniform contracts, where the intensive margin o
search is effectively eliminated. One is entirely indifferent as to wheth
er one buys 100 shares of General Motors from a taxi company, a littl
old lady, or Alfred P. Sloan, though much search may enter into th

[2] Sanford Cohen, *Labor in the United States* (Charles E. Merrill, 1960), p. 351.
[3] See George J. Stigler, "The Economics of Information," *J.P.E.*, June, 1961.

decision to buy General Motors rather than some other security. Organized exchanges perform a highly effective job of widening the extensive margin of search and need to transmit only a few bits of information (the name of the contract, the quantity, and the price) to conclude a transaction. Labor markets lie as far from this pole as used car markets, and a grain exchange for labor is about as possible as a contract on the Chicago Board of Trade for 1960 Chevrolet sedans.

The large variance of wages within narrowly defined occupations in particular local markets affords some evidence of the variance in the quality of labor and in the attractiveness of jobs, though it has other sources as well. For example, in our sample of maintenance electricians in the Chicago area we found a range of hourly earnings in June, 1963, of from $1.75 to $4.75 an hour. Their formal educations ranged from less than four years of schooling to some college. They worked in places ranging from spotless modern plants in pleasant suburbs to old loft buildings in central city slums.

Variation in the quality of applicants in many dimensions is one reason why employers invest so much in the selection of new employees. A second is that present seniority arrangements, both contractual and traditional, mean that in a large number of occupations an employee who survives the probationary period is likely to be with the firm for many years. The total of his wages over this period will run to tens of thousands of dollars. The hiring of an employee is a transaction analogous in size to the purchase of a car or even a house by a consumer and justifies substantial costs of search.

It is therefore not surprising to find employers using many different selection devices. An applicant for employment may be examined in several or in extreme cases all of the following ways: a written application for employment, an interview, paper and pencil tests, work sample tests, a medical examination, a check of credit standing, a check of school and employment references, and even police record checks. The problem facing the employer is not to get in touch with the largest possible number of potential applicants; rather it is to find a few applicants promising enough to be worth the investment of thorough investigation. This is particularly true since in general the buyer and not the seller in labor markets quotes the starting wage. The employer usually has little interest in discovering applicants willing to work at less than the prevailing rate; if he is covered by a union contract, he has none at all.

Many employer hiring standards can be viewed as devices to narrow the intensive field of search by reducing the number of applicants to manageable proportions. Within the narrowed field defined by hiring standards, extensive search can be conducted through the most ap-

propriate channels. Thus we encounter such rules as the following: clerical workers must be high school graduates; material handlers must weigh at least 150 pounds; janitors must have lived a year in the metropolitan area; employees who use public transportation must not need to make more than two transfers. Each of these rules has some relevance to job performance, but lack of the qualities specified could be compensated for by the presence of others. Such rules are often relaxed if there is a shortage of applicants who can meet them. This flexibility is illustrated by a large Chicago area manufacturing establishment whose newspaper ads for blue-collar workers when the market is loose specify, "Must be high school graduate"; when the market tightens, this is replaced by, "Average piece rate earnings $3.19 an hour." In addition to formal hiring standards, employers have a still more flexible set of preferences among job applicants, such as the preference for married men for unskilled work because they are thought to have lower quit rates.

Most employers have a strong preference for using informal information networks, for a variety of reasons. Employee referrals—the most important informal channel—usually provide good screening for employers who are satisfied with their present workforce. Present employees tend to refer people like themselves, and they may feel that their own reputation is affected by the quality of the referrals. Informal sources also tend to provide applicants from the neighborhood in which the establishment is located; this is particularly important for female employees in reducing turnover, absenteeism, and tardiness resulting from transportation difficulties. Moreover, informal channels are usually costless to the employer, though we have found a few cases in which bonuses are paid for employee referrals that result in hires. Of course, some formal channels such as the state employment service are also costless. The few employers who deliberately avoid informal sources are either those who are seeking to upgrade their work force or those who have had bad experience with nepotism or cliques.

The informal sources also have important benefits to the applicant. He can obtain much more information from a friend who does the kind of work in which he is interested than from an ad in the paper or a counselor at an employment agency, and he places more trust in it. He can ask the counselor about the fairness of supervision in a factory, but he cannot often get an informed or reliable answer. If informal sources result in a placement in the applicant's home neighborhood, he minimizes transportation costs, both in time and in direct outlay. Finally, the presence of a friend in the plant may be an important "fringe benefit," making the job more attractive to the worker at no cost to the employer.

The fact that employers generally prefer informal sources does not mean they are always able to use them. As George Stigler has pointed out, high wages and high search costs are substitutes for an employer; low-wage employers are therefore forced to use high-cost information channels, such as newspaper advertising and private employment agencies.[4] This hypothesis receives strong support from the findings of Joseph C. Ullman, who has analyzed the Chicago market for two female clerical occupations: typists and keypunch operators. Ullman reports significant negative relationships between wages and the proportion of clerical workers hired through newspaper advertising and private agencies.[5]

The literature on formal information networks is uniformly hostile to private employment agencies. One of the leading scholars in the field, E. Wight Bakke, speaks of unemployed workers "falling into the clutches of exploiting fee-charging agencies, who took from ignorant people in desperate need of jobs a big toll from their pay for providing a very poor labor broker service."[6] Many employers also have little use for private agencies, or "flesh peddlers." Some complain of pirating—attempts by agencies to hire away people they have previously placed in order to earn another fee—and many complain of being pestered by phone calls from agency counselors. Some agencies do a poor screening job because of the high turnover of counselors. Since there are many agencies and there is vigorous competition among them, employers who complain about the practices of a particular agency often shift their business to a competing agency rather than turning to an alternative type of hiring channel.

Despite complaints from professors and employers, private agencies have been growing rapidly. Between 1943 and 1958 the number of private employment agencies in the United States increased from 2,200 to 3,900, their receipts tripled, and their payrolls quadrupled.[7] One is forced to at least grudging admiration of an industry that can thrive on selling at substantial fees a service that the government provides gratis.

In fact, our employer interviews reveal that many employers are well satisfied with private agencies. This is especially true in the clerical market, where Chicago employers typically pay agency fees of 60 or 72 percent of a month's salary. There is some tendency for the firms

[4] See George J. Stigler, "Information in the Labor Market," *J.P.E.*, Oct., 1962, Sup.

[5] Joseph C. Ullman, "Inter-firm Differences in the Cost of Search for White Collar Workers" (unpublished doctoral dissertation, Graduate School of Business, Univ. of Chicago, 1965).

[6] E. Wight Bakke, *A Positive Labor Market Policy* (Charles E. Merrill, 1960), p. 15.

[7] See Eaton Conant, "An Evaluation of Private Employment Agencies as Sources of Job Vacancy Data," in *The Measurement and Interpretation of Job Vacancies* (N.B.E.R., 1966).

that use agencies extensively to be smaller than average, with fewer facilities in their own personnel departments, which suggests the presence within limits of economies of scale in hiring. The most satisfactory relationships are often with agencies that are specialized in terms of occupation, industry, or location, and involve dealings over a prolonged period with a particular counselor who knows the employer's needs.

The number of employers in our sample who make frequent use of the Illinois or Indiana Employment Services and are well satisfied with them is considerably smaller than the number who report good results from private agencies. Private agencies placed from 10 to 32 percent of the workers in the four white-collar occupations we studied; the state employment services placed only from 1 to 3 percent. In the eight blue-collar occupations, private agencies were more important than the employment services in three and less important in three others; in no case were hires through the state employment services more than 4 percent of the total.

The highest level of satisfaction with the state employment services was reported by employers who deal with suburban offices rather than central city offices. In these cases they often mentioned regular contact with the same counselor as the key factor in good service.

We encountered some employers who object on principle to the government running an employment service, and several who avoid the employment service because, despite Fair Employment Practices Acts, they do not hire Negroes. However, such cases were clearly not the main sources of dissatisfaction or nonuse in our sample. The most frequent complaints against the employment service are slowness and poor screening. Some respondents gave specific examples, such as this one from a branch store of a large department store chain: "A year or so ago we placed an order with the Employment Service for a couple of high school graduates for openings in the credit department. We didn't care too much about experience, and would take trainees. They sent over forty applicants and about half weren't high school graduates. Most of the rest were overqualified and wouldn't accept the jobs. I finally hired a couple of people, but it just wasn't worth the effort to talk to them." Stories such as this suggest that the number of referrals is a very poor yardstick for evaluating an employment service—the number of placements is a better one, and the ratio of placements to referrals may be better still.

A manufacturing firm that has employed Negro blue-collar workers for many years stated that it does not use the employment service because "instead of trying to meet our qualifications, they just send over people who have trouble finding jobs, and they aren't the best people." Such employer reactions suggest the strong tension between the em-

ployer objective of getting the best for his money and the objectives of agencies that seek to promote social welfare by referring the workers whose needs are greatest. Unfortunately, referrals alone do not alleviate need—only placements do.

Chicago area employers use newspaper advertising extensively in recruiting white-collar workers (from 14 to 23 percent of all hires in four occupations) and to a smaller but still significant extent for blue-collar workers (1 to 13 percent of hires in eight occupations). In no occupation was the employment service more important than newspaper advertising. Many employers prefer neighborhood to metropolitan papers. In some cases this is again a device for racial screening, but more often it is intended to minimize transportation costs and thus to cut turnover and encourage attendance. Trade papers and foreign language papers are important in some industries, such as men's clothing.

The preceding discussion fails to suggest the rich variety of hiring channels in a metropolitan labor market. Referrals from unions are important in trucking and in the printing trades, as well as in construction, which was excluded from our sample. Referrals from one employer to another occur in cases of layoffs and plant shutdowns. Public utilities recruit clerical workers extensively from high schools; private vocational schools are important in the data processing occupations; and college recruiting is important for professional and managerial jobs. Some of the hiring channels we discovered do not fit any of the usual categories. One manufacturer hires truck drivers through a large trucking firm located across the street; another employer hires accountants through the public accounting firm that audits its books. A large distributor of furniture and home appliances hires warehousemen from moving and storage companies, whose slack season coincides with this employer's peak season. Such arrangements seem untidy in terms of the design of an orderly information network; yet they may nonetheless be highly effective.

In some cases matching an opening with an applicant requires search over a wide geographical area. Such cases arise largely for a few highly skilled crafts and for specialized managerial and professional jobs. The employer with a sudden need for a bassoon player, a deep sea diver, or a specialist in the chemistry of fluorine compounds might be willing to recruit from across the country or around the world. Such needs are served in part by the professional office network of the United States Employment Service and in part by the private executive recruiting or "head hunting" agencies. Search at long distances is also indicated when there are serious local imbalances between supply and demand. The employer will engage in long-distance search in cases of excess demand and the employee in cases of excess supply.

It is in such cases that direct communications networks connecting

widely separated locations make the most sense. A highly sophisticated network would enable the university in New England with a vacancy for a mathematical economist to call the nearest professional office of the employment service, which through its link to a computer in Washington would discover in microseconds a well-trained mathematical economist on the Pacific Coast dissatisfied because he had been passed over for promotion. Yet even a system with such capabilities would have to struggle for users against the network of personal contacts built up within industries and professions. It is quite possible that the department chairman in New England would prefer, even in making his initial list of prospects, to phone or write to one or two senior mathematical economists whose judgment he has learned to trust.

For the major portion of the market, the crucial characteristic of an effective formal information system is not the length or the number of interconnections between geographical locations or the number of applications and openings that can be brought together at one place. Rather, it is the richness and reliability of the information carried over each link. The crucial component of such a system will not in our lifetimes be built by I.B.M. or Western Electric. It is the experienced employment service counselor who is a good judge of applicants and of their records and who knows thoroughly and respects the requirements of a small number of employers he has served for a long time. This in turn implies a compensation system in which such skill and experience are well rewarded.

EDUCATIONAL ATTAINMENT AND LABOR FORCE PARTICIPATION*

By William G. Bowen, *Princeton University*
and T. Aldrich Finegan, *Vanderbilt University*

In this paper we analyze the relation between educational attainment, measured by years of school completed, and the labor force participation rate, defined as the percentage of a given population group who, during the census week, were either "employed" or "unemployed" according to the official definitions.[1] In short, what we are trying to find out is how a person's probability of being in the labor force is affected by the amount of formal schooling he has had.

Aside from pure intellectual interest, there are several justifications for studying the relation between educational attainment and labor force participation. First, it is evident that labor force participation rates have an important bearing on the long-term rate of growth of the economy, and education may well be an important determinant of participation. Second, the education-labor force participation relation is important from the standpoint of social policy, the questions here being whether lack of education is associated not only with the kinds of jobs a person can get and with his exposure to unemployment but also with his ability and willingness to seek employment in the first place. The answers to these questions have obvious relevance for antipoverty policies and the structural unemployment controversy.

Finally, the relation between educational attainment and labor force participation is important for an understanding of returns to education. Many studies have shown that education is associated with a larger lifetime earnings stream; but, so far as we know, no one has yet tried to divide this stream into earnings attributable to more labor force participation and earnings attributable to higher rates of pay per "unit" of labor force participation. This distinction is worth making, because, from a welfare economics standpoint, we usually assume that there is some pain-cost associated with work. Consequently, higher

* The research reported here is part of a much larger study of the economics of labor force participation which is being financed jointly by the Berkeley Unemployment Project and the Princeton Industrial Relations Section. This work made substantial use of computer facilities supported in part by National Science Foundation Grant NSF-GP579. Mrs. Virginia Gebhardt has been our chief research associate throughout this study, and it would be hard to exaggerate the contribution she has made. We are also indebted to Orley Ashenfelter and Stephen Goldfeld for their help with statistical problems. Michael Godfrey and Orin Merrill did a most imaginative job of programming, and without their help it would have been impossible for us to use the 1/1000 sample of the 1960 census.

[1] *U.S. Census of Population,* 1960, U.S. Summary, "Detailed Characteristics" [PC(1)—1D], p. xxvi.

earnings obtained by greater labor force participation are presumably worth less, on balance, than earnings obtained by a higher rate of pay per unit of effort.

A Priori Expectations

What relationship would a reasonable man expect to find between educational attainment and labor force participation? The first answer is that there are some spurious positive relations to be expected which we cannot control. There are good reasons to think that educational attainment is positively related to intelligence, to ambition, and probably to physical and mental health as well—and that these characteristics are in turn related to labor force participation. Unfortunately, our inability to measure these qualities in the data with which we work prevents our sorting out the effect of education per se in any precise way.

There is no reason, however, to think that these spurious associations are all that is involved. There are also reasons to think that education changes individuals in ways which will affect their propensity to seek employment. The most obvious point is that education increases a person's expected market earnings and thus increases the differential return between market and nonmarket activity; that is, the opportunity cost of staying out of the labor market is greater for a person with considerable education than for a person with relatively little education.

Educational attainment also increases access to the cleaner, more pleasant, more interesting jobs. This seems to us to be an important consideration, because nonpecuniary considerations may be very important to some people—single women, for example.

Finally, it seems plausible that educational attainment affects the taste for work itself. Here we venture into unfamiliar terrain, but our brand of amateur psychology and sociology suggests that more education may well lead people to want the social contacts and social interchange that employment often affords. The most casual empiricism suggests that the educated housewife is less likely to be content staying home alone than her less well educated counterpart.

So, for all of these reasons, our speculations lead us to expect a positive relation between educational attainment and labor force participation. We could speculate further about expected differences in the effects of educational attainment on the labor force participation of different groups within the population—on the participation of males versus females, the old versus the young, and so on. We could also offer some conjectures about expected differences in the effects of various levels of educational attainment. Is it completing high school that

gives the biggest fillip to the labor force participation of women? Or is it the completion of college? Do college drop-outs have higher participation rates than high school graduates? Our a priori answers to such questions, however, would consist only of commonsense observations —at best—and so, in the interests of space, we turn now to the presentation of our empirical findings.

Data and Methods

Most of the results reported here are cross-sectional findings for the census week of 1960. Rather than lumping the entire working-age population together into one massive aggregate, we shall present separate results for three subsets of the population: prime-age males (aged 25-54), older males (aged 65-74), and married women (aged 18-64) with husband present but no children under six. The advantages of this disaggregated approach are obvious. Labor force behavior differs appreciably among these groups, and it is important to study the differential effects of educational attainment.

Our source of data is the One-in-a-Thousand Sample from the 1960 Census.[2] All told, it provides information on over 180,000 individuals and has the great advantage of permitting us to classify them according to an enormous number of socioeconomic characteristics, ranging from the person's own education, to the income of other members of his immediate family, to national origin, to number of TV sets in the home! We have taken advantage of some of these classification possibilities to exclude from our subsample persons in institutions, persons in the armed forces, and persons living in rural areas. So, we deal only with the civilian, urban, noninstitutional population. What we cannot do with these data is relate labor force participation to local labor market conditions. For this purpose intercity analysis or time series analysis is more appropriate. But for studying the effects on labor force participation of individual characteristics—such as educational attainment—the 1/1000 sample is magnificent.

As to statistical methods, we use ordinary single-equation, multiple-regression techniques, with dummy variables. The dummy variables are necessary because of the nature of the data: people are either in or out of the labor force, and so on. Thus, our basic model consists simply of a regression equation in which a dummy variable for the labor force participation rate is the dependent variable and the primary indepen-

[2] Certain data used in this paper were derived by the authors from a computer tape file furnished under a joint project sponsored by the U.S. Bureau of the Census and the Population Council and containing selected 1960 Census information for a 0.1 percent sample of the population of the United States. Neither the Census Bureau nor the Population Council assumes any responsibility for the validity of any of the figures or interpretations of the figures published herein based on this material.

dent variable is really a set of dummies standing for various levels of educational attainment. We also include sets of dummies for certain "control" variables, as we shall explain presently. The use of a large number of dummy variables is possible here because of the number of observations; in the case of our prime-age male group, for example, we work with information for 22,107 persons. The dummy variable approach has the important advantage of permitting the relation between educational attainment and labor force participation to assume its natural form; we need not force the data into a linear or any particular curvilinear mold.

Because of space limitations, only the regression coefficients for the educational attainment dummies are presented in the text table that follows. However, the impact of the control variables on participation will be summarized briefly. The basic format of our regression equations and the coefficients for the control variables are presented in the Appendix.

Prime-Age Males (25-54)

From here on it is easiest to explain our methods in the context of the results for a particular group: prime-age males. This is an important group because it comprises almost half of the labor force. The overall mean participation rate for prime-age males in 1960 was nearly 97 percent, and given this exceedingly strong inclination to labor force participation, it might seem unlikely that variations in participation would be related in a systematic fashion to any variable. This was our suspicion, but it turned out to be unfounded.

Our findings for prime-age males are summarized in the top panel of Table 1, and under the heading "simple relationship" we first show the results of a regression explaining the labor force participation rate solely in terms of a set of educational attainment dummies. The intercept shows what the labor force participation rate was for persons with zero to four years of schooling, our reference group. It was 88.6 percent. The regression coefficients (*b*'s) then show the differences between the labor force participation rate for this reference group and the rates for those groups having more schooling. For example, the *b* of 6.9 for those with eight years of school completed means that their mean participation rate was 6.9 percentage points higher than the mean participation rate for the group with zero to four years of school.

By adding the regression coefficient for each educational attainment group to the intercept we get the unadjusted mean participation rate for each group, which we plot on Figure 1 as well as show in the table. The unadjusted means are exactly the same as the participation rates one gets by simply classifying prime-age males according to years of

TABLE 1

RELATIONSHIPS BETWEEN EDUCATIONAL ATTAINMENT AND LABOR FORCE PARTICIPATION: THREE POPULATION GROUPS* IN CENSUS WEEK OF 1960

		Simple Relationship†			Partial Relationship‡		
		b	(*s*)	Unadjusted Mean§	*b*	(*s*)	Adjusted Mean§
I. Males, 25–54							
Years of school completed:	0–4	(Intercept¶)		88.6	(Adjusted Intercept¶)		89.9
	5–7	5.0	(0.7)	93.6	4.5	(0.7)	94.4
	8	6.9	(0.7)	95.5	5.8	(0.7)	95.7
	9–11	8.3	(0.7)	96.9	7.0	(0.7)	96.9
	12	9.0	(0.7)	97.6	7.6	(0.7)	97.5
	13–15	9.8	(0.7)	98.4	8.4	(0.7)	98.3
	16	10.0	(0.8)	98.6	8.6	(0.8)	98.5
	17+	10.1	(0.8)	98.7	8.6	(0.8)	98.5
Overall mean=96.7 Number of observations =22,107							
II. Males, 65–74							
Years of school completed:	0–4	(Intercept)		26.1	(Adjusted Intercept)		26.3
	5–7	6.2	(2.5)	32.3	6.1	(2.4)	32.4
	8	14.0	(2.5)	40.1	15.0	(2.4)	41.3
	9–11	19.1	(3.1)	45.2	17.5	(2.9)	43.8
	12	22.9	(3.9)	49.0	20.2	(3.0)	46.5
	13–15	25.7	(3.9)	51.8	24.7	(3.7)	51.0
	16	28.5	(4.9)	54.6	30.2	(4.7)	56.5
	17+	37.2	(6.1)	63.3	37.2	(5.8)	63.5
Overall mean=38.6 Number of observations =3,339							
III. Married women, 18-64, husband present, no children under six years old							

NOTATION: *b*=regression coefficient. (*s*)=standard error of regression coefficient.

* Members of the Armed Forces, inmates of institutions, rural residents, and persons under 35 enrolled in school have all been excluded.

† The simple association between educational attainment and labor force participation.

‡ The net association between educational attainment and labor force participation after the "control variables" have been taken into account. The nature and impact of these control variables are explained in the text; the partial regression coefficients for the control variables are shown in the Appendix.

§ The "unadjusted" figures are the actual mean participation rates for each education interval.The "adjusted" means are based on our multiple regression equations. The partial regression coefficients for the control variables were used to standardize for differences in participation rates associated with these variables. For details, see the Appendix.

¶ The intercept in the simple relationship is the actual mean participation rate of those individuals with 0–4 years of schooling. In the partial relationship, the intercept has been adjusted to take account of the influence of the control variables. For more details, see the Appendix.

TABLE 1—(*Continued*)

		Simple Relationship†			Partial Relationship‡		
		b	(s)	Unadjusted Mean§	b	(s)	Adjusted Mean§
Years of school completed:	0–4	(Intercept)		30.8	(Adjusted Intercept)		22.0
	5–7	6.2	(2.7)	37.0	8.6	(2.7)	30.6
	8	6.0	(2.6)	36.8	11.2	(2.6)	33.2
	9–11	12.2	(2.5)	43.0	19.6	(2.5)	41.6
	12	17.0	(2.5)	47.8	27.6	(2.5)	49.6
	13–15	16.0	(2.7)	46.8	30.1	(2.7)	52.1
	16	21.4	(3.1)	52.2	39.3	(3.1)	61.3
	17+	38.5	(4.1)	69.3	54.0	(4.0)	76.0
Overall mean = 44.0 Number of observations = 12,987							

school completed and calculating the mean participation rate for each group.

Looking at the results of this simple association, we see that education does seem to affect the labor force participation of prime-age
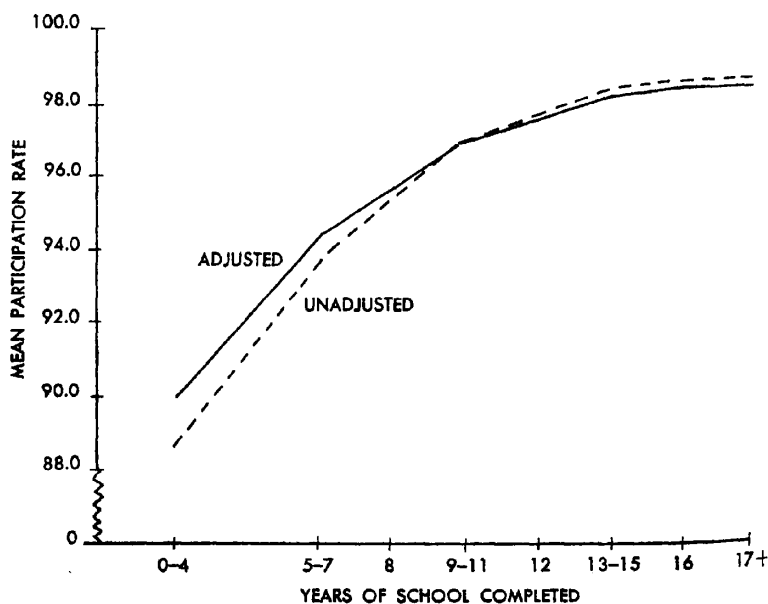
FIGURE 1
RELATIONSHIP BETWEEN EDUCATIONAL ATTAINMENT AND LABOR FORCE PARTICIPATION, MALES 25-54, CENSUS WEEK, 1960

males. The participation rate marches up steadily with educational attainment, though the relationship is not linear.

We also show standard errors in Table 1. But, because our dependent variable is dichotomous, the usual measure of the standard error is not really appropriate. The actual values of our dependent variable must be either zero or one, and so it is obvious that the residuals are not going to be distributed in the manner assumed by classical statistics. In short, these data, by their very nature, have the disease known as heteroscedasticity. We are told by people who know much more than we do about such matters that: (*a*) heteroscedasticity does not bias our regression coefficients; and (*b*) what it does do is produce exaggerated estimates of the size of the standard error. Nonetheless, we show these estimates to provide a rough basis for assessing the significance of regression coefficients; that is, the significance of the difference between the estimated mean for any given educational group and the mean for the reference group (those with 0-4 years of schooling). In general these differences appear to be highly significant, since most of them are more than four times as large as their (exaggerated) standard errors.

It is of course always possible that this kind of simple relationship will be misleading because of intercorrelations with other variables which are not taken into account. To protect against this possibility, we introduced three control variables into our prime-age male regression. The first is a set of dummies for other family income (total family income less the earnings of the prime-age male). Here we expected to find a negative relation, on the grounds that the greater the amount of other family income, the less the financial pressure on the man to work. Our second control variable is a marital status dummy which has the value of one if the man is married with his wife present, and zero otherwise. We expected a positive relationship between the marital status dummy and labor force participation for several reasons. Presumably the married man has greater responsibilities and hence a greater incentive to work for that reason. It may also be that personality factors associated with being married with a wife present are associated with labor force participation. A third consideration, suggested by one of our colleagues who had best remain nameless, is that married men may be especially anxious to get out of the house and into the labor market. Finally, we control for race by using a dummy which has the value of one if the man is white and zero if he is Negro.[8] We expected a positive relation here; that is, a lower participation rate for Negroes, for what are euphemistically called "cultural" reasons—ingrained attitudes, health, and job discrimination.

[8] Nonwhites other than Negroes were dropped from our sample.

The net (partial) regression coefficients for the control variables (see the Appendix) show that our expectations are confirmed at high levels of statistical significance. The coefficient for the race variable is +1.6, which means that we should expect a group of white males to have a labor force participation rate 1.6 percentage points higher than a group of Negroes who were equivalent in terms of the other variables included in this regression. In short, the fact that Negroes have a lower labor force participation rate is not attributable solely to their lower educational attainment, though this is clearly a large part of the explanation.

The impact of marital status is much greater than the impact of race. We find that a group of married men with wife present has an average labor force participation rate 7.5 percentage points higher than a comparable group with some other marital status (widowered, divorced, separated, or single). This is a much greater impact than we had anticipated, and it suggests that the relation between marital status and labor force behavior deserves more attention than economists usually give it.

The dummies for other family income have negative coefficients in all instances except one. (The fact that this one coefficient has the wrong sign is encouraging in a certain sense: it suggests that the results are not rigged and gives greater confidence in the other findings!) There is also some tendency for participation to fall as the amount of other income increases, but the pattern is rather irregular. The big decline comes for the group with other income of $25,000 or more—their participation rate is 19.3 percentage points lower than the participation rate for the reference group. Perhaps there are at least a few idle rich still to be found!

The regression coefficients for the control variables are interesting in themselves, but the real point of introducing them is to enable us to study the impact of educational attainment when these other factors have been taken into account. It is possible to compare directly the simple and multiple regression coefficients for the educational attainment dummies in Table 1, but comparison is easier if we construct adjusted participation rates on the basis of the multiple regression, which can then be compared with the actual (unadjusted) mean participation rates. The way we constructed the adjusted rates from the multiple regression was by estimating what the mean participation rate for a group with a given amount of educational attainment would have been if the group had been "average" in terms of marital status, race, and other income. In short, we used the regression coefficients for the control dummies to standardize our mean participation rates.[4]

[4] See the Appendix for a more detailed explanation of these procedures.

The adjusted means are shown in the last column of Table 1 and on Figure 1. It is apparent that educational attainment still has a pronounced effect on the labor force participation of prime-age males even after we allow for the influence of these other variables. But the curve is a bit flatter now, which signifies that the simple relation exaggerates slightly the quantitative effects of, say, the increment in educational attainment from the 0-to-4 years of schooling level to college graduation. The reason is that lower education groups happen to have more than their share of males who are unmarried and Negro. (The fact that the lower education group also has lower other income does pull in the other direction, but not sufficiently to offset the other associations.)[5]

Before leaving the prime-age male group, we wish to make one last point. In selecting our group of prime-age males, we excluded those men 25-34 who were still enrolled in school. If one does not do this, as we did not the first time around, he finds that the mean participation rate curve dips at the 13-15 years-of-school-completed level and at the 17+ level. This might be thought to imply that there is something special about the labor force behavior of drop-outs, that they are in some sense inferior and therefore less likely to participate. But our subsequent analysis indicates that this would have been a wrong interpretation, since some of the people in these categories are not in the labor force precisely because they are still in school. It is possible to exclude males 25-34 who are still in school when one works with the 1/1000 sample, but not when one works with the published census data. On the basis of the effect of this exclusion on our results, we are now inclined to question the meaningfulness of findings which seem to show that the rate of return to educational investment for drop-outs from any level is appreciably lower than the rate of return for persons who completed that level, when such findings are based only on the published census data. At least part of the apparent differential may be due to the fact that some of the group referred to as drop-outs are still in school and hence are unlikely to hold full-time jobs.

Older Males (65-74)

In studying the effects of educational attainment on the participation of males between the ages of 65 and 74, we used exactly the same methods and the same control variables as in the case of prime-age males. Our findings are summarized in Panel II of Table 1 and in Figure 2.

Before commenting on these results, a few words about the effects of

[5] Given the "constructed" nature of these means it would be inappropriate to use the usual test for the significance of differences between means. It is the difference between any two educational attainment partial regression coefficients which is subject to a standard significance test, and the relevant *t* values are presented in the Appendix.
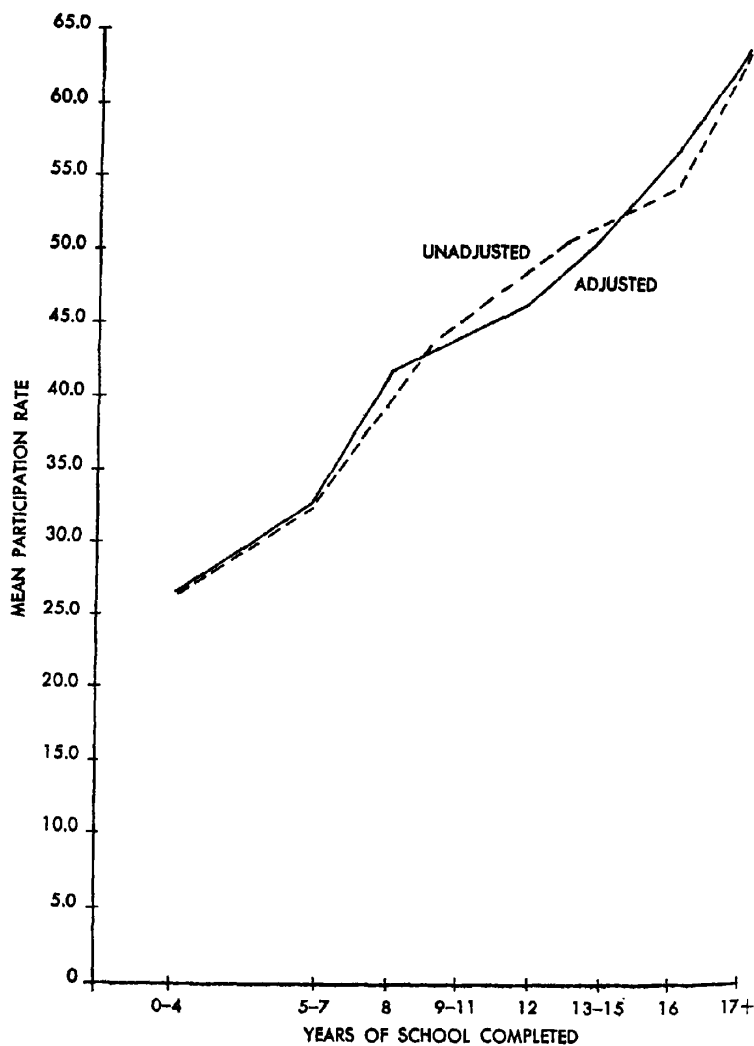
FIGURE 2
RELATIONSHIP BETWEEN EDUCATIONAL ATTAINMENT AND LABOR FORCE PARTICIPATION, MALES 65-74, CENSUS WEEK, 1960

our control variables are in order. First, other family income has a very considerable impact. All of the coefficients are negative and highly significant, which means that, compared with the group having less than $1,000 of other income, the participation of those having at least $1,000 is much reduced. Also, the general pattern of the coefficients indicates that the more other income an older person has, the less likely he is to participate, although, as in the case of the prime-age males, the relation is not perfectly regular.

Race does not matter here, as it did in the case of the prime-age run, but marital status really matters. Older males who were married had an average participation rate roughly 16 points higher than that for other males. Perhaps those who are still married by this age are unusually well endowed with those personality characteristics conducive to employment; or perhaps they are especially anxious to get out of the house and into the labor market; or perhaps it is just that they have greater financial obligations.

Turning now to the educational attainment coefficients, there is very little to be said. The graph speaks for itself. Educational attainment has a very powerful and amazingly regular effect on the labor force participation of this group. The adjusted participation rate for older males with some graduate work is almost 2½ times as high as the adjusted rate for the 0-to-4-years-of-school-completed group.

It is surprising that the unadjusted and adjusted curves are nearly identical, given the power of the control variables. The apparent explanation is that our control variables have almost exactly offsetting effects here. The low education group, for example, has less other income, and hence a greater incentive to participate on this count; but it also has a smaller proportion of its population in the married-with-wife-present category, and hence less of an incentive to participate on this ground.

Married Women (18-64)

Our sample of married women was specified to include only married women, 18-64, with husband present who had no children under 6. We were also careful to exclude women under 35 who were still enrolled in school. We used two control variables: (1) a race variable (white = 1, Negro = 0), which we expected to be negative in the case of this group because of the tradition of the Negro population in favor of the wife working; and (2) a husband's income variable, which we expected to be negatively related to labor force participation. Both of these expectations were sustained at the 99 percent level of significance. We find that white married women have a participation rate appreciably lower than Negro married women, other things equal, and that husband's

income has a pronounced negative effect on labor force participation, especially above the $5,000 level.

Educational attainment again has a definite positive effect on labor force participation, in both the simple and multiple runs, as the bottom panel of Table 1 and Figure 3 indicate. In the case of this group, however, the curve showing the adjusted participation rates is considerably steeper than the curve showing the simple association. The explanation is that the fraction of wives who are Negro and the fraction with impecunious husbands both tend to be higher among married women with little schooling than among those with much, and both of these factors tend to increase participation.

The rise in the adjusted mean participation rates associated with higher educational attainments by this group is truly impressive. From an adjusted rate of 22 percent for women in the 0-4 years of school interval, expected participation rises to 33 percent for those with an elementary education, to 50 percent for those with a high school degree, to 61 percent for those with a college education, and finally to 76 percent for women with one or more years of graduate school. The high expected participation rate for women with some graduate work is, incidentally, relevant to the discussions going on at Princeton and elsewhere with regard to policies governing the admission of women to graduate programs.

Summary Observations

Each step up the educational attainment ladder is associated with some increase in labor force participation for prime-age males, older males, and married women. The remarkable regularity of this pattern is certainly the most striking characteristic of the three sets of partial regression coefficients (or adjusted means) shown in Table 1.

It is also evident, however, that the responsiveness of participation rates to education attainment varies appreciably among our three groups. Roughly speaking, the completion of an extra year of schooling is associated with an average increase of about 3 to 3½ percentage points in the participation rate of our sample of married women and with an average increase of about 2½ percentage points in the participation rate of our group of older males. The education-participation relation for prime-age males is so nonlinear that no similar single-number measure of sensitivity would be meaningful; but, as was to be expected, the participation for members of this group is much less responsive to educational attainment than is the participation of married women and older males. Indeed, differences in participation among prime-age males in the educational attainment categories from 9-11 years of school completed on up the scale are so small that they are probably nonsignificant.

80.0
75.0
70.0
65.0
60.0
55.0
50.0
45.0
40.0
35.0
30.0
25.0
20.0
15.0
10.0
5.0
0
MEAN PARTICIPATION RATE
ADJUSTED
UNADJUSTED
0–4
5–7
8
9–11
12
13–15
16
17+
YEARS OF SCHOOL COMPLETED
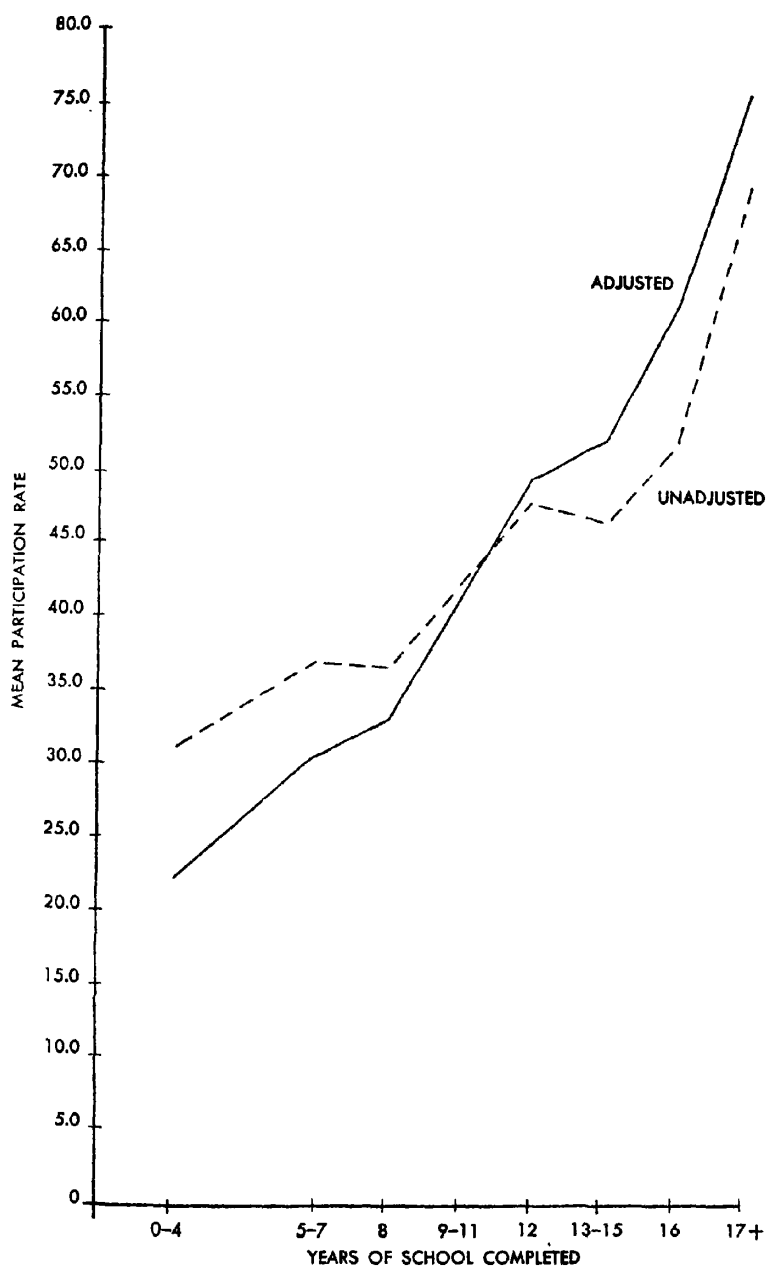
FIGURE 3

More generally, certain stages in the educational process seem to be especially important for the labor force participation of particular groups. Having spent four or more years in college, for example, seems to make a dramatic difference in the participation of married women, a considerable difference in the participation of older males, and very little difference for prime-age males. Having 5-7 years of schooling instead of 0-4 makes a substantial difference for all three groups. This is not surprising, since functional literacy is normally attained prior to the fifth year of school. However, we should also recognize that this particular schooling differential may be closely associated with differences in basic intelligence and health. Completing elementary school has an important effect on the labor force participation of older males, but is not especially important for the other two groups. It is not clear to us whether this finding is related to the kinds of jobs older men hold or to the period in history when they were being educated. Perhaps completing elementary school half a century ago was much like completing high school, or even college, today. This is but one of a number of interesting questions which deserve much more attention than we have been able to give them in this paper.

APPENDIX

I. The multiple regression equations which we estimated are of the form:

$$L/P = \alpha + \sum_{i=1}^{7} \beta_i E_i + \sum_{i=1}^{11} \gamma_i Y_i + \theta R + \phi M$$

where:

L/P = labor force participation rate dummy (1 if in the labor force, 0 if not)

E_i = set of educational attainment dummies, E_1 = 5–7 years of school completed, E_2 = 8 years, · · · E_7 = 17+ years (1 if in the class, 0 if not)

Y_i = set of "other family income" dummies (husband's income in the case of married women), Y_1 = \$1,000–1,999, Y_2 = \$2,000–2,999, · · · , Y_{11} = \$25,000+ (1 if in the class, 0 if not)

R = race dummy (1 if white, 0 if Negro)

M = marital status dummy (1 if married with spouse present, 0 otherwise)

$\beta_i, \gamma_i, \theta, \phi$ = partial regression coefficients

α = intercept, which in this case is the estimated participation rate for the "reference group" (persons who had all of the following characteristics: 0–4 years of school completed; less than \$1,000 of other family income; member of the Negro race; was not married with spouse present).

II. The t values used to test for the significance of differences between any two educational attainment regression coefficients are presented below:

Males 25–54

Years of school completed:	0–4	5–7	8	9–11	12	13–15	16	17+
0–4		6.31*	8.45*	10.65*	11.59*	11.89*	11.38*	11.04*
5–7			2.77*	5.66*	7.07*	7.65*	7.13*	6.79*
8				2.86*	4.38*	5.38*	5.04*	4.78*
9–11					1.70	3.30*	3.13*	2.96*
12						2.04†	2.03†	1.93
13–15							0.24	0.26
16								0.03

Males 65–74

Years of school completed:	0–4	5–7	8	9–11	12	13–15	16	17+
0–4		2.52†	6.25*	6.09*	6.69*	6.69*	6.44*	6.41*
5–7			3.82*	4.04*	4.75*	5.10*	5.18*	5.38*
8				0.90	1.77	2.69*	3.29*	3.86*
9–11					0.80	1.83	2.59†	3.30*
12						1.12	2.02†	2.82*
13–15							1.02	1.95
16								0.99

Married Women 18–64

Years of school completed:	0–4	5–7	8	9–11	12	13–15	16	17+
0–4		3.20*	4.36*	7.83*	11.16*	11.12*	12.79*	13.51*
5–7			1.50	6.69*	11.95*	11.08*	12.65*	12.88*
8				5.94*	12.28*	10.87*	12.40*	12.52*
9–11					6.95*	6.57*	9.13*	10.28*
12						1.67	5.62*	8.00*
13–15							3.94*	6.89*
16								3.93*

* Significant at 1 percent.
† Significant at 5 percent.

III. The regression coefficients for the control variables, which were omitted from Table 1, are given below, with their (exaggerated) standard errors in parentheses.

Other Family Income Categories	Prime-Age Males Regression	Older Males Regression	Married Women Regression
$ 1,000– 1,999	− 2.5 (0.4)	−29.0 (2.3)	—
2,000– 2,999	− 2.4 (0.4)	−36.3 (2.5)	—
3,000– 3,999	− 1.6 (0.4)	−28.8 (3.2)	—
4,000– 4,999	− 2.8 (0.5)	−32.8 (3.5)	—
5,000– 5,999	− 1.9 (0.7)	−24.8 (3.7)	—
6,000– 6,999	− 4.4 (0.8)	−31.7 (4.1)	—
7,000– 8,999	− 5.6 (0.8)	−38.6 (3.8)	—
9,000–10,999	0.2 (1.2)	−29.1 (5.0)	—
11,000–14,999	− 6.4 (1.3)	−42.0 (5.5)	—
15,000–24,999	− 5.8 (2.0)	−43.2 (7.7)	—
25,000+	−19.3 (4.5)	−41.4 (16.1)	—
Husband's Income Categories			
$ 1,000– 1,999	—	—	− 9.3 (2.6)
2,000– 2,999	—	—	− 6.2 (2.4)
3,000– 3,999	—	—	− 4.6 (2.3)
4,000– 4,999	—	—	− 5.8 (2.2)
5,000– 5,999	—	—	−10.7 (2.2)
6,000– 6,999	—	—	−15.6 (2.3)
7,000– 9,999	—	—	−23.4 (2.2)
10,000–14,999	—	—	−36.3 (2.6)
15,000–24,999	—	—	−47.8 (3.2)
25,000+	—	—	−54.4 (3.7)
White	1.6 (0.4)	− 0.3 (3.1)	− 8.3 (1.6)
Married, Wife Present	7.5 (0.3)	16.4 (1.8)	—

IV. The adjusted mean participation rates shown in Table 1 and plotted on Figures 1–3 were calculated as follows: (1) We obtained an adjusted intercept by multiplying the regression coefficient for each control variable by its mean value (the percentage of the total sample in the category) and adding the sum of these products to the "raw" intercept, α. This ad-

justed intercept is an estimate of what the participation rate for the group of persons with 0–4 years of school completed would have been had their group included the sample-wide average of whites, persons married with wife present, etc. (2) We then added the regression coefficient for each educational attainment dummy to the adjusted intercept to obtain the adjusted mean participation rate for each educational attainment category.

This is an "additive" method of standardization, since it makes the same absolute allowance for, say, white-Negro differences in participation at all educational attainment levels. It is possible to avoid this rather restrictive assumption by introducing "interaction" dummies but the number of parameters to be estimated grows astronomically. Also, on the basis of checks of some detailed cross-tabulations, we doubt seriously that this kind of refinement would have had an appreciable effect on our results.

SKILL, EARNINGS, AND THE GROWTH OF WAGE SUPPLEMENTS*

By Robert G. Rice
University of Chicago

Since 1943, aggregate employer expenditures for private pension and welfare benefits have grown from about three-quarters of 1 percent to over 5 percent of wages and salaries in private industry. This paper is based upon a study of some determinants of such private wage supplements. In the available time, I can do no more than describe the approach and highlight the findings, but I expect to present more details in the near future.

I

Among the factors most frequently cited in the literature as having had important effects on the growth of supplements are (1) preferential treatment under the federal personal income tax, (2) savings of group purchase, (3) turnover costs, and (4) unionization. I shall discuss each of these in turn.

Tax preference has created special markets for annuities and insurance—markets to which individuals have had access only as employees and only to the extent that purchases are paid for directly by employers. A progressive tax affording preferential treatment for purchases of particular goods in special markets and exempting some levels of money income from taxation creates levels of total compensation at and below which prices in the ordinary and special markets are the same. Above these compensation levels, effective prices in the special markets vary negatively (relative to prices in ordinary markets) as marginal tax rates vary positively with money income. The elasticities of wage supplements with respect to earnings therefore are not true income elasticities, because, in addition to income effects, they contain substitution effects resulting from the tax treatment. Given our tax structure, theory would predict that observed earnings elasticities of supplements would be greater than the income elasticities of the goods for which supplements are substitutes. Further, since supplement expenditures are in terms of prices paid by employers rather than effective prices paid by employees, the earnings elasticities of supplement

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expenditures also would be greater (*ceteris paribus*) than true income elasticities. Of course, the more price-elastic is the demand for the goods for which supplements are substitutes, the greater will be the earnings elasticity of supplement expenditures relative to the income elasticity of the corresponding goods.

Rising productivity, and hence income growth, thus might have been expected to lead to an increase over time in supplement expenditures, and, given a sufficiently high income elasticity of demand, to an increase in the ratio of supplements to earnings. Fragmentary evidence suggested that, in ordinary markets, income elasticities of the goods for which the bulk of supplements are substitutes might be on the order of one.[1] Consequently, an observed earnings elasticity of supplements substantially greater than one, as is apparent in time series data, would appear to be consistent with the expected effect of preferential tax treatment.

Special markets also have been created in employing firms because of differences between group and individual prices of employee benefits. Beyond meeting whatever requirements may exist to qualify for group prices, it is not obvious that these price differences provide a strong incentive to have the benefits take the form of wage supplement expenditures by employers rather than payroll deductions. However, the observed tendency toward payment of the full cost of various employee benefits by employers (i.e., toward noncontributory plans) may reflect the combined effects of group prices, preferential tax treatment, and rising incomes.

In an analysis of private wage supplements, these price differentials obviously should be taken into account, since supplement expenditures are in terms of group prices, and group prices surely are not constant either absolutely or relative to prices in ordinary markets. There is quite a bit of evidence that, at least over a rather wide range of firm sizes at a point in time, group prices vary negatively with the size of the group for which purchases are made. For this reason, firm size might serve as a proxy for group relative to individual price, provided other influences were not strongly reflected in firm size variations.

[1] A substantial proportion of individual saving is probably undertaken for the purpose of providing for maintenance of consumption during retirement and periods of nonemployment or reduced earnings during working years. Unless the income elasticity of individual saving for other purposes differs greatly from that of total saving, the income elasticity of saving for which some supplements are substitutes would tend to be close to that of total saving, which Friedman's findings suggest may be unitary [6]. Estimates of the income elasticity of family payments for life insurance premiums, derived from two sets of data [7] [9] were close to one. Feldstein has estimated the income elasticity of family payments for health insurance to be somewhat less than one [5]. The life and health insurance data were in the form of expenditures and for this reason, as well as others, yield questionable estimates of income elasticities. However, I found no evidence of elasticities much greater than unity.

Abstracting from substitutability between employer contributions (wage supplements) and employee contributions, the effect of differentials between group and individual prices upon expenditures for supplements is uncertain, since it depends upon the relationship between price and firm size and the elasticity of demand with respect to price.[2] If price varies negatively with firm size, a price-elastic demand would be reflected in a positive association between supplement expenditures and firm size; an inelastic demand would produce a negative association. The oft observed positive variation of supplement expenditures with firm size therefore might reflect a price-elastic demand, and the secular growth of supplements might be attributable in part to growing differentials between group and individual prices.

Tax preference and group prices might lead us to expect growth of supplements because of price and income effects. The former influences effective prices paid by employees, but not observed prices paid by employers. The latter affects both, presumably to the same extent.

Still another factor may have been operating upon employers' effective supply prices of supplements, though not on observed prices. It has often been argued that private wage supplements—and especially such supplements as nonvesting pension plans—are an effective means of reducing turnover rates, and therefore turnover costs. Jacob Mincer's estimates [8, p. 57] indicate that aggregate annual investment in on-the-job training by employees more than doubled between 1939 and 1958. It is conceivable that part of this increase reflected a shift in training costs from employers to employees. However, it seems reasonable to assume that employer investment in workers, and hence potential turnover cost, has increased substantially over time, probably to an aggregate level of at least several billion dollars a year by the late 1950's [8, pp. 62-63]. Consequently, expenditures by employers to reduce turnover rates might have been expected to rise. If supplements are more effective than higher wages in reducing turnover rates, the secular increase of supplement expenditures relative to money earnings may be attributable, in part, to rising potential turnover costs, due in turn to the growth of skills specific to the firm acquired on the job.

The role of unions in the development of pension and welfare benefits has often been stressed. Albert Rees, for example, has suggested that the secular growth of private wage supplements may reflect the combined effects of unionization, preferential tax treatment, and turn-

[2] Specifically, the elasticity of supplement expenditures with respect to firm size (η_{sf}) is given by

$$\eta_{sf} = \eta_{pf}(1+\eta_{sp}),$$

where η_{pf} is the elasticity of price with respect to firm size and η_{sp} is the elasticity of demand with respect to price (without sign reversal of the latter).

over costs [13]. The growth of supplements has coincided with the drive by unions to secure pension and welfare benefits for their members, and it has been observed that supplement expenditures are higher, both absolutely and relative to money earnings, in plants where workers are organized than where they are unorganized. What we should like to know is whether unionization is associated with wage supplement expenditures higher than might be expected on other grounds.

II

The BLS 1959 study of supplements for production workers in nineteen two-digit manufacturing industries [2] provided some basic data which could be used with data from other sources for cross-sectional tests of various hypotheses regarding private wage supplements. Measures of supplements used were industry averages of plant-man-hour expenditures for (1) pension and retirement plans, (2) health, accident, and life insurance, (3) pension and insurance expenditures combined, and (4) all private wage supplements.[3] Two measures of the prevalence of private wage supplements in these industries were also used: the proportion of workers employed in establishments reporting expenditures for pensions and the corresponding proportion for health, accident, and life insurance.

Average earnings rates were derived from the published data on supplements and unpublished data furnished by BLS. For firm size, the average number of employees per company in 1954 was used. The proportion of workers employed in establishments in which the majority were covered by collective bargaining agreements in 1958 was used as a measure of the degree of unionization.

In the absence of data on turnover costs, the influence of such costs on wage supplements had to be estimated indirectly. One approach to estimating this influence is to use a measure of skill. Skill reflects general as well as specific training, of course, but Jacob Mincer's findings suggest a positive association between the two [8].

An index of skill was developed based upon the "richness of skill mix" used by Melvin Reder [11]. Since this measure utilizes nationwide median annual earnings in occupational categories, it is, as might be expected, rather highly correlated with actual earnings rates. For this reason, two alternative measures of skill were also developed. In

[3] The latter were derived from the published series of expenditures for private welfare plans by excluding the item year-end and Christmas bonuses, which was considered part of money income. In addition to pension and insurance expeditures, all private wage supplements include a number of quantitatively minor welfare plans such as supplemetary unemployment benefits, as well as other categories of expenditures such as "Private plans not reported separately," which are quantitatively important in some industries and might account for certain peculiarities in the results of the analysis. (In subsequent supplement studies, including the recently published survey of manufacturing industries in 1962 [3], BLS has redefined private welfare plans to exclude year-end and Christmas bonuses.)

the first, educational attainment (median years of schooling com pleted) was substituted for earnings in the Reder-type index. In th second, the measure of skill composition was the ratio of craftsmen foremen, and kindred workers to all production workers in the indus try. The correlations with earnings rate of these skill measures wer somewhat lower than that of the "richness of skill" mix. For all thre measures of skill, *1960 Census of Population* data were used.

A different approach to estimating indirectly the influence of turn over costs on wage supplements utilizes turnover rates. Gary Becke has shown that both quit and layoff rates will tend to be negativel correlated with specific training [1]. If turnover rates are negativel related to the amount of training and therefore are an indicator of po tential turnover cost and if private wage supplements are an effectiv means of protecting employer investment in workers, then supplement should be negatively related to the rate of turnover. For the turnove rate variable, the combined quit and layoff rate was used.

III

Simple correlations showed supplements varying positively wit earnings rate, firm size, and unionization, and varying in the expecte direction with the turnover cost proxies (positively in the case of ski and negatively in the case of turnover rate). However, the skill mea sures were more highly correlated with insurances than with pension in terms of both expenditures and prevalence. Of the turnover cos proxies, only turnover rate was more closely related to pension than t insurance expenditures, but the correlation between turnover rate an the prevalence of insurance was slightly greater than that betwee turnover rate and the prevalence of pensions. This was an unexpecte result, since it seems reasonable to expect turnover costs to be mor closely related to pensions (which typically are nonvesting or provid for considerably delayed or incomplete vesting) than to other supple ments. (Supplements other than pensions may impose some costs upo workers who quit their jobs, however, and therefore may bear som relationship to costs of turnover.)

Gross correlations provide no evidence, of course, either of indepen dent effects or of the hypothesized effect of preferential tax treatment In an attempt to sort out various influences, multiple regression anal yses were run of each supplement measure on earnings, firm size, de gree of unionization, each of the four turnover cost proxies in tur and, for a set of fourteen of the industries for which data were avail able, age and sex composition of the labor force.

Except in the case of life and health insurance expenditures alon (where the results were inconclusive), the multiple regressions re vealed a consistent positive variation between supplement expenditure

and both earnings rate and firm size. Neither unionization nor the turnover cost proxies had a statistically significant[4] independent influence on supplement expenditures. None of the turnover cost proxies were significantly related to the prevalence of either pensions or insurances. Unionization was found to have a significant positive effect on the prevalence of pensions, but not on the prevalence of insurances. Neither age nor sex composition of the labor force was significantly related to any of the supplement measures.

Net elasticities of supplement expenditures with respect to earnings generally were substantially higher than one, except in the case of the life and health insurance expenditures variable. The multiple regressions explained a substantial proportion of the variance of supplements in every case, but the coefficients of simple determination between earnings rate and the various supplement measures were in most cases not much lower than the coefficients of multiple determination. Thus, the first stage of the analysis suggested that (1) wage supplements generally vary positively with money earnings independently of any of the other factors considered, (2) variations in wage supplement expenditures can be explained largely in terms of variations in money earnings, and (3) the manner in which supplements vary with earnings appears to be generally consistent with the expected effect of preferential tax treatment.

IV

The significance of earnings rate in the BLS 1959 cross-section (earnings rate alone explained over 70 percent of the variance in expenditures for all supplements) suggested further investigation of the supplement-earnings relation. The apparent general consistency of earnings elasticities with the tax hypothesis suggested that data be examined on an annual rather than an hourly basis, since, if preferential tax treatment is a determinant of supplement expenditures, annual income, not hourly rate, is relevant.

Accordingly, the BLS manufacturing industry data for 1959 were analyzed on an annual basis. In addition, annual data from a number of other cross-sections were analyzed. These included: (*a*) production workers in eighteen manufacturing industries in 1962 (BLS, [3]), (*b*) production workers in seventeen manufacturing industries common to both 1959 and 1962 surveys (BLS, [2] [3]), (*c*) employees in twenty manufacturing and nonmanufacturing industries in seven biennial surveys, 1949 to 1961, in nineteen of these industries in 1963, and in eighteen of the industries in all surveys from 1949 to 1963 (Chamber of Commerce [4]),[5] (*d*) employees in fourteen manufacturing

[4] Statistical significance at the .05 level was the criterion used throughout.

[5] The hotel industry, not included in the 1963 survey, was excluded from data for other years. In addition, the category "Miscellaneous Industries" was excluded from all surveys

industries in 1949, 1957, and 1963, first differences in these industries from 1949 to 1957 and from 1957 to 1963, and pooled first differences 1949-57 and 1957-63 (Chamber of Commerce [4]), (*e*) employees in seventy-eight selected firms included in the Chamber of Commerce 1961 survey (unpublished data).

The unpublished data referred to in (*e*) made possible analyses of the relation between supplements and earnings with both firm size and employee contributions taken into account. Additional unpublished data furnished by the Chamber of Commerce made it possible to analyze data referred to in (*d*) with firm size included.

These analyses revealed that supplements are indeed quite systematically related to annual earnings in a manner apparently consistent with the tax hypothesis, after taking account of various other factors that may have been at work and that these other factors generally add little to the analyses. Further, in comparable cross-sections for different years, although the relation between particular types of supplements and earnings varied, there appeared to be a very stable relation between earnings and expenditures for all supplements. For all supplement expenditures, the analyses of comparable cross-sections produced regression lines which were practically coincident over the observed ranges of annual earnings.

V

If the high earnings elasticity of wage supplement expenditures does reflect the effect of preferential tax treatment, it is attributable to the two aspects of the tax structure mentioned in section I: (1) some income is not subjected to taxation and (2) marginal tax rates vary positively with taxable income. To take into account the first factor, the earnings elasticities of expenditures for all supplements from all cross-sections were converted into estimates of elasticity with respect to taxable earnings by applying the standard deduction and assuming the number of exemptions to be the national mean number of persons per consumer unit in the year of each cross-section.[6]

Almost all of these estimates of taxable earnings elasticities were close to one. Thus, it appeared that in several cross-sections spanning

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because it includes railroads, in which private wage supplements do not reflect pension expenditures in the form of Railroad Retirement Tax.

[6] The conversion formula used was:

$$\eta_w' = \left[1 - \left(\frac{\bar{N}X}{\bar{W}}\right)\left(\frac{1}{1-z}\right)\right]\eta_w$$

η_w' = estimated elasticity W.R.T. taxable earnings
η_w = elasticity W.R.T. total earnings at means of cross-section
$\bar{N}$ = national mean number of persons per consumer unit *n* year of cross-section
X = exemption per person under the federal personal income tax
$\bar{W}$ = mean total annual earnings in cross-section
z = the standard deduction (10 percent).

a period of fifteen years, wage supplement expenditures had been roughly proportional to taxable earnings. This is what we might expect to observe in full adjustment if (*a*) all earnings in excess of some non-taxable amount were subjected to taxation at the same rate, (*b*) wage supplements were accorded preferential tax treatment, (*c*) the goods for which wage supplements were substitutes had a true income elasticity of unity, and (*d*) other factors exerted only marginal influences on wage supplements.

It appears so far that (*c*) and (*d*) may be true, and (*b*) is true. It is not true that all taxable earnings are subjected to the same marginal rate, but rates higher than the minimum (non-zero) rate apply to relatively few taxpayers. In all of the earnings data examined, for only one observation was the annual earnings level such that a family of average size would have been taxed at a marginal rate higher than the minimum non-zero rate. These were, of course, averages and consisted only of earnings of individual family members (and possibly not the entire earnings of these individuals), not total family income, but most observations were sufficiently below the level at which rates higher than the minimum marginal rate would apply that it seems not particularly unreasonable to assume the minimum non-zero rate to have been the relevant rate applicable to taxable earnings in each cross-section.

VI

The finding that wage supplement expenditures seemed proportional to estimated taxable earnings in cross-section suggested examination of the relation between supplements and taxable annual earnings in time series. Unpublished supplement data furnished by Albert Rees, in combination with his published data on money earnings [12], and published BLS *Employment and Earnings* data, made it possible to examine the relation for production workers in manufacturing. The Department of Commerce aggregates were used to examine the relation in the economy as a whole.

The Commerce Department data, placed on a per-worker basis, revealed that annual employer expenditures for private pension and welfare funds had been nearly proportional to estimated annual taxable earnings from the middle of World War II to 1963. Changes in the tax rate structure (the minimum non-zero rate was used as a proxy for the tax structure), in average firm size and in degree of unionization of the labor force had little influence, and standardizing for these factors did not alter the basic finding of near proportionality between supplements and estimated taxable earnings over a period of some two decades. Similarly, wage supplements were found to be closely related to and roughly proportional to estimated taxable earnings for production

workers in manufacturing from World War II to 1957. In addition, turnover rate was included in one analysis of Rees's data, and contributed virtually nothing to the explanatory power of a model containing taxable earnings, tax rate, firm size, and degree of unionization which, in turn, was only a little more powerful than a simple regression model based upon the relation between supplement expenditures and taxable earnings alone.

VII

All data examined suggested that wage supplement expenditures are closely related to money earnings, that the manner in which such expenditures vary with money earnings appears consistent with the expected effect of preferential treatment under the federal personal income tax, and that variations in supplement expenditures, both secularly and in cross-section, can be explained largely in terms of income effects compounded by the effects of preferential tax treatment.

However, I must qualify this statement. Use of the standard deduction induced a systematic upward bias in the estimation of taxable earnings, in both cross-sections and time series. Actual use of the standard deduction varies negatively with income in a given year, and its prevalence has declined from some four-fifths of all returns at its inception in 1944 to perhaps one-half now. Thus, my estimates consistently understated the true elasticity of wage supplement expenditures with respect to taxable earnings, and this elasticity therefore might be somewhat greater than one.

For an explanation of this phenomenon, I return to the expected effect of potential turnover cost on wage supplements. It is possible that potential turnover cost is poorly correlated with the proxies for it used in the first stage of the analysis and that, consequently, the effects of such cost on supplements were not accounted for by these proxies. Walter Oi has presented some evidence that turnover cost varies positively with earnings and, further, that the ratio of such cost to earnings increases with earnings [10]. In fact, Oi assumed earnings rate to be a proxy for turnover cost in his empirical analysis.[7] Under this assumption, my findings might be interpreted as reflecting a positive association between wage supplements and turnover cost, with the high total earnings elasticities reflecting a ratio of turnover costs to earnings varying positively with earnings.

Furthermore, it is possible that part of the effect of potential turn-

[7] Interestingly, in the BLS manufacturing industry data for 1959, earnings rates were more highly correlated with pensions, both expenditures and prevalence, than with insurances. Thus, the behavior of earnings rate was more consistent with the turnover cost hypothesis than was the behavior of any of the turnover cost proxies used in the analysis. Also, the skill measures were more highly correlated with earnings rate than with the supplement measures.

over cost is reflected in the relation between supplements and firm size. This relation might reflect the positive association between the amount of specific training and size of firm as well as a negative association between firm size and price. Such an interpretation of the results would seem to be supported by the fact that the positive association between supplements and firm size in cross-section was much more consistent and far more significant when pension expenditures, either alone or in combination with other supplements, were included in the analysis. Firm size was much more closely correlated with pensions than with insurances, in both the expenditures and prevalence measures. This, too, might reflect consistency with the turnover cost hypothesis.

Preferential tax treatment appears to account for much of the growth of wage supplements relative to money earnings in recent years. However, to the extent that the elasticity of supplements with respect to earnings exceeds unity, the effects of preferential tax treatment and other factors having been accounted for, we may be observing a phenomenon reflecting, at least partly, the relation between earnings and potential turnover costs. Unfortunately, this could be no more than a guess without turnover cost data.

VIII

Regardless of the extent to which it is attributable to preferential tax treatment or turnover costs, the finding that wage supplement expenditures vary systematically with money earnings and that the ratio of such expenditures to total earnings varies positively with earnings level, in cross-section as well as through time, has an obviously important research implication. It means that relative money wage differentials progressively understate differentials in compensation, as measured by the sum of money earnings plus wage supplement expenditures. Skill margins based only upon money wage differentials therefore are biased downward, unless one wishes to argue that private wage supplements are not part of employee compensation. Otherwise, it is clear that we should not examine money wage differentials assuming that private wage supplements are proportional to total wages. Such expenditures may turn out to be roughly proportional (*ceteris paribus*) to taxable earnings, but this is very different from proportionality between supplements and total earnings.

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DISCUSSION

W. LEE HANSEN: Research on patterns of labor force participation has grown steadily over the past half-dozen years, owing largely to the stimulus of Mincer's path-breaking work. Of this research, the most ambitious and systematic is that of Bowen and Finegan, who have already reported a number of findings. Whereas their earlier research studied the effect of unemployment on participation patterns, they now explore the impact of differential educational attainment on participation patterns.

Among all three of the age-sex groups examined—prime males, older males, and married women—participation rates are positively associated with educational attainment, even after the introduction of selected control variables, such as race, marital status (for males), and income. However, the overall strength of these associations varies among the three groups: among prime males, for example, the strength of the association steadily declines with movement up the educational ladder.

Two questions about these findings immediately come to mind. First, to what extent do they reveal underlying differences in the patterns of participation? And second, what explains the differences which are observed? With respect to the first question, there are several possibilities: (*a*) the results do reflect the underlying differences; (*b*) they reflect an accentuated trend of labor displacement—the structuralist argument; and (*c*) they reflect short-run influences of above-average cyclical unemployment, manifested in "hidden" unemployment or in the "discouraged" worker phenomenon. There is no evidence which would encourage us to take the structuralist argument (*b*) very seriously in this context. However, something might have been done to isolate the short-run influences (*c*) by adding in an unemployment control variable. Unemployment might be at least partially controlled through the use of occupational unemployment rates, in the absence of data for geographic labor markets. Since unemployment rates vary inversely with educational attainment and presumably do so in an accentuated way when unemployment is above average, they would very likely explain some part of the observed differences. Conceivably, they might also shed some light on the nonlinear relationship observed for prime males.

Regarding the second question and point (*a*) above the underlying forces—intelligence, health, and so on—are a bit harder to isolate. Since health information is not obtainable from the census sample, recourse must be made to other such bodies of data to piece together any fuller explanation. For example, some of the data from the reports of the National Center for Health Statistics Survey might be useful. And the Current Population Survey has at times provided breakdowns of the reasons for part-year or nonwork during the year; illness and disability usually figure importantly. Before leaving this point, let me suggest one other type of control. In general, participation rates decline with age for males, while they fall, rise, and then fall with age for married females. Since average educational attainment tends to fall with age,

it might be useful to further control by age within groups, e.g., prime males and married women, thus better isolating the effect of educational attainment.

The operation of the income control variables is about what might be expected. But I suspect that for males the difference between their total income and their earnings may be a more powerful explanatory variable than the difference between total family income and their (male) earnings. The implicit assumption here is that males work or do not work, depending upon their own rather than upon their wives' resources. As it is, the Bowen-Finegan model makes wives' participation a function of their husbands' incomes; but it then makes the participation of prime males (most of whom are married) in part a function of their wives' participation, which shows up in total family income. Were this alternative version of the model tried, we might find that the income control variable would emerge more strongly and some of the indeterminancy of the model would be eliminated. An even stronger income control variable might be obtained by adjusting income for weeks worked.

If we accept the major findings of the paper, i.e., the importance of educational attainment on labor force participation patterns, then several paradoxes emerge when these findings are contrasted with time series results. First, among prime males the labor force participation rates from the early post-World War II period to the present have been roughly unchanged at about 96 percent. At the same time, median years of school completed rose substantially—from about 10.5 to 12.2 years between 1952 and 1964. Yet, the 1960 cross-section results would have led us to predict a rise in the participation rate during this period, as a result of educational upgrading. Second, among married women the labor force participation rate has roughly doubled since 1947. At the same time, median years of school completed rose far less than for prime males from about 11.7 (this is an estimate of my own) to a recorded 12.3 years between 1952 and 1964. Here the cross-section results would have led us to predict a large increase in the married women participation rate; yet the presumed "causal" factor, increased educational attainment, was notably weak during the period. And so in these two cases it becomes difficult to know exactly how to interpret the cross-section results presented in the paper.

All of this takes us back to an earlier point: what factors account for the observed differences in participation rates? On the one hand, the results may be affected strongly by cyclical factors. On the other hand, some other sorting devices may be at work which then manifest themselves through the educational system. What these sorting devices are is of course the key issue.

Rice attempts to explain the growth of wage supplements over the past several decades. The cross-section results show that the net elasticity of supplement expenditures relative to earnings is generally higher than one. At the same time, the time series data reveal that supplements have grown relative to earnings from mid-World War II until 1963.

However, to throw light on the preferential tax hypothesis, Rice reestimates the cross-section elasticities with respect to taxable earnings, producing elasticities which on average approximate unity. He then finds this consistent with adjusted time series data which reveal that supplements as a proportion of taxable income have been constant since the middle of World War II.

What seems to be critical in this analysis is the way in which the elasticities with respect to earnings are translated into elasticities with respect to taxable earnings. This translation involves applying the standard deduction and average exemptions to "correct" the elasticities. But given the way in which the use of the standard deduction may vary among different groups in the cross-sections (i.e., workers in different industries with different mean earnings), possible variations in numbers of family members, and the extent to which dependent exemptions can be used, a more refined method of adjustment would strengthen our confidence in the values of the adjusted elasticities.

Rice then indicates the conditions under which the resulting proportionality holds. These are: (*a*) all earnings in excess of some nontaxable amount were subjected to taxation at the same rate; (*b*) wage supplements were accorded preferential tax treatment; (*c*) the goods for which wage supplements were substitutes had a true income elasticity of unity; and (*d*) other factors exerted only marginal influences on wage supplements. Condition (*b*) is true, and (*c*) and (*d*) may be true, as noted. Condition (*a*) is certainly debatable, as is the assumption that the data may reveal a condition of full adjustment. On (*a*), however, one might argue that additional taxable earnings are not subjected to taxation at the same marginal rate. If, for example, the number of exemptions is similar for all groups and therefore some constant amount, the fact that average tax rates rise with respect to income would seem to suggest that marginal tax rates rise even faster. In view of this, I would prefer some harder evidence than the "plausible" line of argument Rice advances.

The finding that unionism seems to have little or no effect on supplements is somewhat surprising, or so it would seem to me. Of course, it is possible that new supplement programs first won by unions are quickly gained by nonunionized groups as well. If so, then the chance of picking up such an effect in cross-section data is greatly reduced. But should this not be the case, one might have expected unions to be in the vanguard in securing even greater additions to the wage package through the substitution of supplements for money increases.

One of the most important conclusions we can draw from this paper is that even greater caution is required in all analyses that involve relative earnings comparisons. We are already aware of a number of problems; being alerted to this additional one is most useful.

ROBERT EVANS, JR.: These three papers impressed me as being very good. They are representative of what might be termed "The New Labor Market Analysis." The new labor market studies seek to bring up to date and broaden our understanding of labor market phenomenon which until recently has been largely based upon studies by George Shultz, Charles Myers, Richard Lester, and others completed shortly after the second World War. This new approach represents an improvement over the old because it generally seeks to accommodate both "labor," following Alfred Marshall's comment about bricks, sewers, and laborers, and "markets in the tradition of neoclassical economics" (or should one say Adam Smith's economics). Mr. Rees's com-

ments concerning an earlier and to some extent continuing failure to take proper cognizance of alternative models against which it is appropriate to test labor market operation are illustrative of this change, and a very beneficial change it has been.

In turn, the new analysis has its own weaknesses, a few of which may be evident in these papers. There is a tendency to slide over, thought not without a ceremonial bow in that direction, the distinction between (1) the internal functioning of particular labor markets, that is, how it affects those within it, and (2) the process involved when an individual attempts to move across market boundaries. When reasonably efficient processes and monetary rationality within markets are found, as these papers do, the temptation is to believe that these exist equally for movements across market boundaries. Yet, my own work of recent years has impressed me with the separateness of our labor markets, so much so that I find the Japanese labor market with its permanent and nonpermanent employees a useful model for the study of some of our own labor problems. Alternatively this could be expressed as a threshold effect—a term quite appropriate for a study of labor force participation and education. Mr. Bowen and Mr. Finegan take note of this effect at several places within their paper. Perhaps, however, threshold effects are deserving of more emphasis and additional research should focus on these points rather than on participation in general. In a certain period a given level of formal education may be necessary for labor market entrance, witness Mr. Rees's illustration, but ten years later will it be formal education or work experience that will be most important? Contrary to certain popular views, let me suggest that it will be work experience.

A lack of attention to individual life styles and life cycles, so dear to the hearts of our colleagues who are students of consumption behavior, is another weakness of the new labor market analysis. We rely too much on individuals in cross-section instead of following groups of families over periods of years, with all of the pitfalls which that approach entails. In this vein I found the Bowen-Finegan results relative to the state of marriage, even when compared to those who had passed through that state, quite interesting, and certainly concur with their suggestion for a closer examination.

An emphasis upon longitudinal studies of different groups would also tend to focus attention upon other differences which may be important for labor market behavior. An example among these papers might be Mr. Rice's use of an income elasticity of one. Since total savings for working men are small compared to total savings and the same is probably true for health and life insurance, this elasticity is based upon the behavior of groups other than factory workers and may or may not be appropriate for them.

While I am impressed with Mr. Rice's research and his use of a number of alternative hypotheses, I remain unconvinced of the accuracy of one of the conclusions, that unions have played a relatively minor role. The following are some of the factors which give rise to this view. One, the overlap between industries with high wages, large-scale operation, particular types of market organization, high profit levels, and unionization is too great to give me

confidence in the ability of statistical methods to separate out the important factors. Two, this type of analysis presupposes countless individual decisions; yet the nature of interunion politics and the reality of bargaining relationships suggest that the number of really independent decisions in the area of fringes may be very small. Three, a minor point, but the fact that this audience pays higher taxes, participates more largely in contributory fringe programs, than does Mr. Rice's population, and is almost completely nonunion is not without significance.

The other two papers are but enticing previews of forthcoming books. Thus, words of guidance and comments on some of the implications are offered with some trepidation for they may already have been found wanting by other portions of the author's research. As part of the rationale for their study, Messrs. Bowen and Finegan list social policy, but here they do not relate their results to that social policy. To this reader, their results lend support to the view that the economy in general has not experienced a general shortage of formal education, and the corollary that it is unlikely that the higher than average unemployment rates observed for the uneducated stem from their inability to perform job-related functions. This latter view is reenforced by an examination of the educational levels of workers in low-wage and non-low-wage occupations within major occupational groupings like "Operatives," etc. This is not to deny that formal education may not be important at certain points or that it may not be used to allocate jobs, especially in movements across market boundaries.

This emphasis on worker quality leads back to the paper by Mr. Rees. A central point in that paper was the concept that employment practices seek to provide the employer with a maximum amount of usable information concerning worker quality, or what the employer believes measures worker quality, at a minimum of cost. Thus, we find the suggestion that the success of private employment agencies, and one might add certain nonprofit ones, in Chicago lies in their meeting the above requirement. Where then does this leave the public employment service, especially where it concerns individuals with a "high social need for employment"? The implication would not seem to be in the direction urged by some, for a requirement that "good" jobs must also be listed with the service—even economists do not require as much at this meeting. The key would seem to lie in providing "needy workers" with elements of quality which employers require. This is only partly formal education, as unemployment rates for youth attest. In part then it means the provision of successful employment experience including in all probability the subsidization of employers.

JOSEPH SHISTER: Time limitations being what they are, I shall confine my comments to only one of the three excellent papers. The principle used for selection was a very "sophisticated" one: first come, first served. And Al Rees's piece arrived first.

Some time back I was discussing the question of local labor market studies with a very knowledgeable economist. He argued that in view of the relatively numerous market studies of the 1940's and 1950's, further studies would not

represent a constructive allocation of resources; they would yield very marked decreasing returns. I disagreed with him then and disagree even more now that I have read Rees's paper.

There are several reasons justifying additional labor market research of the type being done by Rees and his colleagues. First, despite the studies of the 1940's and 1950's (and not ignoring Gladys Palmer's, *Labor Mobility in Six Cities*), we are still a very long way from knowing whether different markets operate in a significantly similar fashion where there are differences in such crucial variables as geographical location, size, industrial complexion, occupational structure, degree of unionization, etc. Second, even if we assume that the operational patterns are largely similar in the various markets, there is no assurance that the similarity remains constant over time. That is, the various local markets may all display a similar pattern in period one; they may also display a similar pattern in period two; but the pattern of period one may be significantly different from that of period two. Such a shift can be caused by the introduction of new factors into the markets and/or changed weights of existing factors. Thus, for example, the relative importance of white-collar workers is now very much greater than it was in the 1940's and 1950's; again, the direct role of public policy in the labor market is, and particularly will become, far more significant than it was in the peacetime years of the 1940's and 1950's. Third, with the passage of time we may well develop new concepts and new frameworks which will enable the researcher to force the facts to disclose new secrets. Finally, now that we are in the process of developing a national manpower policy—however haltingly—it becomes imperative that we understand just how different labor markets function; and since we are concerned with policy, the understanding will have to be cast in truly operational terms—quantified wherever possible. But the older labor market studies were not cast in such an operational mold.

So much for the justification of more local labor market studies. Now how does Rees's study fulfill the expectations implicit in these justifications? On the whole, very well indeed. To begin with, he has examined the white-collar sector as well as the blue-collar portion of the market, whereas the studies of the 1940's and 1950's were concerned dominantly with blue-collar workers. And interestingly enough Rees has found a significant difference between the two groups with regard to the quantitative use of informal sources of information. That immediately poses the obvious—but highly important—question: Why this difference? Rees does not provide us with an answer, but hopefully one will be forthcoming before the study is completed.

In my judgment the major contribution of Rees's paper is the utilization of "the cost of search" concept. The older studies did not use this concept in an explicit and analytic fashion. Rees does, and finds that once account is taken of the cost of search, both employees and employers behave far more rationally than we had supposed. And by implication, it seems to me, he is saying that once this concept is brought into play, it can be shown that the local labor market operates in a fashion not too inconsistent with the conclusions of competitive labor market theory. That, of course, is diametrically opposed to the

conclusions of the older labor market studies. Should we, then, discard the older findings? Not yet. For the Rees paper represents only his preliminary findings. It is therefore conceivable that he may revise his verdict when all the evidence is in. But even if he does not, there is still no assurance that other labor markets, characterized by different location, size, industrial and occupational complexion, degree of unionization, etc., operate the same way as does Chicago. That is precisely why, as I indicated at the outset, we need more such studies.

One final word: I find nothing in Rees's paper that discloses if he and his colleagues have attempted to ascertain whether in an area so large as Chicago there are in actual fact several local markets—"submarkets," that is; and if there are, how they relate to each other. In my judgment that would be an interesting and constructive area of investigation.

AMERICAN ECONOMIC ASSOCIATION

PROCEEDINGS

OF THE

SEVENTY-EIGHTH

ANNUAL

MEETING

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NEW YORK, NEW YORK

DECEMBER 28-30, 1965

PROCEEDINGS OF THE AMERICAN ECONOMIC ASSOCIATION

ANNUAL BUSINESS MEETING, DECEMBER 30, 1965
NEW YORK HILTON HOTEL, NEW YORK, NEW YORK

The Seventy-eighth Annual Business Meeting of the American Economic Association was called to order in the Beekman Room of the New York Hilton Hotel, New York, New York, at 4:30 p.m. by President Joseph J. Spengler. The minutes of the Business Meeting of December 30, 1964, were approved and the minutes of the Executive Committee meetings and reports of officers and committees of the Association were ratified. These reports are published in these "Proceedings" and constitute the official actions of the Association when approved at the annual meeting.

It was VOTED to approve the following changes in the bylaws of the Association:

Section I, Paragraph 2

There shall be six classes of members other than honorary: members paying an annual fee of $10.00; family members (two or more living at the same address, second membership without subscription to the publications of the Association) paying an annual fee of $1.00; junior members (available to registered students for three consecutive years only) paying an annual fee of $5.00; subscribing members paying an annual fee of $12.00; contributing members paying an annual fee of $25.00 or more; and life members comprising those members who contribute $200.00 or more in a single payment. Life members shall be exempt from annual fees. Members shall have each year the privilege of designating the class of membership they choose for that year.

Section I, Paragraph 3

Foreign economists of distinction, not exceeding twenty-five in number, may be elected honorary members of the Association. Past presidents of the Association and members who have been awarded the Walker Medal shall be Distinguished Fellows. In addition, the Executive Committee may elect up to a total of twelve additional Distinguished Fellows, but not more than two in any one calendar year, from economists of high distinction in the United States and Canada. Candidates for Distinguished Fellowships shall be nominated by the Nominating Committee or the Executive Committee, and they shall be elected by the combined vote of the two committees. The Nominating Committee shall solicit and give due consideration to the recommendations of the Committee on Honors and Awards. The Nominating Committee should be free to make no nominations in any particular year. However, it should not be limited as to the number of candidates it may nominate in any year. It should accompany each nomination with a supporting brief. Election to Distinguished Fellowship does not preclude election to any office of the Association.

The report of the Secretary was presented by H. F. Williamson. The schedule of times and places for future annual meetings through 1971 was outlined. Attention was called to the fact that the total number of members and subscribers as of November 30, 1965, was 19,826, a net increase of 2,010 for the year.

In addition, the Secretary reported on the following activities of the Association:

1. *Clearinghouse Project.* There are currently over 3,000 individuals in the roster of economists interested in overseas assignments.

2. *National Register of Scientific and Technical Personnel.* The data obtained on economists from the survey of scientific personnel for 1964 were

used as a basis for the report, *The Structure of Economists' Employment and Salaries, 1964,* prepared by Professor Arnold Tolles and issued as a supplement to the December, 1965, issue of the *American Economic Review.* The Association will cooperate with the National Science Foundation in the survey of scientific personnel for 1966.

3. *Foreign Graduate Student Screening Project.* This project is now in operation and prepared to handle requests from economics departments for an evaluation of foreign students.

4. *Group Flights.* Three group flights were arranged for attendance at the New York meeting, and it is planned to arrange similar flights for future meetings.

5. *Association Archives.* Attention is again called to the fact that the archives of the Association have been classified and are available to interested scholars.

6. *United States Employment Service.* The United States Employment Service, which has operated for a number of years at our annual meeting, has agreed to make its services available on a year-round basis.

The report of the Treasurer was summarized by H. F. Williamson. The net operating income of the Association for the year ending November 30, 1965, was $31,494 and the unappropriated surplus was $237,892. The full details are shown in the Treasurer's report printed below.

H. F. Williamson reported for the Finance Committee on the changes in the Association's investment portfolio. The full report of the Finance Committee is published below.

The report of the Managing Editor, published below, was summarized by J. G. Gurley. He expressed his appreciation for the effective work done by the Editorial Board and consultants in preparing the *American Economic Review.*

The Secretary then presented the report of the Committee on Elections and the certification of the election of new officers for the year 1966, as follows:

In accordance with the bylaws on election procedure, I hereby certify the results of the recent balloting and present the reports of the Nominating Committee and the Committee on Elections.

The Nominating Committee, consisting of George J. Stigler, Chairman, Karl A. Fox, W. David Maxwell, Lionel W. McKenzie, Anthony D. Scott, and Warren Scoville, presented to the Secretary the list of nominees for the respective offices:

For President-elect

Milton Friedman

For Vice-Presidents	*For Executive Committee*
Dorothy S. Brady	James M. Buchanan
Charles P. Kindleberger	Earl O. Heady
Tjalling C. Koopmans	Hendrik S. Houthakker
Guy H. Orcutt	Lawrence R. Klein

The Committee on Elections, consisting of George Dalton, Chairman, Lester B. Lave, and Harold F. Williamson, prepared biographical sketches of the candidates and ballots were distributed early in November. The canvass of ballots was made on December 16, 1965, and the results were filed with the Secretary.

From the report of the Committee on Elections, I have the following information:

Number of envelopes without name for identification	87
Number received too late	26
Number of defective ballots	—
Number of legal ballots	4,839
Number of returns from the mail ballot	4,952

On the basis of the canvass of the votes cast, I certify that the following persons have been duly elected to the respective offices:

President-elect (for a term of one year)
Milton Friedman

Vice-Presidents (for a term of one year)
Charles P. Kindleberger
Tjalling C. Koopmans

Members of the Executive Committee (for a term of three years)
James M. Buchanan
Lawrence R. Klein

Following the report of the Committee on Elections, President Fritz Machlup took over the chair and introduced Milton Friedman, the new President-elect.

The President recognized Mrs. Alice Hanson Jones, Chairman of the Committee on Resolutions, who presented the following resolution, which was adopted:

Be it resolved that the Association expresses its deep appreciation and thanks to those who have participated in the planning, organizing, and conducting of the Seventy-eighth Annual Meeting. The Association is particularly indebted to its President, J. J. Spengler, for his fine conduct of his office and for the preparation of a Presidential Address of deep scholarship and great contemporary relevance, to its President-elect, Fritz Machlup, for arranging a challenging program centering on problems of basic importance to the membership and to our society, to the Executive Committee for assisting in the task of planning and staffing the program, to the Committee on Local Arrangements for the fine performance of its duties, and to the many participants in the program who have prepared and presented new ideas in economics for our enlightenment.

ALICE HANSON JONES, *Chairman*
WILLIAM HICKS
JAMES A. MORRIS

The meeting was adjourned at 5:10 p.m.

HAROLD F. WILLIAMSON, *Secretary*

THE JOHN BATES CLARK AWARD

Citation on the Occasion of the Presentation of the Medal to Zvi Griliches, December 29, 1965

Every other year the American Economic Association awards the John Bates Clark Medal "to that economist under the age of forty who is adjudged to have made a significant contribution to economic thought and knowledge." The award is made by a joint vote of the Association's Executive Committee and its Committee on Honors and Awards. This year the recipient of the medal is Professor Zvi Griliches, of the University of Chicago.

Professor Griliches has made noteworthy theoretical and empirical contributions to the study of technological change. In his initial work he introduced and tested most interesting hypotheses on the diffusion of innovations, relating their spread both to the difficulties of learning and communication and to the profitability of innovations. In subsequent research he has been concerned with quality changes over time; and in the course of these investigations he has made important contributions related to the problems of the measurement of capital and of quality changes in price index numbers. Recent work on technological change in relation to growth in agriculture and manufacturing in the United States has yielded ingenious and significant contributions concerning the impact on production functions of quality changes in inputs.

MINUTES OF THE EXECUTIVE COMMITTEE MEETINGS

1. Minutes of the spring meeting held in New York City, March 12-13, 1965:

The *first meeting* of the *1965 Executive Committee* was called to order at 9:30 a.m. at the New York Hilton Hotel, New York City, March 12, 1965. The following were present: J. J. Spengler, presiding, Moses Abramovitz, W. J. Baumol, E. D. Domar, J. G. Gurley, Gottfried Haberler, C. J. Hitch, H. G. Johnson, W. A. Lewis, Fritz Machlup, R. M. Solow, G. J. Stigler, and H. F. Williamson. Absent was: N. H. Jacoby. Present as members of the Nominating Committee were: K. A. Fox, W. D. Maxwell, L. W. McKenzie, A. D. Scott, and Warren Scoville. Present as quests were: B. F. Haley, Simon Rottenberg, Arthur Smithies, and N. A. Tolles.

1. *President's Remarks* (J. J. Spengler). President Spengler outlined the items on the agenda to be covered at the meetings.

2. *Minutes*. The minutes of the December 27, 1964, Executive Committee meeting were approved as previously sent out to members of the Executive Committee.

3. *Report of the Secretary* (H. F. Williamson).

Annual Meetings. The Secretary reported that the total gross registration by affiliations at the Chicago meeting was 7,397, of which 3,164 (42.8 percent) were affiliated with the A.E.A. The net registration figure was 6,179 (including 1,244 with no indicated affiliation).

The question was raised of scheduling a joint meeting with the American Statistical Association for New York City in 1973. The Secretary was instructed to explore this possibility with Donald Riley, Secretary of the A.S.A.

The question of the daily time schedule for the 1965 meeting was raised. It was decided to follow a schedule of two morning sessions, 8:30-10:00 and 10:30-12:00, and one afternoon session, 2:30-4:00. Following a discussion, it was decided to schedule the 1965 meeting in New York City for December 28-30 and to hold the meeting of the Executive Committee on December 27, starting with luncheon at 12:30 p.m.

Papers and Proceedings. The Secretary announced that it was planned to print 20,000 copies of the 1965 *Papers and Proceedings* and that the total manuscript pages was approximately 1,080.

Clearinghouse Project. The Secretary reported that the activities of the Clearinghouse were expanding and that for the eighteen-month period ending March 1, the Association had been asked to recommend candidates for approximately 150 positions. He also noted a recommendation by Theodore Morgan that a list of Asian students who have completed or are about to complete their graduate training be prepared. This list would be circulated among research and educational institutions in Asia interested in obtaining the services of such students. It was agreed that this proposal should be implemented as a part of the Clearinghouse Project.

N.S.F. Register. The Secretary reported that plans were under way for the Association to participate in the 1966 Register of Scientific Personnel.

Foreign Graduate Student Screening Project. The Secretary reported that: (1) requests for information on economics and agricultural economics departments to be included in the bulletin had been sent to 176 departments and that it was hoped to have this material ready for publication as of June 1, 1965; (2) evaluations of foreign graduate departments are being received and the quality of these reports is excellent; (3) plans are under way to establish overseas panels of individuals qualified to evaluate the credentials of students applying for graduate work in economics and if requested to interview such candidates; (4) graduate departments in the United States wishing to participate in the Screening Project have been asked to contribute \$25.00, plus \$3.00 for each foreign graduate student enrolled; and (5) the Department of State has authorized a one-year grant to the Project of the equivalent of \$15,000 in foreign currencies, which will be available to cover the expenses of overseas travel and payments to members of overseas screening and interview panels.

Association History Project. Arrangements have been made for A. W. Coats to spend the summer of 1965 completing his research on the history of the Association.

4. *Report of the Treasurer* (H. F. Williamson). The Treasurer presented a budget estimate for fiscal 1965. He also reported that the Finance Committee had no changes to report.

5. *Report of the Managing Editor* (J. G. Gurley). The Editor presented a revised budget as follows: printing (including paper, postage, reprints), \$57,000; Managing Editor's salary, \$8,000 (later increased to \$9,000 by action of the Executive Committee); editorial and

clerical assistance, $13,660; supplies and postage, $1,350; contributors, $1,425; and office equipment and decoration, $100. It was VOTED to accept the revised budget as presented.

6. *Reports of Standing and Special Committees.*

Committee on Research and Publications (I. B. Kravis). The Chairman reviewed the status of the Association's publications. He raised the question of plans for additional survey articles. It was decided that a proposal for such a series should be presented at the December meeting of the Executive Committee. It was also decided that the recommendation of an editor for the translations series be presented at the same time.

In this connection, G. J. Stigler reported on negotiations for the publication of a new translation series. Following a discussion, it was decided to enter into a contract with Richard D. Irwin, Inc., for the publication of the series, provided the company is willing to undertake publication without a subsidy. (Subsequent to the meeting a contract was signed with Richard D. Irwin, Inc.)

It was agreed that the decision to drop the publication of individual volumes in the "Readings" and "Survey" series be made by the President and the Secretary of the Association.

Journal of Economic Abstracts (Arthur Smithies). The Chairman reported on the current status of the *Journal of Economic Abstracts* and recommended on behalf of the Committee that the *Journal* be added to the publications going to members and subscribers. Following a discussion, it was VOTED to accept the recommendation of the Committee, effective January 1, 1966. In connection with this decision, the question of financing the *Journal* was raised. Following the discussion, it was VOTED to amend Section I, Paragraph 2, of the bylaws to make the following changes, effective as of January 1, 1966: annual dues, $10.00; junior membership dues, $5.00; family membership, $1.00; subscribing membership dues, $12.00; contributing membership dues, $25.00 or more; life membership dues, $200.00; subscriptions, $10.00.

Committee on Economic Education (G. L. Bach). No report.

Committee on the N.S.F. Report on the Profession (N. A. Tolles). The Chairman outlined a proposal for the preparation of a report on the economics profession, based on data collected and made available by the N.S.F. Register of Scientific Personnel for 1964. Following a discussion, it was VOTED to accept the offer of the Chairman to prepare a report to be issued as a 64-page supplement to the December, 1965, *American Economic Review* and to authorize the following budget for the project:

Personal services	$4,340
Supplies, postage, and telephone	200
Travel	575
Printing (20,000 copies)	2,729
	$7,844

Committee on Honors and Awards (B. F. Haley). The Chairman submitted the Committee's recommendations for the John Bates Clark award for 1965. After discussion, the recipient was elected as a result of a ballot of the members of the Electoral College and the Committee on Honors and Awards.

Nominating Committee (G. J. Stigler). The Executive Committee and the Nominating Committee met as an Electoral College to consider nominees for the office of President-elect for 1966. After discussion, the nominee was selected and his acceptance obtained. Nominations for the other offices were discussed. The Chairman also presented to the Electoral College the nominations for the elections of two Distinguished Fellows. Following discussions the two candidates were selected, with the understanding that the awards would be made at the December, 1965, meeting of the Association.

7. *Reports of Representatives or Advisory Committees.*

A.C.L.S. (R. L. Andreano). No report.

S.S.R.C. (G. H. Hildebrand). No report.

N.B.E.R. (W. L. Thorp). No report.

A.A.A.S. (K. E. Boulding, Acting). The Secretary read a report from K. E. Boulding, who, in the absence of Bert Hoselitz, represented the Association at the Montreal meeting of the A.A.A.S. in December. Following are the two sessions held, cosponsored by the A.E.A. and the A.A.A.S: (1) "Structuralism in the Social Science," by Gilles Paquette, Carleton University, with discussion by Albert Breton, University of Montreal; and (2) "Synopsis of Some Simple Models of Growth," by A. Asimakopulos and J. C. Weldon, McGill University, with discussion by T. K. Rymes, Carleton University.

I.E.A. (B. F. Haley). B. F. Haley reported on his negotiations with the Ford Foundation for a new five-year grant to the I.E.A. He indicated that prospects for such a grant

were not particularly good, largely because of the existence of a large number of international associations. He did indicate, however, that the Foundation might be willing to provide support for "Refresher Conferences" in underdeveloped countries and for regional conferences.

I.I.E. (Simon Rottenberg, Acting). No report.

Census Advisory Committee (Solomon Fabricant). No report.

U.S. National Commission for UNESCO (K. E. Boulding). The Secretary read a report from K. E. Boulding, indicating that UNESCO's interests have moved sharply in the direction of economic development and that the U.S. National Commission for UNESCO is actively engaged in developing proposals to transmit to UNESCO through the United States Department of State. Proposals and suggestions from members for the work of UNESCO will be welcomed and may be directed to K. E. Boulding, our representative to the Commission.

Joint Council on Economic Education, Special Advisory Committee (G. L. Bach). No report.

National Academy of Sciences—National Research Council (Robert Dorfman). No report.

National Council of Testing of English as a Foreign Language (Simon Rottenberg). No report.

8. *Unfinished and Miscellaneous Business.*

The Secretary outlined a proposal by the United States Employment Service to operate a placement service for the Association on a year-round basis. After consideration, it was VOTED to accept the proposal, with the understanding that the President would appoint an advisory committee when and if requested by the United States Employment Service.

The Secretary reported that J. P. Miller was willing to undertake the preparation of Volume VII of the *Index of Economic Journals* and that he estimated that the project would take two years and the cost would be approximately $10,000. It was VOTED to have J. P. Miller undertake the preparation of Volume VII at the cost indicated.

E. D. Domar presented a proposal by R. R. Hough for the preparation of a Key Word in Context Index of articles in economic periodicals, with a request that the A.E.A. cooperate in the preparation of the Index. Following a discussion, it was agreed that the President of the Association, after consulting with the Advisory Committee on the *Index of Economic Journals*, should decide what action should be taken in connection with this project.

A problem encountered by the Interuniversity Committee on Travel Grants in obtaining visas for American scholars to visit and do research in the Soviet Union was presented by E. D. Domar. It was agreed that the President should write to the Committee, protesting the restriction on visas by the Soviet Union and that copies of his letter should go to the other social science groups involved.

The President announced the reappointment of J. G. Gurley as Managing Editor of the *A.E.R.* for a three-year term beginning as of January 1, 1966, with the understanding that the Editor has the option of terminating his appointment at the end of two years.

J. J. Spengler presented the recommendations of the Committee on Administrative Staff Salaries. Following a discussion, it was VOTED to increase the annual salaries of the Managing Editor and the Secretary-Treasurer to $9,000, effective as of April 1, 1965, and to instruct the Secretary to review the retirement status of Miss Gertrude Tait, with particular reference to whether the Association's contributions had been consistent with the practice of Northwestern University and to make an appropriate recommendation to the Committee.

It was also VOTED to authorize a payment to the Secretary-Treasurer of $3,600 per year for the added responsibility in connection with the Foreign Graduate Student Screening Project, beginning August 1, 1965, with the understanding that this amount is to be charged directly to the Project.

The President-elect, Fritz Machlup, discussed his plans for the program for the 1965 annual meeting.

The meeting was adjourned at 11:30 a.m., March 13, 1965.

2. Minutes of the meeting held in New York, New York, December 27, 1965:

The *second meeting* of the *1965 Executive Committee* was called to order at 2:00 p.m. at the New York Hilton Hotel, J. J. Spengler presiding. Others present were: W. J. Baumol, E. D. Domar, J. G. Gurley, Gottfried Haberler, C. J. Hitch, H. G. Johnson,

W. A. Lewis, Fritz Machlup, R. M. Solow, and H. F. Williamson. Absent were: Moses Abramovitz, N. H. Jacoby, and G. J. Stigler. Present as guests were: G. L. Bach, K. E. Boulding, Solomon Fabricant, Milton Friedman, B. F. Haley, C. P. Kindleberger, T. C. Koopmans, and Arthur Smithies. The meeting was adjourned at 11:00 p.m.

1. *President's Remarks* (J. J. Spengler). The President reviewed the order of the items on the agenda.

2. *Minutes*. The minutes of the March 12-13, 1965, meeting were approved.

3. *Report of the Secretary* (H. F. Williamson). The Secretary presented the schedule of future meetings, membership growth and compositon, publication costs of the 1965 *Papers and Proceedings,* requests to reprint, the use of the mailing list, and new activities of the Association—all more fully treated in the Secretary's report as presented below.

Attention was called to the changes in the bylaws recording the approved increase in membership dues and the creation of the category of Distinguished Fellow of the Association which were to be ratified at the Business Meeting.

It was VOTED to approve and accept the Secretary's report.

It was VOTED to hold the spring meeting of the Executive Committee in New York City, March 11-12, 1966.

4. *Report of the Treasurer* (H. F. Williamson). The Report of the Treasurer, published in full below, was summarized. Special attention was called to the fact that the net operating income for the fiscal year ending November 30, 1965, was $31,494. It was VOTED to accept the report of the Treasurer.

5. *Report of the Auditor* (H. F. Williamson). The Secretary reported that David Himmelblau & Co., the Association's former auditors, had been absorbed by Arthur Andersen & Co., which had agreed to continue auditing the Association's accounts. It was VOTED to accept the report of the auditor as printed below. The Secretary was instructed to express to Arthur Andersen & Co. the appreciation of the Executive Committee for their services.

6. *Report of the Finance Committee* (H. F. Williamson). H. F. Williamson outlined the main features of the report of the Finance Committee, published below. Following a discussion of financial policies appropriate for the Association, it was VOTED to accept the Committee's report and to reappoint the members of the Committee to serve during 1966. The Secretary was instructed to express to the members of the Committee the appreciation of the Executive Committee for their services.

7. *Report of the Managing Editor* (J. G. Gurley). The Editor called special attention to the fact that the number of manuscripts received during 1965, 420, was close to the record high of 431 manuscripts received during 1964 and that he had a twelve-month backlog of manuscripts accepted for publication. It was VOTED to accept the report of the Managing Editor and his proposed budget for 1966.

8. *Reports of Standing and Special Committees.*

Committee on Research and Publications (I. B. Kravis). On behalf of the Committee, H. G. Johnson proposed that the Association sponsor the publication of a series of surveys of national economic policy problems and issues. It was VOTED to accept this proposal and to appropriate an amount not to exceed $10,000 to cover the cost of the preparation of the surveys. It was also agreed that if no other outlet is available, the publication of the series will be undertaken by the Association.

The Secretary read a report from John Perry Miller on the status of Volume VII of the *Index of Economic Journals,* which indicated that the new volume should be available in published form by the fall of 1966. Professor Miller asked the Executive Committee to decide whether the present *Index* series should be continued beyond Volume VII. It was VOTED to accept the report and to ask Professor Miller to arrange for Yale University to supervise future volumes in the present format.

Journal of Economic Abstracts (Arthur Smithies). Following a report by Professor Smithies, it was VOTED to transfer the periodical listing presently in the *American Economic Review* to the *Journal of Economic Abstracts.* It was also VOTED that the articles in the *Journal* be classified according to subject and that a committee consisting of J. G. Gurley, Arthur Smithies, and Fritz Machlup be appointed to work out details.

Committee on Economic Education (G. L. Bach). The Chairman summarized his report, which is printed below. It was VOTED to approve the payment of $1,163.88 to G. L. Bach to cover travel expenses of the Committee during 1965.

Committee on Honors and Awards (B. F. Haley). No report.

Committee on the N.S.F. Report (N. A. Tolles). In the absence of the Chairman, the Secretary commented briefly on the report of the Committee, printed below.

Nominating Committee. Fritz Machlup announced that B. F. Haley would serve as Chairman of the Nominating Committee for 1966.

9. *Reports of Representatives and Members of Advisory Committees.*

A.C.L.S. (R. L. Andreano). Report printed below.

S.S.R.C. (G. H Hildebrand). No report.

N.B.E.R. (W. L. Thorp). Report printed below.

A.A.A.S. K. E. Boulding reported informally on the activities of the A.A.A.S.

I.E.A. (B. F. Haley). B. F. Haley presented his report, printed below. Following the recommendation of Professor Haley, it was VOTED that the Association continue to pay double the annual dues of $300.00 to the I.E.A. It was VOTED to approve the payment of $661.85 to D. G. Johnson to cover his travel expense to Rome to represent the Association at the meeting of the I.E.A.

I.I.E. (Benjamin Higgins). Report printed below.

Census Advisory Committee (Solomon Fabricant). The Chairman furnished a report, printed below.

U.S. National Commission for UNESCO (K. E. Boulding). The Chairman read a report on the activities of UNESCO during 1965 (see below).

J.C.E.E. Special Advisory Board (G. L. Bach). See Committee on Economic Education report.

National Academy of Sciences—National Research Council (H. M. Oliver). Report printed below.

10. *Unfinished and Miscellaneous Business.*

The Secretary reported that Abe Fortas could no longer serve as our Counsel because of his appointment to the U.S. Supreme Court. It was agreed that the President should take steps to appoint a new Counsel to represent the Association.

It was VOTED to have the Association sponsor an *Index of Economic Essays Published in Festschriften and Symposia* and to ask J. P. Miller if this project could be carried on in conjunction with the preparation of the *Index of Economic Journals.*

It was VOTED to have the President appoint an *ad hoc* committee to discuss the possibilities of cooperating with the American Association of Law Schools on questions of mutual interest.

It was VOTED to have the President appoint a member of the Association to consider the problem of improving the quality of government statistics.

Following a discussion of the timing of the annual business meeting, it was VOTED to hold the business meeting in San Francisco immediately following the presidential address.

Following a discussion, it was VOTED to ask A. W. Coats to prepare a monographic history of the Association. It was agreed that an advisory group, appointed by the Committee on Research and Publications, should work with Professor Coats on this project.

G. L. Bach outlined a proposed visiting scientist program in economics, to be financed by a grant from the N.S.F. and administered by the Committee on Economic Education. It was VOTED to authorize the Treasurer to make formal application to the N.S.F. for a grant for this project.

It was VOTED to print the pictures of new Distinguished Fellows in the *American Economic Review.*

It was VOTED to authorize the Treasurer to pay up to $1,500.00 for travel expenses of committees appointed by the Association.

The meeting was adjourned at 11:30 p m.

REPORT OF THE SECRETARY FOR THE YEAR 1965

Annual Meetings. The final report of the 1964 meeting in Chicago indicated a net registration of 6,271, of which 3,164 were members of the American Economic Association. The net income from the meeting was $24,156.14. Of this amount, the Association received $11,502.80, based on the number who registered as members.

The schedule for future annual meetings is: 1966, San Francisco, San Francisco Hilton; 1967, Washington, D.C., Sheraton Park Hotel; 1968, Chicago, Pick-Congress Hotel; 1969, New York City, New York Hilton Hotel; 1970, Detroit, Sheraton-Cadillac Hotel; and 1971, New Orleans (hotel to be confirmed).

Dr. Gault Lynn, of the Federal Reserve Bank of San Francisco, will serve as Chairman of the Committee on Local Arrangements for the 1966 meeting in San Francisco.

Membership. Exhibit I below shows that the total number of members and subscribers was 19,826 as of November 30, 1965, a net increase of 2,010 for the year.

Advertising and Announcements. The number of advertising pages in the *American Economic Review* for 1965 was 167½ paid and 51 exchange pages compared with 162 and 54 pages in 1964. The "Vacancies and Applications" announcements totaled 35 pages for 1965 compared to 33 for 1964.

Papers and Proceedings. A comparison of the size and cost of the *Papers and Proceedings* for the period 1957-65 is made in Exhibit II below.

Permissions to Reprint and Translate. The number of permissions to quote from, reprint, or translate articles from the *Review* and the *Papers and Proceedings* totaled 161 for 1965 compared to 201 for 1964. Under the policy adopted by the Executive Committee at the March, 1962, meeting, permission to reprint or translate is granted only after the consent of the author has been obtained by the editor or publisher.

Use of the Mailing List. Requests for the use of the mailing list continue to come largely from publishers of books and periodicals and from corporations and government agencies wishing to send out reports or reprints of speeches. Income from the use of the mailing list during 1965 amounted to $12,164.

Group Flights. Flights originating in San Francisco, St. Louis, and Chicago were organized for the members attending the annual meeting in New York. Plans have been made for group flights for the annual meeting in San Francisco in 1966.

Asia Foundation Grant. The grant of $2,500 from the Asia Foundation was used principally to pay the expenses of Asian students to attend the annual meeting in New York City. An application for a renewal of the grant will be made to the Foundation in the near future.

Clearinghouse Project. There are currently nearly 3,000 individuals registered with the A.E.A. Clearinghouse Project. Demand for the services of the registry continues to grow, especially from overseas educational, research, and

government institutions. Plans are being made to put the Clearinghouse Project on a self-sustaining financial basis, since the original Ford grant supporting the project terminates as of July, 1966.

National Register of Scientific Personnel. The Association has renewed its contract with the National Science Foundation to participate in the survey of scientific personnel for 1966. It is expected that the information from the 1966 survey will be useful in continuing the analysis of the structure of the economics profession.

Foreign Student Screening Project. Arrangements have been made for an evaluation of foreign students applying to American departments for graduate work in economics and agricultural economics in the major countries in the Middle East, Far East, and Latin America. Contacts have also been made to provide a similar service in Africa. This project has been made possible by a grant from the Ford Foundation and financial support from the United States Department of State.

Employment Service. As in the past several years, the U.S. Employment Service arranged to operate the register at the annual meeting in New York City. Under the authorization of the Executive Committee, plans are being worked out to use the facilities of the U.S. Employment Service on a year-round basis. The proposal is to have the Chicago office of the U.S. Employment Service act as a clearinghouse for vacancies and applications for positions.

Standing Committees

CENSUS ADVISORY COMMITTEE
Solomon Fabricant, *Chairman* (1968)
Bert G. Hickman (1966)
Guy H. Orcutt (1966)
Sherman J. Maisel (1967)
Ralph W. Pfouts (1967)
Gideon Rosenbluth (1967)
Daniel Creamer (1968)
H. Gregg Lewis (1968)
John Lintner (1968)
Arthur M. Okun (1968)
Morris A. Adelman (1969)
Robert R. Nathan (1969)
Fred H. Klopstock (1969)
Ernest Williams, Jr. (1969)
Douglass C. North (1969)

COMMITTEE ON ADMINISTRATIVE STAFF SALARIES
Joseph J. Spengler
Fritz Machlup
Milton Friedman

COMMITTEE ON ECONOMIC EDUCATION
G. L. Bach, *Chairman* (1967)
R. A. Gordon (1965)
Ben W. Lewis (1965)
Marshall R. Colberg (1967)
Rendigs Fels (1967)
Emanuel T. Weiler (1967)
Harold F. Williamson, Ex Officio

COMMITTEE ON HONORS AND AWARDS
Bernard F. Haley, *Chairman* (1966)
Earl J. Hamilton (1966)
Martin Bronfenbrenner (1966)
James Tobin (1970)
Anthony D. Scott (1970)
Richard A. Musgrave (1970)

COMMITTEE ON THE N.S.F. REPORT ON THE PROFESSION
N. Arnold Tolles, *Chairman*
Alice Hanson Jones
Ewan Clague

COMMITTEE ON RESEARCH AND PUBLICATION
Irving B. Kravis, *Chairman* (1966)
Franco Modigliani (1965)
G. H. Hildebrand (1966)
Karl A. Fox (1967)
W. L. Thorp (1970)
Bert F. Hoselitz (1966)
Harry Johnson (1966)
Harold F. Williamson, Ex Officio

INDEX OF ECONOMIC JOURNALS ADVISORY COMMITTEE
John Perry Miller, *Chairman*
Robert Bishop
Earl J. Hamilton
Fritz Macklup
Joseph J. Spengler

INSTITUTE OF INTERNATIONAL EDUCATION ADVISORY AND POLICY BOARD REPRESENTATIVES
Benjamin Higgins, *Chairman* (1966)
Simon Rottenberg (1967)
Andrew Kamarck (1965)
John Sheahan (1965)
Anthony Tang (1966)
Raymond Mikesell (1967)
Henry Rosovsky (1967)

JOINT COUNCIL ON ECONOMIC EDUCATION SPECIAL ADVISORY COMMITTEE
G. L. Bach (1966)
Rendigs Fels (1966)
Ben W. Lewis (1965)

Committees Appointed During the Year

COMMITTEE ON ELECTIONS
George Dalton, *Chairman*
Lester B. Lave
Harold F. Williamson, Ex Officio

COMMITTEE ON FUTURE OPERATIONS OF JOURNAL OF ECONOMIC ABSTRACTS
Arthur Smithies
John G. Gurley
Harold F. Williamson

AD HOC COMMITTEE ON PUBLICATION OF TRANSLATIONS OF WORK OF FOREIGN ECONOMISTS
George J. Stigler
Harold F. Williamson

COMMITTEE ON UNESCO
Kenneth E. Boulding, *Chairman*
James Blackman
Jerry Miner

FINANCE COMMITTEE
C. Wells Farnham, *Chairman*
Corliss D. Anderson
James Washington Bell
Harold F. Williamson

NOMINATING COMMITTEE
George J. Stigler, *Chairman*
Karl A. Fox
W. David Maxwell
Lionel W. McKenzie
Anthony D. Scott
Warren Scoville

TECHNICAL SUBCOMMITTEE ON BUSINESS CYCLE DEVELOPMENTS
Bert G. Hickman, *Chairman* (1966)
Gottfried Haberler (1966)
Lorman C. Trueblood (1966)
Donald J. Daly (1967)
Lawrence R. Klein (1967)

Arthur M. Okun (1967)
Geoffrey H. Moore (1968)
Frank E. Morris (1969)
Beryl W. Sprinkel (1969)
Harry Stark (1969)

Council and Other Representatives

A.A.A.S.
Bert Hoselitz (1965)

A.C.L.S.
Ralph L. Andreano (1966)

I.E.A.
Bernard F. Haley (1965)
Robert M. Solow (1968)

N.B.E.R.
Willard L. Thorp (1970)

NATIONAL COUNCIL OF TESTING OF ENGLISH AS A FOREIGN LANGUAGE
Arthur E. Burns

NATIONAL RESEARCH COUNCIL OF THE NATIONAL ACADEMY OF SCIENCES
Henry M. Oliver (1966)
Lionel McKenzie (1967)
James N. Morgan (1968)

N.S.F. INTERDEPARTMENTAL PANEL ON SCIENTIFIC INFORMATION ACTIVITIES
Richard Ruggles

S.S.R.C.
Franco Modigliani (1965)
G. H. Hildebrand (1966)
Karl A. Fox (1967)

U.S. NATIONAL COMMISSION FOR UNESCO
Kenneth E. Boulding

Representatives of the Association on Various Occasions

DINNER ON RETIREMENT OF EWAN CLAGUE
Kermit Gordon

FAIRLEIGH DICKINSON UNIVERSITY, DEDICATION OF WROXTON COLLEGE, WROXTON ABBEY, ENGLAND
Paul B. Streeten

UNIVERSITY OF KENTUCKY, CENTENNIAL YEAR CELEBRATION OF FOUNDERS DAY
John T. Masten

AMERICAN ACADEMY OF POLITICAL AND SOCIAL SCIENCE, SIXTY-NINTH ANNUAL MEETING
Clay J. Anderson
James Cross

I.E.A., ROME MEETINGS
Bernard F. Haley
D. Gale Johnson

INAUGURATIONS
J. Ralph Holly, Greensboro College
David H. Shelton

George Bruce Dearing, State University of New York at Binghamton
Laurence E. Leamer
Harold E. Sponberg, Eastern Michigan University
John W. Simpson
John A. Fitterer, Seattle University
Vernon A. Mund
Willard Deming Lewis, Lehigh University
Elmer C. Bratt
Leland Eldridge Traywick, University of Omaha
James F. Hanley
Joseph Cornwall Palamountain, Jr., Skidmore College
James S. Youtsler
Wayne F. Geisert, Bridgewater College
Bernard S. Logan
Arthur Lee Mallory, Southwest Missouri State College
James W. Robinson
Charles Henry Watts, II, Bucknell University
Patrick M. Boardman
Robert Donald Clark, San Jose State College
Robert G. Schroeder
Frederick William Ness, Fresno State College
Paul D. Bush
Gordon Williams Blackwell, Furman University
George H. Aull
William Rea Keast, Wayne State University
Richard A. La Barge
Harold Clark Martin, Union College
Lawrence Abbott

Respectfully submitted,
HAROLD F. WILLIAMSON, *Secretary*

EXHIBIT I
Members and Subscribers

	Totals 11/30/64	Gain or *Loss*	Totals 11/30/65
Class of membership:			
Annual	10,960	942	11,902
Junior	1,557	120	1,677
Family	156	28	184
Complimentary	96	4	100*
Life	239	8	247
Honorary	17	—	17
Total members	13,025	1,102	14,127
Subscribers	4,791	908	5,699
Totals	17,816	2,010	19,826

* Includes 31 who do not receive the publications.

EXHIBIT II
Publication Costs

	Papers and Proceedings			Handbooks		
Year*	Number of pages	Number of copies	Cost	Number of pages	Number of copies	Cost
1957	754	12,400	$16,253	548	10,100	$15,815
1958	677	12,700	15,471	32	9,300	1,434
1959	689	14,000	16,780			
1960	745	14,800	18,914			
1961	675	15,800	19,759			
1962	615	16,000	18,277			
1963	753	17,700	23,440			
1964	698	18,500	23,362	472	16,000	48,626
1965	652	20,000	23,264			

* This is the year of publication and pertains to the meeting of the preceding year.

REPORT OF THE TREASURER
FOR THE YEAR ENDING NOVEMBER 30, 1965

The comparative results of the financial operation of the Association for 1964 and 1965 are shown in Table 1. While receipts from the sale of back numbers of the *A.E.R.*, the 1964 *Handbook,* and the mailing list were lower in 1965 than in 1964, the return from other sources expanded sufficiently to increase the operating income for 1965 to a total of approximately $226,000, about $5,000 above operating income for 1964. An increase in virtually all individual expense items resulted in total operating expenses of $180,678 for 1965, up nearly $20,000 over operating expenses for 1964. Net operating income for 1965 was $45,359 compared with $60,102 for 1964.

The comparative financial status of the Association for 1964 and 1965 is shown in Table 2. After taking into account deductions from the surplus to cover the cost of Volume VII of the *Index of Economic Journals*, an appropriation for future editions of the *Handbook,* and an appropriation for the Committee on Economic Education, the Association's unappropriated surplus for 1965 of $444,429 was nearly $20,000 larger than at the end of 1964.

Tables 3 and 4 give summaries of the investment portfolios and returns on investment for the years 1945-65. An alphabetical list of the Association's security holdings for 1965 is shown in Table 5.

Whether the Association will be able to maintain a net operating income during 1966 is open to question. The normal sources of income will no doubt continue to expand at a moderate rate, but operating expenses during 1966 may be considerably higher than in 1965. Printing costs of the Association's publications, for example, are up approximately 9 percent, while at the same time the Editor has been authorized to expand the size of the *Review*. We anticipate that during the year there will be certain nonrecurring expenses involved in adding the *Journal of Economic Abstracts* to the publications received by members and subscribers. Finally, the Executive Committee has authorized several projects which may necessitate additional expenditures by the Association during 1966.

Respectfully submitted,
HAROLD F. WILLIAMSON, *Treasurer*

TABLE 1
COMPARATIVE RESULTS OF OPERATIONS, 1964 AND 1965

	11/30/64	11/30/65
Income		
Membership dues	$ 94,477	$102,378
Subscriptions	38,955	42,726
Sales	5,315	3,987
Advertising	43,473	47,273
Republications income	397	3,860
Handbook	7,244	1,935
Sale of mailing list	7,283	5,858
Sundry income	1,034	1,496
Dues and publications income	$198,178	$209,513
Interest	$ 6,594	$ 8,441
Dividends	6,364	6,330
Less custodian fees	357*	439*
Profit on sales of securities (net)	10,215	2,192
Investment income (less fees)	$ 22,817	$ 16,524
Total income	$220,995	$226,037
Expenses		
Office salaries	$ 37,333	$ 41,859
Annuity payments	2,612	8,992
International Economic Association	400	1,131
Archives	1,009	3,060
N.S.F. Report on the Profession	—	5,162
Other administrative expenses	12,085	13,153
Annual meeting	8,631*	10,003*
Executive Committee	1,870	2,208
Other committee expenses	1,133	1,346
Administrative and operating expenses	$ 47,811	$ 66,908
Review printing	$ 53,735	$ 66,290
Papers and Proceedings printing	23,362	23,264
Editorial office (*Review*):		
Contributors	1,580	1,388
Editorial and clerical salaries	20,023	20,270
Other expenses (net)	2,285	2,558
Publications	$113,082	$113,770
Total expenses	$160,893	$180,678
Net operating income or loss	$ 60,102	$ 45,359†

* Denotes red.

† It should be noted that the Association's excess of income over expenses (net operating income) for 1965 shown in the Auditor's Report is $31,494, compared to the figure of $45,359 shown in Table 1. This difference is accounted for by the procedures of the present auditors to allocate items such as reserves for the *Handbook* and the *Index of Economic Journals* to operating expenses rather than as a deduction from unappropriated surplus, the procedure followed by the firm that formerly audited the Association's accounts.

TABLE 2

COMPARATIVE FINANCIAL CONDITION, 1964 AND 1965

	11/30/64	11/30/65
Assets		
Cash on deposit and on hand	$117,505	$126,672
Receivables (net)	19,992	30,305
Prepaid expenses and inventories	2,794	1,615
Furniture and fixtures (net)	3,709	3,953
Investments at cost:		
Bonds	149,198	124,048
Stocks	131,247	157,836
Total assets	$424,445	$444,429
Liabilities and Funds		
Accounts payable	$ 14,274	$ 22,327
Deferred income	19,748	26,774
Committee on Research and Publications	32	—
Committee on Economic Education	368	—
Reserve for *Handbook* supplement	8,000	16,000
Outside grants:		
Clearinghouse for economists (Ford)	22,290	11,567
National register (N.S.F.)	1,982*	3,302*
Foreign Student Screening Project—State	—	13,008
Economic abstracts (Ford)	7,344	1,226
Foreign economic research translations (Ford)	15,750	14,837
Foreign economic surveys (Ford)	26,635	26,152
Survey articles (Rockefeller)	1,625	—
Travel grant (Carnegie)	982	382
Asia Foundation grant	1,654	1,648
Foreign student screening project (Ford)	74,591	48,340
Agricultural Development Council	1,161	667
Sundry	176	111
Life memberships	25,400	26,800
Total liabilities and funds	$218,048	$206,537
Surplus		
Balance at beginning of period	$161,995	$206,397
Expenditures in excess of grant, *Index of Economic Journals*	5,500*	5,000*
Appropriation for *Handbook* supplement	8,000*	8,000*
Appropriation for Committee on Economic Education	2,500*	1,164*
Life memberships written off less reinstatements	300	300
Net income or loss for period	60,102	45,359
Unappropriated surplus	$206,397	$237,892
Total footings	$424,445	$444,429

* Denotes red.

TABLE 3
INVESTMENT PORTFOLIO

Year	At Par	Cost			Market
	Bonds	Bonds	Stocks	Total	Stocks and Bonds
1945	$ 40,000	$ 36,705	$ 44,955	$ 81,661	$103,574
1948	35,000	33,108	48,624	81,732	84,841
1950	35,000	33,108	51,978	85,087	104,177
1951	43,000	43,340	49,764	93,104	117,316
1952	42,000	42,312	58,934	101,246	130,836
1953	68,000	68,308	46,458	114,766	134,562
1954	61,000	61,518	38,082	99,600	132,280
1955	75,000	75,370	59,394	134,764	166,772
1956	75,000	75,370	60,237	135,607	168,337
1957	75,000	75,370	55,084	130,454	151,638
1958	75,000	75,370	67,741	143,111	175,609
1959	75,000	75,386	67,652	143,038	191,506
1959*	175,000	175,616	67,652	243,268	291,506
1960*	160,000	160,508	94,910	255,418	299,768
1961*	170,000	169,794	109,071	278,865	356,131
1962*	125,000	125,367	116,699	242,066	293,039
1963*	90,000	90,367	119,524	209,891	284,160
1964*	150,000	149,198	131,249	280,447	371,556
1965*	125,000	124,048	157,836	281,884	409,653

* Includes bonds held in temporary operating fund.

TABLE 4
RETURN ON INVESTMENTS

Year	Bonds	Stocks	Total	Rate of Return on Cost
1945	$1,479	$2,488	$3,968	4.71%
1948	1,194	2,944	4,139	5.06
1950	1,117	3,860	4,977	5.85
1951	1,026	4,607	5,633	6.05
1952	1,117	3,681	4,799	4.75
1953	1,435	3,587	5,022	4.36
1954	1,621	2,961	4,582	4.58
1955	1,750	3,002	4,752	3.53
1956	1,770	3,336	5,106	3.76
1957	1,770	3,397	5,167	3.90
1958	1,770	3,182	4,952	3.46
1959*	2,518	3,231	5,749	3.90
1959†	3,894	3,231	7,125	2.90
1960†	6,693	3,772	10,465	4.09
1961	5,460	4,143	9,603	3.44
1962	4,838	4,489	9,327	3.85
1963‡	3,320	5,041	8,361	3.98
1964‡	3,341	6,364	9,705	3.46
1965‡	5,286	6,330	11,616	4.12

* Does not include income from bonds held in temporary operating fund.
† Includes income from bonds held in temporary operating fund.
‡ Does not include interest on savings account.

TABLE 5
LIST OF SECURITIES HELD BY THE ASSOCIATION

Number of Shares of Stock	Stocks	Cost	Approximate Market Value 11/30/65
300	Abbott Laboratories	$ 6,133	$ 12,300
150	Bayer A. G.	6,822	6,450
300	Central and South West Corp.	2,101	15,000
200	Deere and Co.	4,240	9,000
$212\frac{18}{100}$	Walt Disney	8,254	11,024
200	Gulf Oil Corp.	1,394	11,600
200	Houston Lighting and Power Co.	827	11,000
150	Inland Container Corp.	4,944	5,100
38	International Business Machines Corp.	9,300	19,950
100	International Nickel Co. of Canada	3,911	9,200
200	La Salle National Bank	7,676	7,400
100	Marsh and McLennan	3,687	4,700
100	McIntyre Porcupine Mines	4,818	8,700
150	Montgomery Ward and Co.	5,013	5,100
150	Motorola	6,746	24,300
200	Olin Mathieson Chemical Corp.	8,731	11,400
275	Peoples Gas Light and Coke Co.	3,562	11,825
150	Rex Chain Belt Co.	6,621	9,150
200	Royal Dutch Petroleum	8,530	8,600
200	St. Paul Fire and Marine Ins. Co.	12,640	14,000
110	Siemens and Halske	5,519	6,710
200	Standard Oil Co. (Ind.)	3,650	9,600
116	Swedish Ball Bearing	9,023	7,540
200	Union Oil	12,890	14,000
175	Wells Fargo Bank—American Trust Co.	3,522	6,650
50	Xerox	5,305	9,800
150	Zenith Radio Corp.	1,977	17,700
		$157,836	$287,799

Par Amount	Bonds	Cost	Approximate Market Value 11/30/65
$35,000	U.S. Treas. Bills, due 2/24/66	$ 34,641	$ 34,640
50,000	U.S. Treas. Bonds, $3\frac{3}{4}$%, due 5/15/66	50,000	49,610
5,000	U.S. Treas. Notes, $4\frac{1}{4}$%, due 5/15/67	5,000	4,963
20,000	U.S. Treas. Bonds, $3\frac{7}{8}$%, due 11/15/71	19,951	19,254
15,000	U.S. Treas. Bonds, $2\frac{1}{2}$%, due 12/15/72–67	14,456	13,387
	Bonds	$124,048	$121,854
	Stocks	157,836	287,799
	Total	$281,884	$409,653

REPORT OF THE FINANCE COMMITTEE

Executive Committee
American Economic Association
Evanston, Illinois

GENTLEMEN:

The accompanying tables show the list of securities held by the Association at the end of the fiscal year, November 30, 1965, and the changes made during that year. Table 1 gives a classified list of the stocks and the bonds according to maturity and records the cost and approximate market values on November 30, 1965. The securities account of the Association does not include "temporary operating funds" which are deposited in a savings account and approximated $30,000 at the end of the last fiscal year.

The total market value of the securities account as of November 30, 1965, was $460,699. This compares with $421,056 at the end of the previous year.

You will note from Table 2 that again few changes were made in the securities account during the year. The Finance Committee remains mildly optimistic as to the trend of general business and corporate profits and believes that the federal government will make great efforts to prevent or postpone any significant business recession. Therefore, the Committee believes that a fairly aggressive investment position is still desirable in spite of the high level of the stock market and long duration of the present period of business expansion.

Regarding the bonds held by the Association, you will note from the schedule that more than 80 percent is invested in a savings account or in short-term governments. The Committee believes the time may be close at hand when a more aggressive bond investment program should be seriously considered.

Respectfully submitted,
C. WELLS FARNHAM, *Chairman*
CORLISS D. ANDERSON
JAMES WASHINGTON BELL
HAROLD F. WILLIAMSON

TABLE 1

INVENTORY AND APPRAISAL OF SECURITIES AND CASH AS OF 11/30/65

	Par or Shares	Market Value	Cost
FIXED-INCOME SECURITIES			
Cash Equivalent			
Savings Deposit 4%	$50,000	$50,000	$50,000
U.S. Treas. Bills 2/24/66	35,000	35,000	35,000
U.S. Treas. $3\frac{3}{4}$ 5/15/66	50,000	50,000	50,000
U.S. Treas. $4\frac{1}{4}$ 5/15/67	5,000	5,000	5,000
		$140,000	
Bonds			
U.S. Treas. $3\frac{7}{8}$ 11/15/71	$20,000	19,400	20,000
U.S. Treas. $2\frac{1}{2}$ 12/15/72–67	15,000	13,500	15,275
		32,900	
Preferred Stocks			
Union Oil 2.50 Conv. Pfd.	200	14,000	12,890
TOTAL FIXED-INCOME SECURITIES		$186,900	
COMMON STOCKS			
Utilities			
Central & South West	300	$ 15,000	2,100
Houston Lighting and Power	200	11,000	827
Peoples Gas Light and Coke	275	11,825	3,560
		$ 37,825	
Financial			
LaSalle National Bank	200	7,400	7,675
Marsh & McLennan	100	4,700	3,685
St. Paul Fire and Marine	200	14,000	12,640
Wells Fargo Bank	175	6,650	3,521
		$ 32,750	
Merchandising			
Montgomery Ward	150	$ 5,100	5,013
Paper and Textiles			
Inland Container	150	$ 5,100	4,944
Machinery and Construction			
Deere & Company	200	$ 9,000	4,240
Rex Chain Belt	150	9,150	6,620
		$ 18,150	
Mining and Metals			
International Nickel	100	$ 9,200	3,911
McIntyre Porcupine Mines	100	8,700	4,818
		$ 17,900	
Oil and Gas	200		1,393
Gulf Oil	200	$ 11,600	8,530
Royal Dutch	200	8,600	3,650
Standard Oil of Indiana		9,600	
		$ 29,800	

TABLE 1 (*Continued*)

	Par or Shares	Market Value	Cost
Chemicals and Drugs			
Abbott Laboratories	300	$ 12,300	6,135
Olin Mathieson	200	11,400	8,730
		$ 23,700	
Electrical Products			
Motorola	150	$ 24,300	6,745
Zenith Radio	150	17,700	1,975
		$ 42,000	
Office Equipment			
International Business Machines	38	$ 19,950	9,300
Xerox	50	9,800	5,305
		$ 29,750	
Miscellaneous			
Walt Disney Productions	212-18/100	$ 11,024	8,255
Foreign			
Bayer A.G.	150	$ 6,450	6,820
Siemens & Halske	110	6,710	5,520
Swedish Ball Bearing	116	7,540	9,025
		$ 20,700	
TOTAL COMMON STOCKS		$273,799	
TOTAL SECURITIES		$460,699	
TEMPORARY OPERATING FUND Savings Deposit	$30,000		

TABLE 2

SUMMARY OF SECURITIES PURCHASED AND SOLD
YEAR ENDED NOVEMBER 30, 1965

	Shares	Cost	Proceeds	Gain or *Loss*
Sales—Stocks				
Continental Illinois National Bank	220	$6,618.58	$8,432.09	$1,813.51
Proceeds from sale of subscription rights to bonds of Farbenfabriken Bayer A. G..................			55.67	55.67
Proceeds from sale of subscription rights to stock of Swedish Ball Bearing Co..................			280.00	280.00
Proceeds from sale of $\frac{1}{8}$ths fractional interest in share of Swedish Ball Bearing Co..................			43.06	43.06
		$6,618.58	$8,810.82	$2,192.24
Purchases—Stocks				
La Salle National Bank of Chicago	200	$ 7,676.00		
St. Paul Fire and Marine Insurance Co..........................	200	12,640.56		
Union Oil Co. of California.......	200	12,890.80		
In addition, the following shares were received as a result of stock dividends and splits............				
Continental Illinois National Bank	20			
Farbenfabriken Bayer A. G.......	50			
Inland Container Corp...........	50			
Motorola, Inc..................	50			
Siemens & Halske...............	10			
Walt Disney Productions.........	6			
Swedish Ball Bearing Co..........	16$\frac{1}{8}$			

REPORT OF THE AUDITOR

To the Executive Committee,
American Economic Association:

We have examined the statement of assets and liabilities of American Economic Association (a District of Columbia corporation, organized not for profit) as of November 30, 1965, and the related statements of changes in restricted fund balances and income and expenses for the year then ended. Our examination was made in accordance with generally accepted auditing standards, and accordingly included such tests of the accounting records and such other auditing procedures as we considered necessary in the circumstances. It was not practicable to confirm accounts receivable, but we have satisfied ourselves as to such accounts by means of other auditing procedures.

In our opinion, the accompanying statements present fairly the assets and liabilities of American Economic Association as of November 30, 1965, and the changes in restricted fund balances and income and expenses for the year then ended, in conformity with generally accepted accounting principles applied on a basis consistent with that of the preceding year.

Chicago, Illinois,
December 16, 1965

ARTHUR ANDERSEN & Co

EXHIBIT 1

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF ASSETS AND LIABILITIES
NOVEMBER 30, 1965

Assets

CASH:		
Savings account	$ 80,203	
Checking account	46,469	$126,672
MARKETABLE SECURITIES, at cost:		
United States Government obligations (quoted market $121,854)	$124,048	
Corporate stocks (quoted market $287,799)	157,836	281,884
ACCOUNTS RECEIVABLE, less reserve of $800		22,134
ADVANCES, related to U. S. Government foreign student grant		11,473
PREPAID EXPENSES		1,615
OFFICE FURNITURE AND EQUIPMENT, at cost less reserve for depreciation of $1,720		3,953
		$447,731

Liabilities

ACCOUNTS PAYABLE		$ 22,327
RESERVE FOR HANDBOOK SUPPLEMENT		16,000
DEFERRED INCOME:		
Life membership dues	$ 26,800	
Prepaid dues	6,720	
Prepaid subscriptions	20,054	53,574
RESTRICTED FUND BALANCES (Exhibit 2)		117,938
GENERAL FUND BALANCE:		
Balance, November 30, 1964	$206,398	
Add—Excess of income over expenses for the year ended November 30, 1965 (Exhibit 3)	31,494	237,892
		$447,731

EXHIBIT 2

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF CHANGES IN RESTRICTED FUND BALANCES FOR THE YEAR ENDED NOVEMBER 30, 1965

Fund	Balance Nov. 30, 1964	Receipts	Disbursements	Balance Nov. 30, 1965
The Ford Foundation grants for—				
Translation of foreign economic publications and survey of foreign economic research	$ 42,385	$ 46	$ 1,442	$ 40,989
Publication of the *Journal of Economic Abstracts*	7,344	800	6,918	1,226
Clearinghouse for economists interested in overseas assignments	22,291	418	11,142	11,567
Foreign student screening and selection	74,591	4,549	30,800	48,340
The government of the United States of America grant for foreign student screening and selection	—	15,319	2,311	13,008
The Carnegie Corporation of New York grant for travel expenses of delegates to international meetings	982	—	600	382
The Asia Foundation for Asian economist membership dues to the American Economic Association and related travel expenses	1,654	2,500	2,506	1,648
The Agricultural Development Council, Inc.	1,162	—	495	667
The Rockefeller Foundation grant for survey articles on recent developments	1,625	—	1,625	—
Sundry grants	176	—	65	111
	$152,210	$23,632	$57,904	$117,938

EXHIBIT 3

AMERICAN ECONOMIC ASSOCIATION
STATEMENT OF INCOME AND EXPENSES FOR THE YEAR ENDED NOVEMBER 30, 1965

INCOME:		
Membership dues		$102,678
Subscriptions		42,726
Advertising		47,273
Sale of copies, republications and handbooks		9,783
Sale of mailing list		12,164
Annual meeting		11,503
Interest and dividends		14,771
Gain on sale of securities		2,192
Sundry		1,496
Total income		$244,586
EXPENSES:		
Publication (Exhibit 4)	$130,015	
Administrative (Exhibit 4)	78,077	
Grant to Yale University for cumulative *Index of Economic Journals*	5,000	213,092
Excess of income over expenses		$ 31,494

EXHIBIT 4

American Economic Association
Statement of Publication and Administrative Expenses for the Year Ended November 30, 1965

Publication Expenses:	
Printing of—	
Review	$ 66,29
Proceedings	23,26
Provision for handbook supplement	8,00
Addressing machine rental and expenses	8,24
Editorial salaries	20,27
Payments to contributors	1,38
Editorial supplies and expense	2,55
Total publication expenses	$130,01
Administrative Expenses:	
Secretary's salary, less portion allocated to restricted funds of $4,200	$ 8,36
Office salaries	33,49
Office machine rentals	55
Postage	3,10
Stationery, printing and supplies	2,86
Insurance	36
Annual meeting expenses	1,50
Executive Committee expenses	2,20
Committee on the National Science Foundation Report on the Profession expenses	5,16
Committee on Economic Education expenses	1,16
Other committees' expenses	1,34
International Economic Association dues and expenses	1,13
Archives	3,06
Pension annuity payments	8,99
Payroll taxes	1,34
Depreciation of furniture and fixtures	44
Telephone and telegraph	94
Sundry	2,02
Total administrative expenses	$ 78,07

REPORT OF THE MANAGING EDITOR FOR THE YEAR ENDING DECEMBER 1965

The number of manuscripts submitted during 1965 was 420, which is only a few under last year's record high of 431. Table 1 shows these figures for each year since 1948; it also records the number of manuscripts published each year, and the ratios of published papers to the totals received. Although in the past two years 126 papers have been published, an even larger number (143) has been accepted for publication, resulting in a fairly large backlog of papers awaiting publication.

TABLE 1
MANUSCRIPTS SUBMITTED AND PUBLISHED, 1948–65

Year	Submitted	Published	Ratio of Published to Submitted
1948	205	48	.23
1949	200	53	.27
1950	197	53	.27
1951	222	49	.22
1952	190	47	.25
1953	234	51	.22
1954	231	47	.20
1955	245	41	.17
1956	242	48	.20
1957	215	40	.19
1958	242	46	.19
1959	279	48	.17
1960	276	46	.17
1961	305	47	.15
1962	273	46	.17
1963	329	46	.14
1964	431	67	.16
1965	420	59	.14

Table 2 provides the breakdown of the 1965 volume's contents between articles, review articles, communications, book reviews, etc. Similar information for 1963 and 1964 is also shown. The number of pages devoted to leading articles increased in 1965 over the previous year but there were only minor changes in the other sections.

TABLE 2
SUMMARY OF CONTENTS, 1963–65

	1965		1964		1963	
	NUMBER	PAGES	NUMBER	PAGES	NUMBER	PAGES
Leading articles	17	443	19	367	19	508
Review articles	1	22	2	16	0	0
Communications:						
Original	12	110	13	95	11	66
Comments and replies	29	118	33	131	16	57
Books reviewed	189	377	190	386	209	372
Classified lists:						
New books		77		76		71
Periodicals		84		73		74
Dissertations		43		47		39
Notes		53		46		46
		1,327*		1,237*		1,233*

* Plus some blank pages.

Table 3 shows the several most popular "fields" in 1965, as reflected in the subject matter of all manuscripts submitted during the year. About 25 percent of all manuscripts fell in the four areas of monetary theory and policy, fiscal theory and policy, production functions, and international financial problems. Other popular "fields," not shown in the table, were industrial organization, consumption and investment functions, and neoclassical growth models.

TABLE 3
NUMBER OF MANUSCRIPTS SUBMITTED IN THE MOST POPULAR FIELDS IN 1965

Field	Number
Monetary and banking theory and policy	36
Fiscal and tax theory and policy	29
Production functions	19
International financial problems	18
Price and demand theory	17
Structural and other unemployment	16
Economics of underdeveloped countries	16
International trade theory	15

Table 4 summarizes the subject-matter distribution of accepted articles, review articles, and communications for 1965. The most interesting figures are those for articles and original communications, since to some extent these figures indicate the areas in which most work of publishable quality and of broad interest to economists is being done. The table shows, however, that we have accepted manuscripts in almost all of the listed areas.

TABLE 4
SUBJECT-MATTER DISTRIBUTION OF ACCEPTED MANUSCRIPTS IN 1965

	Articles	Review Articles	Original Communi-cations	Comments; Replies	Totals
General economics.......	2		1	5	8
Price theory............	2		4	5	11
Income theory..........	3		3	2	8
History of economic thought..............					0
Economic history, development, national economics...............	3			3	6
Social accounting........			1		1
Economic systems.......				2	2
Business fluctuations.....	2				2
Money and banking.....	3	1	1		5
Public finance...........	2		3	2	7
International economics..	2		2		4
Business finance.........				2	2
Business organization....					0
Industrial organization...	3		1	3	7
Land economics; housing.				2	2
Labor economics........	3		2		5
Consumption; welfare; population............	1			2	3
Unclassified.............					0
Totals..............	26	1	18	28	73

Table 5 presents the actual annual expenditures from 1961 through 1965 for the four regular issues of the *Review* and the estimated budgets for 1965 and 1966. Actual expenditures have risen since 1961 owing principally to increases in printing and mailing expenses and to higher office costs—the former because of the increased number of pages per copy and the larger number of copies printed, and the latter because of the appointment of a book review editor and increased expenses associated with the sharp rise in the number of manuscripts handled.

Budgeted expenditures for 1965 (as revised in the spring) were $82,535, while actual expenditures were $85,604. The difference is traced principally to higher printing and mailing costs; my budgeted expenditure for this item was based on an estimate of 19,000 copies per issue of the *Review*, but the actual number of copies averaged 20,400. On the other hand, actual office expenses were $1,600 under the budget figure.

The 1966 budget shows printing and mailing costs at $65,900, a figure based on an average of 21,500 copies per issue and a total number of pages for the four regular issues of 1,600 (including 1,325 pages of "text" and 275 pages of advertising). The budget also shows about a $1,000 increase over 1965 for office expenses.

TABLE 5

	Actual Expenditures					Budgets	
	1961	1962	1963	1964	1965	1965	1966
Printing and mailing	$44,146.91	$47,785.86	$51,644.09	$53,660.84	$61,519.13‡	$57,000.00	$65,900.00
Payments to contributors	2,921.90	3,375.00	2,308.00	1,424.50	1,598.00	1,425.00	1,600.00
Subtotal	$47,068.81	$51,160.86	$53,952.09	$55,582.11	$63,117.13	$58,425.00	$67,500.00
Office Expenses							
Editor's salary	$ 8,000.00	$ 8,000.00	$ 8,000.00	$ 8,000.00	$ 9,000.00	$ 9,000.00	$ 9,000.00
Assistant editor	—	1,500.00	—	1,833.32	2,000.00	2,000.00	2,000.00
Editorial assistance	9,750.79	10,385.05	9,702.06	11,626.74	10,024.08	11,660.00	10,900.00
Assistant to editor	6,600.00	6,600.00	6,234.44*	6,619.32†	6,619.32†	6,619.32†	7,000.00
Secretarial	1,544.38	1,664.38	1,442.25	1,632.75	1,725.00	1,800.00	1,800.00
Clerical	662.82	827.75	1,089.12	2,213.75	1,120.51	2,000.00	1,500.00
Proofreading, classif., etc.	943.59	1,292.92	936.25	1,160.92	559.25	1,240.68	600.00
Postage and supplies	901.25	851.95	1,165.50	1,348.23	1,462.32	1,350.00	1,500.00
Office equipment	—	—	812.63	122.07	—	100.00	100.00
Subtotal	$18,652.04	$20,737.00	$19,680.19	$22,930.36	$22,486.40	$24,110.00	$23,500.00
Total	$65,720.85	$71,897.86	$73,632.28	$78,512.47	$85,603.53	$82,535.00	$91,000.00

* Assistant's basic salary = $5,700.
† Assistant's basic salary = $6,200.
‡ Includes estimates for December issue.

Table 6 gives detailed information about printing and mailing costs by issues during 1965. The number of copies printed in 1965 averaged 20,400 compared to 18,500 the year before and to 18,050 in 1963. The successive increases are of course mainly related to the increases in membership and subscriptions.

TABLE 6
COPIES PRINTED, SIZE AND COST OF PRINTING AND MAILING IN 1965

	Printed	Pages		Cost		
		Net	Gross	Issue	Reprints	Total
March	20,000	327	408	$15,318.98	$ 92.02	$15,411.00
June	20,000	364	416	14,737.58	129.29	14,866.87
September	20,500	366	424	15,840.14	151.12	15,991.26
December	21,000	270	352	15,150.00*	100.00*	15,250.00*
	81,500	1,327	1,600	$61,046.70	$472.43	$61,519.13

* Estimated.

During the year I have had the advice and assistance of the following foreign correspondents, who have been particularly helpful with regard to the selection of foreign books for listing and review:

Maurice Flament (France)
Erich Schneider (Germany)

Three members of the Board of Editors complete their three-year terms of office at this time: Robert Clower, Abba Lerner, and Albert Rees. The Association owes them a heavy debt of gratitude for the generous expenditure of time they have made in the interests of the *Review,* and I very much appreciate their constant willingness, even when other obligations pressed, to review manuscripts and offer editorial advice. I nominate as their successors on the Board, for three-year terms beginning in 1966: Hirofumi Uzawa, Robert Eisner, and Stanley Lebergott. The burden of an increased work-load makes it desirable to raise the number of members of the Board of Editors from eight to ten. To meet this need, I nominate Hollis B. Chenery and Richard R. Nelson for three-year terms beginning in 1966.

During the year I have frequently sought the aid of members of the profession in addition to the members of the Editorial Board—partly to relieve the latter of what would otherwise be an impossibly heavy burden and partly to obtain advice of specialists in particular areas not represented on the Board. The following have assisted in this way:

M. Abramovitz
M. Adelman
W. Alberts
A. Alchian
O. Altman
A. Ando
K. Arrow
M. Bailey
B. Balassa
D. Bear
G. Becker
S. Becker
J. Berliner
F. Boddy
R. Bodkin
W. G. Bowen
M. Brennan
M. Bronfenbrenner
E. C. Brown

M. Brown
O. H. Brownlee
H. Bruton
P. Cartwright
J. Chipman
A. Collery
J. Cross
J. Crutchfield
G. Debreu
F. De Leeuw
H. Demsetz
E. Denison
T. Dernburg
E. Despres
D. Dewey
P. Diamond
D. Dillard
R. Dorfman
J. Dunlop
R. Easterlin
O. Eckstein
R. Eisner
D. Ellsberg
C. E. Ferguson
M. Friedman
I. Friend
G. Fromm
V. Fuchs
L. Gallaway
F. Gehrels
R. A. Gordon
M. Gort
H. Grubel
F. Hanna
A. Harberger
J. Harsanyi
B. Hickman
J. Hirshleifer
D. Hodgman
D. Holland
G. Horwich
Y. Ijiri
H. G. Johnson
R. Jones
D. Jorgenson
T. Justin
A. Kahn
J. Kareken
J. Kendrick
P. Kenen
C. Kindleberger
T. Koopmans
L. Krause
I. Kravis
M. Kreinin
E. Kuh
S. Kuznets
J. Letiche
W. Liebhafsky
J. Lintner
G. Maddala
B. Malkiel
A. Manne
J. Margolis
J. Marschak
A. Marty
E. Mason
R. McKinnon
G. Meier
A. Meltzer
H. P. Miller
M. Miller
J. Mincer
F. Modigliani
R. Musgrave
R. Nelson
M. Nerlove
W. Nichols
F. Norton
G. W. Nutter
G. Orcutt
B. Pashigian
J. Pechman
S. Peltzman
E. Phelps
R. Powell
J. Power
R. Radner
F. Raines
M. Reder
A. Rivlin
E. Rolph
G. Rosenbluth
R. Russell
P. Samuelson
R. Sato
W. Schmidt
J. Schmookler
E. Shaw
H. Simon
P. Simpson
P. E. Smith
V. Smith
W. L. Smith
E. Solomon
H. Somers
R. Solow
H. Stein
J. Stein
D. Suits
P. Sweezy
L. Tarshis
R. Teigen
E. Thompson
J. Tobin
J. Viner
H. Wagner
H. Wallich
T. Whitin
J. Witte
L. Yeager
E. Zabel

Respectfully submitted,
JOHN G. GURLEY, *Managing Editor*

REPORT OF THE COMMITTEE ON ECONOMIC EDUCATION

Pursuant to the new policy statement adopted by the Executive Committee in March, 1964, the Committee on Economic Education has moved to develop a more active program for the Association in this area. In addition to carrying on the projects reported last year, we have undertaken eight new activities. The first three are at the high school and prehigh school levels; the next two involve college teaching; and the last three are concerned with new relationships with government agencies in connection with new programs they are developing.

1. If better economic education is to occur in the high schools and grade schools, the key moves must be made in the schools at the local levels. This Committee, therefore, has developed an informal roster of well-known economists who have indicated they are willing to help local school boards, school administrators, and teachers in their individual areas in the development of new courses, the choice of reading materials, and other issues in connection with the teaching of economics in the schools. We have written to superintendents and other leading officials in most of the areas represented, many of them large metropolitan areas, indicating the availability of these economists. Suggestions from school officials indicate that availability of such professional competent help can play a valuable role when school systems review and reconsider their teaching in particular areas such as economics.

2. Three members of this Committee serve as a special advisory committee to the Joint Council on Economic Education. In this connection we have had a considerable role in the development of a major, nationwide project of the J.C.E.E. to stimulate thorough reconsideration of the teaching of economics at the grade and high school levels in a total of some thirty major school systems. In three of these systems major three-year projects are under way, developed with the advice of the members of this committee. These projects involve a thorough restudy of the place of economics in the curricula, teaching materials used, training of teachers, teaching methods, and the like. In the other schools the projects are scheduled for one year rather than three, but the general objectives are the same, though usually limited to only one or two grades. It is planned that the results of these "model" and "pilot" school projects, including curriculum design, new teaching materials, findings on teaching methods, and other developments of value will be widely disseminated to other school systems throughout the United States.

3. The advisory committee to the J.C.E.E. has also participated actively in the development of a new J.C.E.E. publications program to provide improved materials for students in the high schools on economics for use in "noneconomics" courses—notably American history, problems of democracy, civics, and general business. School administrators and teachers indicate a strong need for "units" on economic issues and problems for inclusion in these courses at appropriate times. Many more high school students are exposed to economics in

such courses than in such courses designated as "economics." The J.C.E.E program aims at developing small to medium-sized units on economic issues for all these courses. Each unit is to be developed jointly by a well-known economist and one or two high school teachers who understand at least something of economics and who have a first-hand grasp of the teaching problems involved in the individual courses concerned. This project will extend over at least two or three years; the first two units have already been commissioned. As in the preceding project, funds are being provided partly by the Ford Foundation and partly by grants from a variety of sources to the J.C.E.E.

4. At the college level, this committee developed with the J.C.E.E a project to make available to the profession information on interesting experiments in the teaching of basic economics at different institutions. The Committee invited institutions throughout the United States to submit reports for possible publication in a special volume which will be widely distributed. Information was received on some twenty-five courses. After reviewing this material, the Committee felt it would be preferable to have a special article or brochure developed providing an overview of the main lines of development illustrated by these approaches rather than selecting only four or five for full publication. Professor Bernard Haley has agreed to prepare such a summary. His study will be published in the *American Economic Review* or as a special brochure within the year; it will include references to projects at individual institutions, and detailed information on the developments at those institutions will be made available for those who want more complete information.

5. The Committee has recommended to the J.C.E.E. that it establish a special committee of distinguished economists, aided by outstanding men in the psychology-testing field, to develop a college-level "Test of Economic Understanding," which might be used by different institutions to evaluate the success of their elementary courses. Independent financing has been obtained for development of such a test, and the committee is now being formed. Technical assistance will be provided by two of the nation's outstanding psychologists, and the field testing and evaluation of questions will be handled by a highly reputable professional testing organization. It is hoped that the test will be available within about a year and a half. This parallels the high school level "Test of Economic Understanding," developed by a comparable committee about two years ago, which has now been used widely and which has been acclaimed as an extremely useful tool for teachers and administrators in making comparative judgments on the successes and weaknesses of their teaching of economics at the high school level.

6. This Committee, at the request of interested senators and representatives, supported the proposal to include economics as one of the discipline eligible for support under the Education Act of 1965. For the first time, economics has now been included as an eligible discipline. This means that economics will be eligible for support in the various activities covered by the Education Act.

7. At the request of the U.S. Office of Education, members of this Committee have served on a special advisory group to help the Office of Education de-

velop guidelines for the support of summer and academic-year workshops on economic education, especially for the development of teachers below the college level. An announcement of this new program was carried in the March, 1965, issue of the *American Economic Review*. The Committee is now attempting to improve the flow of information between the Office of Education and potentially interested departments of economics so that maximum advantage can be obtained from the substantial sum being allocated through this program to the improvement of the teaching of economics in the schools.

8. The National Science Foundation has indicated its interest in considering a proposal from the American Economic Association for the establishment of a "visiting scientist program" in economics, and this Committee is developing a proposal to the Foundation for such an experimental program. This will involve development of a roster of well-known economists who might visit smaller campuses in their areas for one or two days to discuss new developments in economics, teaching activities and problems, and any other issues which might serve to improve the teaching of economics at the undergraduate level on the campuses visited. If the program is established, an announcement will be carried in the *American Economic Review*. This will include information on how to request visits from the "distinguished scientist" roster. We hope that the program will be established by the autumn of 1966.

Beyond these particular activities, this Committee has received a steady, and surprisingly large, flow of correspondence from economists and others over the past year, covering a wide range of activities in the area of economic education at both the college and precollege levels. It has also been increasingly in touch with U.S. government agencies which are interested in developing new programs or improving existing programs that bear on the area of economic education. We consider this increased interest a substantial step forward in the general endeavor of the Association to stimulate more interest in economic education at all levels.

G. L. BACH, *Chairman*

REPORT OF THE COMMITTEE ON THE N.S.F. REPORT ON THE ECONOMICS PROFESSION

The Supplement to the December, 1965, issue of the *American Economic Review* represents a completion on schedule of the work of this Committee, as this work was outlined to the Executive Committee in December, 1964, and authorized in March, 1965. Because economists were included in the National Register for the first time in 1964, the report includes 25 pages of basic data, in addition to 60 pages of detailed analysis and 13 pages of summary, foreword, and indexes. In view of some unforeseeable problems in obtaining tabulations at the anticipated times, the Committee is pleased that it was able to obtain publication of its analysis of the 1964 data before the end of the succeeding calendar year. Officials of the National Science Foundation have praised this A.E.A. report as the most adequate one provided by any of the twelve cooperating professional societies.

An especially noteworthy feature of the current report is a regression analysis which summarizes the relative gross and net association with the economists' salaries of each of seven of their identifiable characteristics (Part V). This summary serves to place in perspective the more usual detailed distributions of absolute characteristics by various combinations of these same characteristics (Part III). The regression analysis was the voluntary contribution of Dr. Emanuel Melichar, Federal Reserve Board, whose attention to the data was attracted by the publication of the Committee's interim report in the A.E.A. *Papers and Proceedings* (May, 1965, pp. 629-32). Other unusual features of the current report include the attention given to the varying definitions of "economists" (Part I), the comparisons of economists' salaries shown by the National Register returns with those shown by the Census and other surveys (Part II), and a special report on the status of women economists (Part IV) contributed by Committee member, Alice Hanson Jones. In many respects, however, the Committee could do no more in the time available than to scratch the surface of the rich material which is potentially available from the 12,143 economists' schedules, 9,981 of which show basic salaries obtained from full-time civilian, professional work in 1964.

The National Science Foundation has indicated informally a considerable interest in making both a broader and a more intensive use of its cooperative agreement with the American Economic Association. Already budgeted for its 1966 National Register survey is a continuation of the essential contribution of this Association's Secretary-Treasurer for the formulation, collection, and editing of the economists' schedules. Beyond this, the N.S.F. would welcome consultation, based on the experience of members of the present Committee, in regard to its analysis and publication plans for future National Register data. More immediately, the N.S.F. would like to find ways to encourage additional analysis of the 1964 data along the lines suggested on pages 9-10 of the Committee's present report.

Dr. Melichar and the undersigned, in consultation with the National Regis-

ter analysts, have established a priority order for a number of specific research projects and have made tentative arrangement for a much faster and more flexible system of computer processing of National Register tapes than the system which the Committee had to use. It is believed that the results of these varied activities would not only be in the job interests of A.E.A. members but would also contribute the somewhat unique skills of economists to the scholarly analysis of manpower availability and the employment and salary structures of economists and of the professions generally. Accordingly, it is recommended that further implementation of the existing N.S.F.-A.E.A. agreement be given early and serious consideration.

N. ARNOLD TOLLES, *Chairman*

REPORT OF THE CENSUS ADVISORY COMMITTEE

The Census Advisory Committee of the American Economic Association met with the Director and Staff of the Census Bureau in January, 1965. The Secretary of Commerce, the Assistant Secretary of Commerce, the Deputy Assistant Secretary of Commerce, and representatives of the Office of Business Economics and the Office of Statistical Standards also joined in the meeting. At the Bureau's request, Committee members offered suggestions for new or expanded programs, most of which emphasized the need for (1) pertinent information at appropriate levels of regional, state, and area detail on the country's current economic problems—for example, poverty in relation to income and educational attainment, and impact of urban renewal and transportation systems on metropolitan congestion; and (2) making unpublished Census data more accessible to users outside the government through machine-readable punched cards and computer tape "data banks." The Committee also reviewed a discussion draft of the Bureau's proposed annual report on long-term economic growth, and recommended its publication as soon as possible. The Census-IRS Link project, the proposed 1967 Census of Construction, and recent Bureau activities in publicizing available unpublished Census data were also discussed.

The Technical Subcommittee on Business Cycle Developments of the Census Advisory Committee met with the Director and Staff of the Census Bureau in October, 1965. Also participating in the meeting was a representative of the International Monetary Fund.

The main focus of the discussion, as in earlier meetings, was on ways and means of improving the important Census periodical, *Business Cycle Developments*. The Technical Subcommittee reviewed a revised list of business cycle indicators—leading, coincident, and lagging—under preparation by the National Bureau of Economic Research. Under existing arrangements, these will be used by the Bureau of the Census in the *BCD* monthly publication. The Subcommittee offered various suggestions for modification, both in the plan of approach and in the specific series. In addition, the Subcommittee considered, without making a formal recommendation, proposals to add more international data to *BCD*—for example, on wholesale prices, stock prices and interest rates.

SOLOMON FABRICANT, *Chairman*

REPORT OF REPRESENTATIVE TO THE NATIONAL BUREAU OF ECONOMIC RESEARCH

The research program in 1965 included investigations in five principal areas: economic growth; national income, consumption and capital formation; business cycles; financial institutions and processes; and international economic relations. New studies were started or planned for early launching on: productivity, employment and price levels; economics of education; exploratory study of banking structure and performance; and balance-of-payments adjustment factors.

The conference program comprised the following: a Conference on Production Relations, sponsored by the Conference on Research in Income and Wealth, in October, 1965; a Conference on Investment Behavior, sponsored by the Universities-National Bureau Committee for Economic Research, in June, 1965; a Special Conference on Measurement and Interpretation of Job Vacancies devoted to discussion of plans for obtaining comprehensive information on job vacancies in the United States, in February, 1965. The volume reporting the proceedings of this last conference is expected to be published early in 1966.

Nine reports resulting from the National Bureau's program were published in 1965 and six others were in press in December, scheduled to be issued early in 1966. Of these fifteen publications, five are books by the staff, six are occasional papers by the staff, and four are books of conference proceedings.

At the 1965 Annual Meeting of the National Bureau, Walter W. Heller, Geoffrey H. Moore, J. Wilson Newman, and Gus Tyler were elected Directors at Large. Officers elected were Frank W. Fetter, Chairman; Arthur F. Burns, President; Theodore O. Yntema, Vice President; Donald B. Woodward, Treasurer; Solomon Fabricant, Director of Research; Geoffrey H. Moore and Hal B. Lary, Associate Directors of Research; and William J. Carson, Executive Director and Secretary.

Following Solomon Fabricant's request to be relieved of administrative duties, the Board elected Geoffrey H. Moore as Director of Research effective June 1, 1965. Dr. Fabricant is continuing as a member of the Board and of the research staff. Victor R. Fuchs was appointed Associate Director of Research. Douglas H. Eldridge was appointed Executive Director effective July 12, 1965. Dr. William J. Carson, formerly Executive Director, is continuing to serve as Secretary of the Board through February, 1966. Michael Michaely, Hebrew University, and George H. Hempel, Washington University, were appointed research fellows in 1965-66. Joseph W. Conard, a member of the research staff since 1960, died on April 5, 1965, and Nicholas Kelley, a member of the Board, died on October 28, 1965.

Columbia University Press is the distributor of current National Bureau publications. Effective November 19, 1965, arrangements were made with University Microfilms, Inc., to make available and to distribute all National

Bureau out-of-print publications. Under the arrangement out-of-print reports may be obtained in one of three forms: (1) microfilm; (2) xerography; or (3) offset print. Orders for any out-of-print reports can be transmitted directly to University Microfilms, Inc.

WILLARD L. THORP

REPORT OF THE REPRESENTATIVE TO THE UNITED STATES NATIONAL COMMISSION FOR UNESCO

As a representative of the American Economic Association on the United States National Commission for UNESCO, I have participated actively in the Social Science Committee of that Commission and have also been appointed to the Executive Committee of the Commission. I attended the national conference organized by the Commission in Kansas City in November on the subject of "Man, Knowledge, and Freedom in International Development."

The Social Science Committee met in Washington in January and again in Chicago in March and prepared a very careful report. The Commission, however, only has an advisory capacity to the State Department, and the State Department did not see fit to take our advice. Because of its peculiar composition, the lack of continuity, and lack of support on the part of the State Department, both for the Commission and for UNESCO, working on the Commission is a highly frustrating experience. The Commission, however, is one of the very few windows which the State Department has on the world, especially on the world of the social sciences, and I recommend, therefore, that we continue our membership until such time as the Association is rotated off the Commission.

The Social Science Department of UNESCO in Paris has been doing a study of the state of the social sciences in their constituent countries, to which I have made a modest contribution of a largely personal nature.

KENNETH E. BOULDING

REPORT OF THE REPRESENTATIVE TO THE NATIONAL ACADEMY OF SCIENCES—NATIONAL RESEARCH COUNCIL

Most activities sponsored by the Division of Behavioral Science during 1964-65 continued to be those started before the Division came into existence; i.e., to reflect the interests of its predecessor, the Division of Anthropology and Psychology. Upon the recommendation of the Division, however, the Governing Board of the Academy-Research Council approved acceptance of funds to investigate the social aspects of economic development. Also, active advisory committees appointed by the Division's central office considered the possibilities of a large-scale documentation, information-processing project and special problems of certain types of governmental research in the social sciences. Other possible projects and advisory activities were considered.

The membership of the Division has been broadened. A statistician and a psychiatrist have been added as members at large, the Association of American Geographers has (at the Division's invitation) appointed a representative, and the Linguistic Society of America has been invited to appoint a representative.

HENRY OLIVER

REPORT OF THE REPRESENTATIVE TO THE AMERICAN COUNCIL OF LEARNED SOCIETIES

The A.C.L.S. has for the past few years been actively supporting efforts to have the federal government establish a counterpart to the National Science Foundation for the humanities and the arts. This effort, with the legislative and political support of high-ranking members of Congress, materialized with the passage by the 89th Congress of Public Law 89-209, establishing a National Foundation on the Arts and Humanities. President Johnson signed the Act into law on September 29, 1965.

It is unlikely that members of the A.E.A. will find the arts endowment personally productive as a source for research support and/or fellowship aid. Roger Stevens serves as the Chairman of the arts endowment, with an advisory group, the National Council of Arts. Authorized funds are $5 million for each of three fiscal years for the arts and humanities endowments. There is also provision for matching grants ($5 million for humanities) and special authorization ($1 million per year) to the Office of Education to support statewide programs in the humanities.

The structure and fiscal authorization for the national humanities endowment parallel exactly that for the arts. But the humanities part of the law may well be of some interest to members of the economics profession. It is your delegate's impression, gained from statements made at a panel discussion at the 1966 annual meeting of the A.C.L.S., that projects in economics with humanistic interest would fall into the scope of the programs authorized for the humanities endowment. Discussions are presently underway to establish guidelines for scholars, universities, and other organizations seeking support from the endowment. It is not yet clear, for example, whether or not predoctoral fellowships will be included. The wording of the Act does, however, seem broad enough to include programs and projects paralleling those of N.S.F.

The relevant portions of the humanities endowment titles of the Act are: "(1) develop and encourage the pursuit of a national policy for the promotion of progress and scholarship in the humanities; (2) initiate and support research and programs to strengthen the research potential of the United States in the humanities by making arrangements (including grants, loans, and other forms of assistance) with individuals or groups to support such activities; (3) award fellowships and grants to institutions or individuals for training and workshops in the humanities (fellowships awarded to individuals under this authority may be for the purpose of study or research at appropriate nonprofit institutions selected by the recipient of such aid, for stated periods of time); (4) foster the interchange of information in the humanities; (5) foster, through grants or other arrangements with groups, public understanding and appreciation of the humanities; and (6) support the publication of scholarly works in the humanities." Further information may be obtained from the Chairman, National Endowment for the Humanities, 1800 G Street, N. W., Washington, D. C.

Inasmuch as your delegate did not file a report of the 1965 Annual Meeting of the A.C.L.S., it may be useful to recount a few matters of interest to members of the A.E.A. The A.C.L.S. is composed of thirty-two constituent scholarly societies in the humanities, and, as set forth in its constitution, its purpose is "the advancement of humanistic studies in all fields of learning and the maintenance and strengthening of relations among the national societies devoted to research studies." The Council's funds, derived mainly from gifts by educational foundations, are used principally for support of individual scholars for research fellowships and grants. The Council has six area programs (jointly sponsored with the Social Science Research Council) but directly administers only the Asian Studies and Slavic Studies. It has authorized for the coming fiscal year larger amounts for Studies of Chinese Civilization. The Council has an extensive program to encourage American studies in the universities of Western Europe, supported from Ford Foundation funds. Special language programs, exchanges with U.S.S.R. scholars, and support for international scholarly congresses in the U.S. are other programs that might be of interest to economists.

Your delegate feels, after attending two of the annual meetings of the Council and examining closely its various programs and awards, that members of the Association are underrepresented in terms of the available awards for which economists are eligible. Your delegate wishes to encourage members of the Association to examine more carefully than seemingly has been done in the past the various kinds of support offered by the A.C.L.S. A copy of the Annual Report describing the various activities of the Council may be had free by writing to the A.C.L.S., 345 East 46th Street, New York, New York, 10017.

RALPH L. ANDREANO

REPORT OF THE REPRESENTATIVE TO THE INTERNATIONAL ECONOMIC ASSOCIATION

In 1965, the International Economic Association held two conferences:

1. The fourth annual conference for younger economists was held at Ditchley Park, Oxfordshire, England, in April. The subject was, "The Economic Problems of Housing." S. J. Maisel, then of the University of California, Berkeley, served as chairman of the program committee along with A. Andrezejewski (Poland) and A. Lindbeck (Sweden). There were twenty-six participants, including James Gillies, W. G. Grigsby, R. F. Muth, and Richard Netzer from the United States (in addition to Maisel).

2. A conference on "Economic Problems of Agriculture in Industrial Societies" was held in Rome, Italy, in September in connection with the meeting of the Council and the Executive Committee of the Association. G. U. Papi (Italy), president of the Association, served as program committee chairman. There were seventy-nine participants (larger than usual because Council members were included). Those from the United States were: George R. Allen, C. E. Bishop, Karl A. Fox, N. Georgescu-Roegen, Gottfried Haberler, B. F. Haley, D. E. Hathaway, Sidney Hoos, D. Gale Johnson, Glenn L. Johnson, W. H Nicholls, and J. H. Richter.

Two other conferences were held under the auspices of the Association during the year, but these were administered by other agencies:

1. A conference on "International Capital Movements and the Economics of Development," organized by the International Bank for Reconstruction and Development in collaboration with the International Monetary Fund and the Inter-American Development Bank. Henry Wallich (U.S.A.) was chairman of the program committee. The conference was held in Washington in July.

2. A conference on "Economic Organisation in Eastern Europe," under the auspices of a committee of which John Dunlop (U.S.A.) was chairman, was held at Bellagio, Italy, in September.

The Council of the Association held its triennial meeting at the time of the September conference in Rome. Representatives on the Council from the American Economic Association were B. F. Haley and D. Gale Johnson. Gottfried Haberler, honorary president of the Association, also attended. Activities of the Association over the past three years were reviewed, and particular attention was devoted to the financial situation of the Association. The five-year grant of the Ford Foundation ($35,000 per year) comes to an end with 1965, and the Association has not been given any encouragement as to the possibility of a renewal of a general grant. Activities at their current level require about $50,000 a year, of which dues and voluntary payments from member associations provide about $4,000, UNESCO contributes $9,000 (to be increased to $10,000 next year), and gifts from national banks about $7,000 (about $20,000 altogether). The Council proposed that contributions of member associations be increased, more extensive contributions from banks and

business concerns be sought, with the aid of member associations, and a new proposal for grants to aid the Association's activities in specified areas be made to the Ford Foundation.

New officers were elected for a three-year term beginning in 1966: Paul A Samuelson, United States, President; Erik Lundberg, Sweden, Vice-president; D. Delivanis, Greece, Treasurer.

It was agreed that the next Congress of the Association should be held in 1968; and the Executive Committee subsequently decided to accept an invitation to hold the meeting in Canada, probably in Montreal, with the proffered financial support of the Canadian government. The general subject for the Congress favored by the Council is "The Future of International Economic Relations," but the matter of subject as well as other details were left for a program committee, under the chairmanship of P. A. Samuelson, incoming president, to decide.

Conferences scheduled for 1966 are: (1) a spring conference for younger economists on "Risk and Uncertainty," to be held in Smolenice, Czechoslovakia; and (2) a September conference, in connection with the annual meeting of the Executive Committee, on "Economics of the Public Sector," with Julius Margolis (U.S.A.) as chairman of the program committee.

Two volumes resulting from earlier conferences have appeared during the past year: *The Theory of Interest Rates* (ed. D. C. Hague), and *Problems in Economic Development* (ed. E. A G. Robinson). Seven further volumes are in preparation or in press.

B. F. HALEY

REPORT OF THE POLICY AND ADVISORY BOARD OF THE INSTITUTE OF INTERNATIONAL EDUCATION

THE ECONOMICS INSTITUTE IN 1965

The 1965 Economics Institute was the biggest on record, with a final enrollment of 95 as compared to 61 in the previous year and 36 in the first year of operation (1958). An innovation of the 1965 session was the admission of a limited number of students for the second half of the Institute only; there were 25 of these. Another innovation was a lecture course with weekly seminars on the United States economy, designed to make it easier for foreign students to understand illustrative material based on United States economic institutions and to answer examination questions requiring a knowledge of these institutions. Special efforts were made to avoid giving a "propaganda" flavor to this course. On the Graduate Record Examination Advanced Test in Economics, however, Institute students still scored almost 25 percent lower on nontheory questions than on theory questions. Other innovations in 1965 were offering both micro- and macroeconomics in both halves of the program and increased emphasis on microeconomics for the lowest class in economics.

Responses to an American Economic Association questionnaire indicated that the major fields of interest of the students were economic theory and quantitative methods, followed by economic planning and development, international economics, banking and finance, and agricultural economics, in that order. Nearly 70 percent of the students were heading for departments of economics, only 14 percent for departments of agricultural economics, and the rest for special programs.

Among major regions of developing countries, Latin America was most heavily represented with 37 students, of which 12 were from Mexico. India was second to Mexico in number of students with 9 students, Colombia and Turkey had 7 each and Argentina and Japan 6 each. Thirty-two countries were represented in all, with Africa being notably underrepresented.

There continues to be considerable concentration of students of the Institute in a limited number of graduate schools, reflecting partly the general reputation of the economics departments in those universities, but also reflecting the presence or absence of special institutes or programs in economic development, operations in developing countries, and the like. Thus in 1965 the universities receiving the largest numbers of Institute students were Vanderbilt (9), Chicago (8), Colorado (7), California (6), Wisconsin (6), and Williams (5). Cornell, Iowa State, Pittsburgh, and Stanford received 4 students each. Over the entire eight-year life of the Institute the universities receiving the largest numbers of students have been Wisconsin (37), Vanderbilt (34), Colorado (30), Harvard (30), Chicago (28), Yale (24), Stanford (19), and California at Berkeley (19).

Total expenses of the Institute reached a new peak in 1965 but costs per

student were the lowest on record. (See Table 1.) Administrative costs were reduced in 1965 despite the increase in number of students. "Recoveries"—financial support from other organizations—were much higher in 1965 than ever before, and covered more than half of the total expenses (Table 2). Dr. Wyn Owen, of the University of Colorado, continued as Director of the Institute.

BENJAMIN HIGGINS

TABLE 1

SUMMARY COSTS OF THE ECONOMICS INSTITUTES, 1958–65

Year	Number of Students	Expenses			
		Administrative Expense*	Other Expense†	Total Expense	Total Expense per Student
1958	36	$12,395	$ 35,451	$ 47,846	$1,329
1959	49	8,453	45,745	54,198	1,106
1960	46	6,326	47,781	54,107	1,176
1961	53	4,509	52,778	57,287	1,081
1962	50	3,000	57,704	60,704	1,214
1963	55	11,160	67,535	78,695	1,413
1964	61	20,638	74,755	95,393	1,564
1965	95	15,017	87,270	102,287	1,077 (average)
Totals	445	$81,498	$469,019	$455,124	

* Administrative expense consists of off-campus charges, paid directly by I.I.E.: administrative overhead, pre- and post-Institute orientation and travel, Policy Board costs and other direct charges. All figures are rounded to the nearest dollar.

† On-campus charges.

TABLE 2

FINANCIAL STATEMENT—ECONOMICS INSTITUTE
GRANTS, 1963–64: $290,000.00

	Expenses	Recoveries
Expended, 1963 program	$ 78,695.00	
Recoveries, 1963		$27,280.88
Expended, 1964 program	95,393.00	
Recoveries, 1964		7,611.15
Expended, 1965 program	102,286.50	
Recoveries, 1965		52,653.10
Totals	$276,374.50	$87,545.13
Expenses less recoveries	$188,829.37	
Total grants less expenses	101,170.63	

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1966

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**Supplement No. 2.—Handbook. Pp. 143.

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† This issue is not included in the price of the volume.

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AMERICAN ECONOMIC REVIEW

VOL. LVI, No. 4 PART 2 SEPTEMBER, 1966

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THE AMERICAN
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VOL. LVI SEPTEMBER, 1966 NUMBER 4, PART 2

1966
HANDBOOK

Edited by HAROLD F. WILLIAMSON, *Secretary of the Association*

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CHARTER AND BYLAWS OF THE AMERICAN ECONOMIC ASSOCIATION

The charter of incorporation of the American Economic Association, incorporated in the District of Columbia, February 3, 1923, provides as follows:

I. The name or title by which the society shall be known is the American Economic Association.

II. The time for which it is organized is perpetual.

III. The particular business and object of the society are as follows:

1. The encouragement of economic research, especially the historical study of the actual conditions of industrial life;

2. The issue of publications on economic subjects;

3. The encouragement of perfect freedom of economic discussion. The Association as such will take no partisan attitude, nor will it commit its members to any position on practical economic questions.

IV. The number of its trustees for the first year of its existence shall be fourteen.

The following bylaws have been adopted for the government of the Association:

I. Membership

1. Any person interested in economic inquiry may, on the nomination of a member, be enrolled in this Association.

2. There shall be six classes of members other than honorary: members paying an annual fee of $10.00; family members (two or more living at the same address, second membership without subscription to the publications of the Association) paying an annual fee of $1.00; junior members (available to registered students for three consecutive years only) paying an annual fee of $5.00; subscribing members paying an annual fee of $12.00; contributing members paying an annual fee of $25.00 or more; and life members comprising those members who contribute $200.00 or more in a single payment. Life members shall be exempt from annual fees. Members shall have each year the privilege of designating the class of membership they choose for that year.[1]

3. Foreign economists of distinction, not exceeding twenty-five in number, may be elected honorary members of the Association. Past presidents of the Association and members who have been awarded the Walker Medal shall be Distinguished Fellows. In addition, the Executive Committee may elect up to a total of twelve additional Distinguished Fellows, but not more than two in any one calendar year, from

[1] As amended at the December 30, 1965, annual meeting.

economists of high distinction in the United States and Canada. Candidates for Distinguished Fellowships shall be nominated by the Nominating Committee or the Executive Committee, and they shall be elected by the combined vote of the two committees. The Nominating Committee shall solicit and give due consideration to the recommendations of the Committee on Honors and Awards. The Nominating Committee should be free to make no nominations in any particular year. However, it should not be limited as to the number of candidates it may nominate in any year. It should accompany each nomination with a supporting brief. Election to Distinguished Fellowship does not preclude election to any office of the Association.[1]

4. Every member is entitled to receive, as they appear, all reports and publications of the Association.

II. Trustees

The trustees of this Association shall not be less than fourteen or more than fifteen in number and the Board of Trustees shall be known as the Executive Committee. The Board of Trustees shall be composed of the persons elected as the Executive Committee at the time and place and in the manner set forth in the following bylaw providing for the election of officers.

III. Officers[2]

1. The Association shall have the following officers who shall be elective officers: a President, a President-elect, two Vice-Presidents, and six elected members of the Executive Committee. The terms of office of the President, President-elect, and the two Vice-Presidents shall each be one year. The terms of office of the six elected members of the Executive Committee shall each be three years, two of the six terms to expire each year. The President-elect elected in any year shall succeed to the presidency in the following year. Each regular term of office shall coincide with the calendar year or a multiple thereof.

2. Elective officers shall be chosen through elections to be held during the last three months of the terms of office of their predecessors. Each member shall be given the opportunity to vote by mail. The results of the election shall be certified and announced by the Secretary at the annual business meeting.

3. The Association shall have the following officers who shall be appointed by the Executive Committee: a Secretary, a Treasurer, a Managing Editor, and a Counsel. The terms of office of each of these officers shall be three calendar years.

4. The Executive Committee shall consist of the President, the President-elect, two Vice-Presidents, the Secretary, the Treasurer, the Managing Editor, the two ex-Presidents who have last held office, and six

[2] As amended at the December 29, 1958, and December 30, 1960, annual meetings.

elected members, provided that the Secretary, the Treasurer, and the Managing Editor shall not be entitled to vote in the Committee's meetings.

IV. Duties of Officers[2]

1. The President of the Association shall preside at all meetings of the Association and of the Executive Committee. In case of his disability, his duties shall devolve upon the President-elect and then the two Vice-Presidents in the order of their election, upon the Secretary, and upon the Treasurer.

2. Before October 1 of each year, the President-elect of the Association shall appoint a Nominating Committee for the following year, this Committee to consist of a past officer as Chairman and not less than five other members of the Association. The names of the Committee shall be published in the issue of the *American Economic Review* immediately following their appointment, together with an invitation to the general membership that suggestions of nominees for the various offices other than the presidency be sent to the Chairman of the Committee. The Nominating Committee for each year shall be instructed to present to the Secretary of the Association on or before September 1 a nominee for the president-elect and two or more nominations for each other elective office to be filled, except the presidency, all these nominees being members of the Association. The members of the Nominating and Executive Committee shall constitute an Electoral College which shall consider the nominee of the Nominating Committee for the President-elect and select a single candidate for that office; but space shall be provided on the ballot for that office for the individual voter's alternative choice.

The President-elect may, at his discretion and with the advice and consent of the Executive Committee, appoint a Program Committee for the annual meeting of the succeeding year.

3. The Secretary shall keep the records of the Association and perform such other duties as the Executive Committee may assign to him.

4. The Treasurer shall receive and have the custody of the funds of the Association, subject to the rules of the Executive Committee.

5. The Executive Committee shall have the control and management of the funds of the corporation. It may fill vacancies in the list of officers, and may adopt any rules or regulations for the conduct of its business not inconsistent with this constitution or with rules adopted at the annual meeting. It shall act as a committee on time and place of meetings and perform such other duties as the Association shall delegate to it. A quorum shall consist of five members.

6. The Managing Editor shall, with the advice and consent of the Executive Committee, appoint members to an Editorial Board to assist him. He shall be ex officio member and chairman of this Board. The

Editorial Board shall have charge of the publication of the *American Economic Review.*

7. The office of the corporation for legal purposes shall be at the office of the Counsel in the District of Columbia, and legal process against the corporation may be served on said Counsel.

V. Annual Meeting

The annual meeting of this corporation shall be held at such time and place as may be determined by the Executive Committee. Notice of such time and place shall be given by publication in the regular journal of the corporation, now known as the *American Economic Review,* at least ten days prior to such meeting. The first annual meeting shall be held at Providence, Rhode Island, on the 27th of December, 1923, at nine o'clock A.M., unless otherwise ordered by the Executive Committee.

VI. Amendments

Amendments, after having been approved by a majority of the Executive Committee present at a meeting regularly called, may be adopted by a majority vote of the members present at any regular meeting of the Association or by a majority of votes cast in a mail ballot authorized by the Executive Committee.[3]

[3] As amended at the December 30, 1941, annual meeting.

AMERICAN ECONOMIC ASSOCIATION
LIST OF MEMBERS
(As of May 31, 1966)

AARON, Henry, Int. Prog. in Taxation, 1563 Massachusetts Ave., Cambridge, Mass. 02138
AARONSON, David E., 1168 Tamarack St., N.W., Washington, D.C. 20012
ABADJIS, Andreas, 4000 Massachusetts Ave., N.W., Washington, D.C. 20016
ABBOTT, James A., 2208 30th, Lubbock, Tex.
ABBOTT, Jarold, 3706 Broadway, Kansas City 11, Mo.
ABBOTT, John B., 2521 Dwight Way, Berkeley, Calif. 94704
ABBOTT, Lawrence, 1193 Van Curler Ave., Schenectady, N.Y.
ABBOTT, Robert K., 54 Orient Ave., Melrose, Mass. 02176
ABDEL-GAWAD, Wagiha, Clark Univ., Dept. of Econ. and Soc., Worcester 10, Mass.
ABDELKHALIK, Ahmed R., P.O. Box 102, Bloomington, Ind. 47401
ABDEL-MEGUID, Adly F., 177 Hunter Ave., North Tarrytown, N.Y.
ABDO, Albert N., 12705 Buckingham Dr., Bowie, Md. 20715
ABDULAHAD, Faraj, 24 Sidlaw Rd., Apt. 16, Brighton, Mass. 02135
ABDUL-MAGID, Khamis, Min. of Fin. and Ind., Al Kuwait, Kuwait
ABE, Donald K., Standard Oil Co. of N.J., 30 Rockefeller Plaza, New York 20, N.Y.
ABEE, Ralph M., 1314 N. Beverly Glen, West Los Angeles, Calif. 90024
ABEL, Fred H., Farm Adj. Br., Farm Prod. Econ. Div., E.R.S., U.S.D.A., Washington 25, D.C.
ABEL, Harold, 2380 S. Holly Pl., Denver 22, Colo.
ABEL, Kenneth N. N., 9202 Worth Ave., Silver Spring, Md.
ABEL, Martin E., U.S. Dept. of Agric., Econ. and Statis. Anal. Div., Econ. Res. Serv., Washington, D.C. 20250
ABELES, Gerard, 67-75 Fleet St., Forest Hills, N.Y. 11375
ABELLE, Barnie E., Univ. of Missouri at Kansas City, Div. of Continuing Educa., 5101 Rockhill Rd., Kansas City, Mo. 64110
ABELSON, Milton, Ctr. for Int. Affairs, 6 Divinity Ave., Harvard Univ., Cambridge, Mass. 01922
ABERCROMBIE, C. L., Univ. of Illinois, Dept. of Marketing, Urbana, Ill. 61803
ABERNATHY, John H., P.O. Box 16773, Louisiana State Univ., Baton Rouge, La. 70803
ABERNATHY, Lewis M., North Texas State Univ., Denton, Tex.
ABLAHAT, Newton A., 7009 Radnor Rd., Bethesda, Md. 20034
ABLE, Robert L., 968 Fuller Rd., Colorado Springs, Colo. 80907
ABLIN, Richard S., State Univ. of N.Y. at Buffalo, Dept. of Econ., Crosby Hall, Library Circle, Buffalo, N.Y. 14214
ABNER, Carl E., Univ. of Louisville, Dept. of Econ., Louisville, Ky. 40208
ABOUCHAR, Alan, USAID/B ADDP, APO New York, N.Y. 09676
ABOYADE, Oje, Faculty of Econ. and Soc. Studies, Univ. of Ibadan, Ibadan, Nigeria
ABRAHAM, Carl H., 60 W. 57th St., New York, N.Y. 10019
ABRAHAM, James, 6802A Townbrook Dr., Baltimore, Md. 21207
ABRAHAM, William I., New York Univ., Dept. of Econ., Washington Sq., New York 3, N.Y.
ABRAHAMS, Allen E., 270 Jay Street, Apt. 6H, Brooklyn 1, N.Y.
ABRAHAMSON, Albert, P.O. Box 128, Brunswick, Me. 04011
ABRAHAMSON, John D., 3127 Patterson Pl., N.W., Washington, D.C. 20015
ABRAHAMSSON, Bernhard Josef, 1802 Metzerott Rd., Adelphi, Md. 20785
ABRAMOVITZ, (Mrs.) Carrie, 543 W. Crescent Dr., Palo Alto, Calif.
ABRAMOVITZ, Moses, 543 W. Crescent Dr., Palo Alto, Calif.
ABRAMOWITZ, Howard D., Skidmore College, Dept. of Soc., Saratoga Springs, N.Y. 12866
ABRAMSON, Adolph G., Benson Apts., Township Line Rd., Jenkintown, Pa. 19046
ABRASSART, Arthur E., 605 S. Randolph, Champaign, Ill.
ABT, Seymour T. R., 1438 S. Canfield Ave., Los Angeles, Calif. 90035
ABY, Brian H., Sam Houston State Teachers Col., Dept. of Econ., Huntsville, Tex.
ACHENBAUM, Alvin A., 34 Mallard Rise, Irvington, N.Y. 14081
ACHINSTEIN, Asher, 8504 Meadowlark Lane, Bethesda, Md. 21811
ACKERMAN, George F., 111 Dyer Ave., Emerson, N.J. 07630
ACKERMAN, Joseph, 600 S. Michigan Ave., Chicago, Ill. 60605
ACKERMAN, Sol, 8000 Park Overlook Dr., Bethesda, Md. 20034
ACKLEY, Gardner, 4655 Garfield St., N.W., Washington, D.C. 20007
ACKLEY, Stanley W., Rutgers the State Univ., Univ. Col., New Brunswick, N.J.
ACOSTA, José D., 6416 Kenhowe Dr., Bethesda, Md. 20034
ADAMOVICH, Ljubisa S., Zdravka Celara St. 27, Belgrade, Yugoslavia
ADAMS, Aden C., Univ. of Tennessee, Glocker 114, Knoxville, Tenn.
ADAMS, Curtis H., 114 E. 23rd St., Hays, Kans.
ADAMS, E. Sherman, 130 Belden Hill Rd., Wilton, Conn. 06897
ADAMS, Earl William, Jr., Amherst Col., Dept of Econ., Amherst, Mass.
ADAMS, Eric G., 3 Thorncliffe Sq., Toronto 17 Ont., Canada
ADAMS, F. Gerard, 105 Ivywood Lane, Radnor Pa. 16144
ADAMS, George P., Jr., Dept. of Econ., Cornel Univ., Ithaca, N.Y.
ADAMS, James W., Univ. of Georgia, Col. o Bus. Admin., Econ. Dept., Athens, Ga.
ADAMS, John B., 9121 Kahului Dr., Huntingtor Beach, Calif. 92646
ADAMS, John F., 541 Elkins Avenue, Philadel phia 17, Pa.
ADAMS, John P., 8343 Hillside Rd., Alta Loma Calif. 91701
ADAMS, John Q., Univ. of Maryland, Dept. o Econ., College Park, Md.
ADAMS, John S., Univ. of Minnesota, Dept. o Geography, Minneapolis, Minn. 55455
ADAMS, Leonard P., 581 Warren Rd., Ithaca N.Y. 14850
ADAMS, Leonard W., 967 Westmoreland Ave. Syracuse, N.Y. 13210
ADAMS, Nassau A., P.O. Box 20, Room 724 G.C.P.O., New York 17, N.Y.
ADAMS, Robert F., Univ. of Maryland, Dept of Econ., College Park, Md.
ADAMS, Robert W., Univ. of Michigan, Grad Sch. of Bus. Admin., Ann Arbor, Mich. 48104
ADAMS, Thomas C., P.O. Box 3141, Portland Ore. 97208
ADAMS, Walter, Dept. of Econ., Michigai State Univ., East Lansing, Mich.
ADAMS, Walter Bailey, 6643 Sharon Dr., Gar den City, Mich. 48135
ADAMS, Walter W., 4415 Louise, Saginaw Mich. 48603
ADAMS, Warren E., APO 675 AID, New York N.Y. 09675
ADAMS, William C., East Texas State Col. Dept. of Bus. Admin., East Texas Sta., Bo: 3064, Commerce, Tex. 75429

ADAMSON, Raymond K., 27476 Sunnydale, Livonia, Mich. 48154
ADAMSON, Wendell M., P.O. Box 852, Holloman Air Force Base, N.M. 88330
ADDEN, Robert S., The Citadel, Charleston, S.C.
ADDINGTON, Arnold L., Rt. 8, Johnson City, Tenn.
ADELMAN, (Mrs.) Irma G., 5908 Cedar Pkwy., Chevy Chase, Md. 20015
ADELMAN, Morris A., 52 254 A, M.I.T., Cambridge, Mass. 01922
ADIE, Douglas K., 1341 E. Madison Pk., Apt. 2, Chicago, Ill. 60615
ADJEI, Augustine K., 309 W. 99th St., Apt. 7-C, New York, N.Y. 10025
ADLER, Hans A., 6656 Holland St., McLean, Va.
ADLER, John H., 5620 Western Ave., Chevy Chase, Md. 20015
ADLER, Norman, 7923 14th Street, N.W., Washington, D.C. 20012
ADLER, Robert W., R. 1, Box 258, Forest Grove, Ore.
ADLER-KARLSSON, Gunnar, Norrtullsgatan 55, II, Stockholm Va, Sweden
ADUDDELL, Robert M., 9660-E Lois Dr., Des Plaines, Ill. 60016
AERNI, Agathon A., Bank of America, Int. Bus. Dev. and For. Tr. Serv., 550 Montgomery St., San Francisco, Calif.
AFIFI, Hamdy H. H., 1802-D Orchard Pl., Urbana, Ill. 61801
AGAPITIDIS, Sotiris, 79, Vassilissis Sofias Ave., Athens, Greece
AGAPOS, A. Mike, 5960 W. 224th St., Cleveland, Ohio 44126
AGGER, Eugene E., Webb Gardens, Clifton Avenue, New Brunswick, N.J. 08901
AGNELLO, Richard J., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore, Md. 21218
AGOGLIA, Joseph R., 91 McKinley Ave., Brooklyn, N.Y. 11208
AHALT, J. Dawson, 4219 12 Rd. Apt. 21, Arlington, Va.
AHARI, Hossein, Bimeh Iran, Tehran, Iran
AHEARN, (Mrs.) Louise F., Drake Hotel, Apt. 1501, 1512 Spruce St., Philadelphia, Pa. 19102
AHERNE, Raymond J., 29 Grayson Lane, Newton Lower Falls, Mass. 01355
AHMAD, Khurshid, Rider Col., Dept. of Bus. Admin., Trenton, N.J.
AHMED, A. S. Mohiuddin, 143/D Dhanmondai Residential, Road No. 4, Dacca 2, East Pakistan
AHN, Seung Chul, 807 Riley Dr., Albany, Calif.
AHO, Reijo G., 1208 Fair Ave., Marquette, Mich. 49855
AHRENS, Curtis L., Tennessee Valley Authority, Div. of Agric. Dev., Agric. Econ. Br., Wilson Dam, Ala. 36587
AHTIALA, K. Pekka, Sch. of Soc. Sci., Kalevantie 4, Tampere, Finland
AIDENOFF, Abraham, 17 W. 82nd St., New York 24, N.Y.
AIGNER, D. J., Univ. of Illinois, Dept. of Econ., Urbana, Ill.
AIKIN, J. Charles, Dept. of Polit. Sci., Univ. of California, Berkeley, Calif.
AINSWORTH, Kenneth G., Allegheny Col., Meadville, Pa. 16335
AIROV, Joseph, Emory Univ., Dept. of Econ., Atlanta, Ga. 30322
AITCHISON, Beatrice, 1929 S St., N.W., Washington, D.C. 20009
AITKEN, Hugh G. J., 85 Dana St., Amherst, Mass. 01002
AITKEN, Norman D., Univ. of Massachusetts, Dept. of Econ., Amherst, Mass. 01003
AKERMAN, Clement, 332 S.W. Hamilton St., Portland, Ore. 97201
AKGUC, Oztin, Tax Inspection, Ministry of Fin., Maliye Mufettisi, Ankara, Turkey
AKIN, Alfred K., Jr., 7413 Oak Lane, Chevy Chase, Md. 20015
AKOLEKAR, Prabhakar S., St. John's Univ., Col. of Bus. Admin., 96 Schermerhorn St., Brooklyn, N.Y.
ALAIMO, Joseph W., 872 Burr Ave., Winnetka, Ill.
ALBACH, Horst, Waldstrasse 49, 532 Bad Godesberg, Germany
ALBAUM, Gerald S., Univ. of Arizona, Col. of Bus. and Pub. Admin., Tucson, Ariz. 85721
ALBERS, Henry H., Dept. of Labor Mgnt., State Univ. of Iowa, Iowa City, Iowa
ALBERT, Floyd B., 35 Park Rd., Scarsdale, N.Y. 10583
ALBERT, Hans, Turnerstr. 12, 69 Heidelberg, Germany
ALBERTINE, John, Kings Col., Wilkes-Barre, Pa. 18702
ALBERTS, Francis J., St. John Fisher Col., 3690 East Ave., Rochester, N.Y. 14618
ALBERTS, William W., Grad. Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
ALBERTSON, Charles L., 1644 Sixth Ave., Rockford, Ill. 61108
ALBERY, Michael, Econ. Adviser, Presidencia de la Republica, Oficina de Planificacion, San Jose, Costa Rica
ALBETSKI, Judith A., 95 Butterfield Ter., Amherst, Mass. 01003
ALBIN, Peter S., New York Univ., Grad. Sch of Arts and Sci., Dept. of Econ., Washington Sq., New York, N.Y. 10003
ALBRECHT, William P., Jr., Univ. of Iowa, Dept. of Econ., Iowa City, Iowa
AL-BUSTANY, Basil, 2517 20th Rd., N., Arlington 1, Va.
ALCALY, Roger E., 230 Witherspoon St., Princeton, N.J. 08540
ALDEN, Lucas A., Overlook Rd., Locust Valley, L.I., N.Y. 11560
ALDEN, Vernon S., 30 Gedney Park Dr., White Plains, N.Y.
ALDERFER, Evan B., Fed. Res. Bank of Philadelphia, 925 Chestnut St., Philadelphia 1, Pa.
ALDRICH, Neal E., 6015 Woodland Ter., McLean, Va. 22101
ALDRIDGE, James E., 304 1/2 E. 30th St., Austin, Tex. 78703
ALDUNATE-V., Paul, Purdue Univ., Dept. of Agric. Econ., West Lafayette, Ind. 47907
ALEXANDER, Albert, 131 Livingston St., Brooklyn 1, N.Y.
ALEXANDER, Alec P., Dept. of Econ., Univ. of California, Santa Barbara, Calif.
ALEXANDER, Charles K., 962 Waban Hill, Madison 5, Wis.
ALEXANDER, David R., 5563 Sanger Ave., Alexandria, Va. 22311
ALEXANDER, Joseph, Babson Inst., Babson Park 57, Mass.
ALEXANDER, Kenneth O., Michigan Tech., Dept. of Bus., Houghton, Mich. 49931
ALEXANDER, (Mrs.) Lois A., 9927 Taylor Ave., Washington, D.C. 20022
ALEXANDER, Milton, 44 Joan Dr., Yonkers 4, N.Y.
ALEXANDER, Robert J., Rutgers Univ., Dept. of Econ., New Brunswick, N.J.
ALEXANDER, Sidney S., 290 School St., Belmont, Mass. 02019
ALEXIS, Marcus, Univ. of Rochester, Col. of Bus. Admin., Rochester, N.Y. 14627
ALFARO-CASTILLO, Rafael, Avenida Roosevelt 25, San Salvador, El Salvador
ALFRED, Theodore M., Fairhaven Hall, Concord, Mass.
AL-GAILANI, (Mrs.) Rafida, Jadirija. Opposite of Baghdad Univ., Baghdad, Iraq
ALGIE, Harold E., 9446 Dee Rd., Des Plaines, Ill.
ALHADEFF, Albert V., 130 Salem Circle, Apt. A-2, Raleigh, N.C. 27609
ALHADEFF, David A., Univ. of California, 113 South Hall, Berkeley, Calif.
ALI, A. M., Kuwait Planning Inst., P.O. Box 5834, Al-Kuwait, Kuwait
ALI, Mohamed S., Industrial Bank of Sudan, P.O. Box 1722, Khartoum, Sudan
ALIBER, Robert Z., 6820 Meadow Lane, Chevy Chase, Md.
AL-KADDO, Badie J., 1815 Orchard Pl., Urbana, Ill. 61801
ALLAIN, Louis J., Purdue Univ., Dept. of Econ., West Lafayette, Ind. 47907
ALLAN, Ian K., Univ. of Natal, King George V Ave., Durban, S. Africa
ALLAN, John R., McMaster Univ., Dept. of Econ., Hamilton, Ont., Canada
ALLDREDGE, Rendel B., 215 Whitmoor Ter., Silver Spring, Md.

ALLEGRETTI, Enzo V., 56 Fisher Ave., Tuckahoe, N.Y. 13158
ALLEN, Bruce T., Michigan State Univ., Grad. Sch. of Bus., Dept. of Econ., East Lansing, Mich.
ALLEN, Carl W., Dundas Heights, Blacksburg, Va.
ALLEN, (Mrs.) Carol K., 20 Western Way, Princeton, N.J.
ALLEN, Clark L., Econ. Dept., Southern Illinois Univ., Carbondale, Ill.
ALLEN, Edward J., 8 Stratton Place, Plattsburgh, N.Y.
ALLEN, Edward L., 6028 Woodley Rd., McLean, Va.
ALLEN, Harry Kenneth, 813 S. University, Apt. G 813, Carbondale, Ill. 62901
ALLEN, John E., 1229 Dickinson Drive, Apt. 47-H, Coral Gables, Fla. 33146
ALLEN, John W., 906 E. Fairlawn, Urbana, Ill.
ALLEN, Julius W., 9901 Connecticut Ave., Kensington, Md. 20795
ALLEN, Robert F., 808 Bement St., Lansing, Mich. 48912
ALLEN, Robert L., Univ. of Oregon, Dept. of Econ., Eugene, Oregon
ALLEN, William A., Jr., 721 S. Wayne St., Arlington, Va. 22204
ALLEN, William R., Dept. of Econ., Univ. of California, Los Angeles 24, Calif.
ALLER, Curtis C., 2945 Newark St., N.W., Washington, D.C. 20008
ALLEY, W. Edward, 2612 Kingman Blvd., Des Moines 11, Iowa
ALLI, William E., 2803 Baker Lane, Bowie, Md. 20715
ALLIN, Bushrod W., 5214 Goddard Rd., Bethesda, Md. 20014
ALLISON, Don A., Jr., 602 Levering Ave., Los Angeles, Calif. 90024
ALLISON, Robert J., Colorado State Univ., Dept. of Econ. and Soc., Fort Collins, Colo.
ALLSBROOK, Ogden O., Jr., Univ. of Miami, Econ. Dept., Coral Gables, Fla.
ALLSBROOK, Robert C., 1673 Briarcliff Rd., N.E., Atlanta, Ga. 30306
ALMEIDA, Jose, Apardo 710, Rua Das Laranjeiras No. 1, Rio de Janeiro, Brazil
ALMON, Clopper, Jr., Univ. of Maryland, Dept. of Econ., College Park, Md. 20782
ALMOND, George L., Univ. of Maine, Stevens Hall, South, Orono, Me. 04473
ALNASRAWI, Abbas Abdul-Karim, Univ. of Vermont, Dept. of Econ., Burlington, Vt.
ALONSO, José G., Univ. of Puerto Rico, Col. of Soc. Sci., Dept. of Econ., Rio Piedras, P.R. 00745
ALPERN, Murray, 5918 Cross Country Blvd., Baltimore, Md. 21215
ALPERT, Paul, 12 Welwyn Rd., Great Neck, L.I., N.Y.
ALPERT, Philip, 303 W. 66th St., New York 23, N.Y.
AL-SAMARRIE, Ahmad M., 1603 Wrightson Dr., McLean, Va.
AL-SHAIKHLY, Falih, Univ. of Wisconsin, Agric. Econ. Dept., Agric. Hall, Madison, Wis.
ALSOBROOK, Henry F., Washington Univ., Dept. of Econ., Box 208, St. Louis, Mo. 63130
ALT, Richard M., New Eng. Mut. Life Ins., 501 Boylston St., Boston 17, Mass.
ALTAZAN, John E., L.S.U. in New Orleans, Lake Front, Div. of Bus. Admin., New Orleans 22, La.
ALTER, Gerald M., 5124 Linnean Ter., N.W., Washington, D.C. 20008
ALTERMAN, Jack, 9015 Garland Ave., Silver Spring 12, Md.
ALTHAUS, Paul G., Duke Univ., Dept. of Econ., Durham, N.C. 27706
ALTMAN, Oscar L., 922 24th St., N.W., Jefferson House, Apt. 521, Washington, D.C. 20037
ALTMAN, Stuart H., 1201 S. Courthouse Rd., Arlington, Va. 22204
ALTMANN, Murray, Fed. Res. System, Board of Gov., Washington 25, D.C.
ALTMAN-WINOGRAD, Marcos, a-c Cepal, Av. Providencia 871, Santiago, Chile
ALTON, Thad P., 425 Riverside Dr., Apt. 5H, New York 25, N.Y.
ALVES MARTINS, C., Rua D. Joao V, 18, 2°, Lisbon, Portugal
ALVIS, Vance Q., West Virginia Univ., Col. of Com., Morgantown, W.Va. 26506
ALVORD, Ben M., 246 E. 46th St., New York, N.Y. 10017
ALYEA, Paul E., 224 Orange St., Fairhope, Ala.
AMAN, John, 1211 Reid St., Newberry, S.C. 29108
AMANN, Victor F., US AID APO 319, New York, N.Y. 09319
AMARO, Joaquim F., Rua Laura de Araujo 178 Sobrado, Rio de Janeiro, Brazil
AMBACH, Dwight R., Dept. of State, Embassy /Santiago, Washington, D.C. 20521
AMBELANG, Paul L., Interstate Com. Comm. Bur. of Econ., Washington, D.C. 20423
AMEGLIO, Edgar E., 1101 17th St., N.W., Washington, D.C. 20036
AMEL, Bertram I., 800 West End Ave., New York, N.Y.
AMERKHAIL, Najibullah, Research Dept., Ministry of Planning, Kabul, Afghanistan
AMERLING, Gerald J., Jr., 805 Mt. Whitney, Klamath Falls, Ore.
AMERNICK, Jerry, 912 College Dr., Sault Ste. Marie, Mich. 49783
AMES, Edward, Purdue Univ., Dept. of Econ., Lafayette, Ind.
AMILL, Milton, 6 Joffre St., Santurce, P.R.
AMIR, (Mrs.) Herta, 10666 Ohio Ave., Los Angeles 24, Calif.
AMMER, Dean S., Northeastern Univ., Col. of Bus. Admin., Bur. of Bus. and Econ. Res., Boston 15, Mass.
AMMERMAN, Howard K., 10801 Rock Run Dr., Potomac, Md. 20854
AMMERMAN, James E., Asst. Treasury Representative, U.S. Embassy, APO 777, New York, N.Y. 09777
AMMON, A. J., 44 Exeter Rd., Short Hills, N.J. 07078
AMOA, Rowland K., 150-02 Goethals Ave., Apt. 43-C, Jamaica 35, N.Y.
AMOT-OLONG, S. J., Lango Co-op. Union Ltd., P.O. Box 59, Lira, Uganda
AMRHEIN, Joseph, 26 Holly Lane, Burlington, Vt. 05401
AMSELLE, Jean-Pierre, Univ. of California at Los Angeles, Dept. of Econ., 405 Hilgard Ave., Los Angeles 24, Calif.
AMUZEGAR, Jahangir, 2233 Wisconsin Ave., N.W., Washington, D.C. 20007
AMY, Norman L., 6425 Ivy Lane, San Jose, Calif.
AMZALAK, Moses, 9 Rua Gago Coutinho, Cascais, Portugal
ANAGNOS, Nicholas C., 9915 Ashburton Lane, Bethesda, Md. 21811
ANASTASOPOULOS, A., Univ. of Western Ontario, Dept of Econ., London, Ont., Canada
ANDERSEN, Anker V., Room 205 H, Northeastern Univ., 360 Huntington Ave., Boston, Mass.
ANDERSEN, Arthur T., 349 10th St., S.W., Washington, D.C. 20003
ANDERSEN, Leonall, 629 Villa Garden, Kirkwood, Mo. 63501
ANDERSEN, Paul N., Gl. Strandvej 19, Humlebaek, Denmark
ANDERSON, Allan S., Colfax, Wis. 54730
ANDERSON, Bernard, Arizona State Col., Box 4206, Flagstaff, Ariz.
ANDERSON, Bernard E., 1537 N. 60th St., Philadelphia, Pa. 19151
ANDERSON, Bruce W., 1100 McHugh, Apt. 1, Fort Collins, Colo.
ANDERSON, Charles D., 55 E. Tenth St., New York 3, N.Y.
ANDERSON, Claude, 350 Wilmar Ave., Pismo Beach, Calif. 93449
ANDERSON, Clay J., Dept. of Res., Fed. Res. Bank of Philadelphia, Philadelphia 1, Pa.
ANDERSON, Corliss D., 217 Linden Rd., Barrington, Ill. 60010
ANDERSON, Dole A., American Consulate-SP, APO New York 09676
ANDERSON, Edward A., Holicong, Bucks Co., Pa. 18928
ANDERSON, Edwin A., USAID—Santiago State Dept. Mail Room, Washington 25, D.C.
ANDERSON, Evan H., 204 S. 6th St., Zephyrhills, Fla. 33599

ANDERSON, Frederick W., Univ. of Saskatchewan, Regina Campus, Regina, Sask., Canada
ANDERSON, Garner L., 552 Ave. B., Seguin, Tex. 78155
ANDERSON, Gary R., 919 Locust St., Galesburg, Ill. 61401
ANDERSON, George R., Univ. of Michigan, 1213 Angell Hall, Ann Arbor, Mich.
ANDERSON, Gerald H., 1421 Oakwood Dr., Norman, Okla. 73069
ANDERSON, Henry, Univ. of Maryland, College Park, Md. 20740
ANDERSON, Howard C., 1710 H St. N.W., Washington 6, D.C.
ANDERSON, Karl L., Middlebury Col., Middlebury, Vt. 05753
ANDERSON, Leslie P., Univ. of Oregon, Sch. of Bus., Eugene, Ore. 97403
ANDERSON, (Mrs.) Lois M., 7605 Wildwood Dr., Takoma Park, Md. 20012
ANDERSON, Martin, Columbia Univ., Grad. Sch. of Bus., Rm. 619 Uris Hall, New York, N.Y. 10027
ANDERSON, Marvin S., Village Manor Apts., #30, 2601 Soldiers Home Rd., West Lafayette, Ind. 47906
ANDERSON, Paul H., 7605 Wildwood Dr., Takoma Park, Md. 20012
ANDERSON, Paul S., Fed. Res. Bank, Res. Div., Boston, Mass.
ANDERSON, Robert A., 2390 Davidson Ave., New York, N.Y. 10468
ANDERSON, Robert J., 4404 Locust St., Philadelphia, Pa. 19104
ANDERSON, Roy B., Seattle First Nat. Bank, P.O. Box 2087, Seattle 11, Wash.
ANDERSON, Roy C., Long Beach State Col., Dept. of Econ., Long Beach, Calif.
ANDERSON, Roy E., 2618 Flora St., San Luis Obispo, Calif. 93401
ANDERSON, Thomas C., 543 Catalpa, Birmingham, Mich. 48009
ANDERSON, Thomas J., Jr., Sch. of Com., New York Univ., Washington Sq., New York, N.Y.
ANDERSON, Walter C., 16 Northwood Rd., Asheville, N.C. 28804
ANDERSON, W. H., 4509 Don Quixote Dr., Los Angeles 8, Calif.
ANDERSON, W. H. Locke, Univ. of Michigan Dept of Econ., Rm. 221 Econ. Bldg., Ann Arbor, Mich.
ANDERSON, William H., 1735 N. Fairfax Dr., Arlington, Va.
ANDERSON, William O., 54 S. Cassingham Rd., Columbus 9, Ohio
ANDERSSON, Henry F., 1050 Driftwood Ave., Seal Beach, Calif. 90740
ANDIC, Fuat M., Inst. of Caribbean Studies, Univ. of Puerto Rico, Rio Piedras, P.R. 00745
ANDIC, V. E., Academy Sta., P.O. Box 8552, Albany, N.Y. 12208
ANDO, Albert K., Univ. of Pennsylvania, Wharton Sch. of Fin. and Com., Philadelphia, 4, Pa.
ANDREAE, Clemens A., 291 Aachenerstr., Cologne, Germany
ANDREANO, Ralph L., Univ. of Wisconsin, Dept. of Econ., Madison 6, Wis.
ANDRESS, (Mrs.) Ruth A., San Diego State Col., Soc. Sci. Div., Econ. Dept., San Diego, Calif. 92115
ANDREW, Charles F., c/o American Can Co., 100 Park Ave., New York, N.Y. 10017
ANDREW, Christopher O., 1625-J Spartan Village, East Lansing, Mich. 48823
ANDREWS, Charles H., Jr., Stetson Univ., Dept. of Econ., DeLand, Fla. 32720
ANDREWS, Daniel K., 224 Ridge Ave., Evanston, Ill.
ANDREWS, Peter B. B., 327 Howard Ave., Fairlawn, N.J.
ANDREWS, Robert A., Principia College, Elsah, Ill. 62028
ANDREWS, Samuel G., Univ. of Maine, South Stevens Hall, Orono, Me. 04473
ANDREWS, Victor L., Harvard Univ., Grad. Sch. of Bus. Admin., Soldiers Field, Boston, Mass. 02163
ANDREWS, William H., Jr., R. R. 7, Bloomington, Ind. 47401
ANDRON, Mortimer, Dept. of Econ., Univ. of California, Goleta, Calif. 93017
ANDRUS, J. Russell, APO-USAID, San Francisco, Calif. 96352
ANG, Henry S., California State Col. at Los Angeles, Los Angeles, Calif. 90032
ANGELINI, Anthony L. P., St. John's Univ., Col. of Bus. Admin., Jamaica 32, N.Y.
ANGELL, James W., Columbia Univ., Dept. of Econ., New York, N.Y. 10027
ANGELL, Wayne D., Ottawa Univ., Ottawa, Kans. 66067
ANGKATAVANICH, Virote, 429 Stockton Pl., Englewood, N.J. 07631
ANGLIN, (Mrs.) Hermione Y., 215 Whittier St., N.W., Washington, D.C. 20012
ANGUS, William N., Clinton Rd., Bedford Hills, N.Y. 10507
ANGWENYI, Charles B. P., 4 Sunset Ct., Amherst Mass. 01002
ANJARIA, Jashwantrai J., Int. Monetary Fund, Washington 25, D.C.
ANJARIA, Shailendra, 3934 Pine St., Philadelphia, Pa. 19104
ANKER, Wallace R., 1028 Valley Dr., Alexandria, Va. 22302
ANKLI, Robert, Univ. of Illinois, Dept. of Econ., Urbana, Ill.
ANN, Sister M. Carl, Dunbarton Col., Washington, D.C.
ANNABLE, James E., Jr., 2426 Graduate Col., Princeton, N.J. 08540
ANON, Norman S., 1566 Anita Pl., N.E., Atlanta, Ga. 30306
ANROD, Charles W., 180 Crain St., Evanston, Ill.
ANSCHEL, Kurt R., Univ. of Kentucky, Dept. of Agric. Econ., Lexington, Ky.
ANSHEN, Melvin, Grad. Sch. of Bus., Columbia Univ., New York 27, N.Y.
ANSPACH, Ralph, San Francisco State Col., Dept. of Econ., 1600 Holloway Ave., San Francisco 27, Calif.
ANTHONY, Donald E., Dept. of Bus. Admin., Kent State Univ., Kent, Ohio
ANTON, Frank R., Univ. of Alberta, Calgary, Alta., Canada
ANTON, H. R., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
AOKI, Torao, 6308 Marjory Lane, Bethesda, Md. 20034
AOYAMA, Hideo, Faculty of Econ., Kyoto Univ., Kyoto, Japan
APEDAILE, Leonard P., 207 East Hall, Iowa State Univ., Ames, Iowa 50012
APEL, Hans, c/o A. G. Meyer, 4812 Nakoma Dr., Okemos, Mich. 48864
APPELBAUM, Edward M., 53 Edgewood Ave., Albany, N.Y. 12203
APPLEBAUM, Herbert, 12 Western Lane, Wantagh, Long Island, N.Y.
APPLEBAUM, Leon, Ohio State Univ., Labor Educa. & Res. Serv., Columbus, Ohio 43210
APPLEYARD, Dennis R., 2505 Pittsfield Blvd., Ann Arbor, Mich. 48104
APRIGLIANO, Ludwig F., 574 Washington Blvd., Long Beach, N.Y.
APTON, Ralph J., 65 E. 96th St., New York, N.Y. 10028
AQUINO, Tranquilino B., 6537 Oakwood Dr., Falls Church, Va. 22041
ARANT, Willard D., 4028 Grand Ave., Western Springs, Ill. 60558
ARBEN, Philip D., 80 East Elm St., Chicago, Ill. 60611
ARCHER, Stephen H., Univ. of Wash., Col. of Bus. Admin., Dept. of Acctg., Fin., & Statis., Seattle, Wash. 98105
ARCINIEGAS-IREGUI, Guillermo, Calle 70 A # 11-93, Bogota, Colombia
ARDITI, Nessim, Cepal Ave. Providencia 871, Santiago, Chile
ARDITO-BARLETTA, Nicolas, Apartado 4339, Panama, Rep. de Panama
ARDLEY, Harry M., Pacific Tel. & Tel. Co., 140 New Montgomery St., Rm. 1321, San Franciso, Calif. 94105
ARDOLINI, Charles W., 3963 Warner Ave., Hyattsville, Md. 20784
AREHART, Jerald D., 5424 Vale Dr., Denver, Colo.
ARELLANES, Robert V., Peace Corps, U.S. Embassy, Lima, Peru
ARELLANO, Richard G., Florida State Univ., Sch. of Bus., Tallahassee, Fla. 32306
ARENA, (Mrs.) Carolyn, 3900 Tunlaw Rd., N.W., Washington, D.C. 20007

ARENA, John J., 122 Front St., Marblehead, Mass. 22312
ARESVIK, Oddvar, Norges Landbrukshögskole, Institutt for Driftslaere og Landbruksökonomi, Vollebekk, Norway
ARGENTINO, Vincent L., 1957 Mayflower Ave., Bronx, N.Y. 10461
ARIS, Leslie L., Univ. of Michigan, Grad. Sch. of Bus. Admin., Ann Arbor, Mich.
ARLT, Carl T., Univ. of Illinois, Col. of Com. & Bus. Admin., Dept. of Fin. Urbana, Ill. 61803
ARMENT, D. R., Massey-Ferguson Ltd., 200 University Ave., Toronto 1, Ont., Canada
ARMINGTON, Paul S., 3307 W. Coquelin Ter., Chevy Chase, Md. 20015
ARMOUR, Stuart D., 5 Lonsdale Rd., Apt. 2, Toronto, Ontario, Canada
ARMSTRONG, Donald E., 3 Westland Dr., Montreal 28, Que., Canada
ARMSTRONG, Frederick, Dept. of Econ., Wisconsin State Col., Eau Claire, Wis.
ARMSTRONG, George R., State Univ. Col. of Forestry, Syracuse Univ., Dept. of Forest Econ., Syracuse, N.Y. 13210
ARMSTRONG, Robert H. R., "Walton", 22, Wellesley Park, Wellington, Somerset, England
ARMSTRONG, Robert P., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
ARMSTRONG, Rodney E., American Consulate General, Fleet Post Office, San Francisco, Calif. 96661
ARNDT, Helmut H., Milinowskistrasse 5, 1 Berlin 37, Germany
ARNDT, Sven, Univ. of California, Dept. of Econ., Los Angeles 24, Calif.
ARNESON, Howard D., Univ. of Maryland, Dept. of Econ., College Park, Md. 20747
ARNOLD, Carl J., Box 26, Rollins College, Winter Park, Fla.
ARNOLD, Donald W., 651 Normal Rd., DeKalb, Ill. 60115
ARNOLD, Henry C. F., 171 Fordham Dr., Matawan, N.J. 07747
ARNOLD, Robert E., Jr., 24615 McDonald, Dearborn Heights, Mich. 48125
ARNOLD, Robert K., Stanford Research Inst., Menlo Park, Calif.
ARNOLD, Roslyn, 6800 Liberty Rd., Apt. 514, Baltimore, Md. 21207
ARNOLD, S. R., P.O. Box 11098, Oakland, Calif. 94611
ARNOLD, Victor L., Colorado State Univ., Dept. of Econ., Ft. Collins, Colo. 80521
ARNOLDS-PATRON, Paul, Florida Southern College, Box 278, Lakeland, Fla. 33802
ARNOULD, Richard J., 7465 Keystone Lane, Forestville, Md. 20028
ARNOW, (Mrs.) Kathryn S., 2836 Linden Lane, Falls Church, Va. 22042
ARNOW, Philip, 2836 Linden Lane, Falls Church, Va. 22042
AROLA, Aarno K., Embaixada da Finlandia, Ave. General Justo, 275, Apt. 902, Rio de Janeiro, Brazil
ARON, Gary, 6010 Virginia, Kansas City, Mo. 64110
ARONOW, Stephen A., 2401 H St., N.W., Washington, D.C. 20037
ARONSON, Jay R., Lehigh Univ., Dept. of Econ., Bethlehem, Pa. 18015
ARONSON, Robert L., New York State Sch. of Ind. and Labor Rela., Ithaca, N.Y.
ARRILL, Daniel, 4977 Battery Lane, Bethesda, Md. 20014
ARRINGTON, J. Neil, 6 Pierson Pl., Hopewell, N.J. 08525
ARRINGTON, Leonard J., Dept. of Econ., Utah State Univ., Logan, Utah 84321
ARROW, Kenneth J., Dept. of Econ., Stanford Univ., Stanford, Calif. 96128
ARROYO, Gonzalo, Casilla 10445, Santiago, Chile
ARSHAM, Arthur A., 149 Broadway, New York 6, N.Y.
ARTH, Maurice P., 438 Clifton St., Westfield, N.J.
ARTHUR, Henry B., Grad. Sch. of Bus. Admin., Harvard Univ., Soldiers Field, Cambridge 38, Mass.
ARTHUR, I. W., Econ. Dept., Iowa State Univ., Ames, Iowa
ARTLE, Roland, 1435 Hawthorne Ter., Berkeley 8, Calif.
ARUNACHALAM, R. M., 333 W. Stadium Ave., West Lafayette, Ind. 47906
ARZAC, Enrique R., 500 Riverside Dr., New York, N.Y. 10027
ASCH, Peter, Rutgers—The State Univ., Dept. of Econ., New Brunswick, N.J. 08903
ASCHER, Bernard, 811 Caddington Ave., Silver Spring, Md. 20901
ASCHER, Leonard, Univ. of Hawaii, Col. of Bus. Admin. 2500, Campus Dr., Honolulu, Hawaii 96822
ASHBROOK, Arthur G., Jr., 2925 39th St., N.W., Washington, D.C. 20016
ASHBY, David B., 3025 Alder St., Eugene, Ore. 97405
ASHBY, Lowell DeWitt, Regional Econ. Div., Office of Bus. Econ., U. S. Dept. of Commerce, Washington, D.C. 20230
ASHE, A. J., B. F. Goodrich Co., 500 S. Main St., Akron 18, Ohio
ASHENFELTER, Orley C., 271 Varsity Ave., Princeton, N.J. 08540
ASHER, Mukul G., Box 276, C.S., Pullman, Wash.
ASHER, Robert E., Brookings Inst., 1775 Massachusetts Ave., N.W., Washington, D.C. 20036
ASHLEY, John W., California State Col. at Hayward, Dept. of Econ., 25800 Hillary St., Hayward, Calif. 94542
ASHMEN, Roy, Col. of Bus. Admin., Univ. of Maryland, College Park, Md.
ASHOUR, Isam Y., American Univ. of Beirut, Beirut, Lebanon
ASHTON, David J., 22 Myrtle St., Winchester, Mass. 01890
ASHTON, Herbert, 5229 Elliott Rd., Washington 16, D.C.
ASIMAKOPULOS, A., Dept. of Econ and Polit. Sci., McGill Univ., Montreal, Canada
ASKWIG, William J., Southern Colorado State Col., Pueblo, Colo.
ASMUS, Karl H., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
ASOFSKY, Abraham A., 3980 Orloff Ave., Bronx 63, N.Y.
ASON, Elias R., 112 Wilson Way, Albany, Calif. 94710
ASPELIN, Arnold L., 3814 Jay Ave., Alexandria, Va.
ASPINWALL, Richard C., Columbia Univ., 208 Uris Hall, New York, N.Y. 10027
ASPIS, Daniel, Pratt Inst., Sch. of Humanities and Soc. Sci., Brooklyn, N.Y. 11205
ASSEILY, Robert, P.O. Box 4126, Beirut, Lebanon
ATEFAT, Bahram, 500 West End Ave., New York, N.Y. 10024
ATELJEVICH, Sava, 3374 Las Huertas, Lafayette, Calif. 94549
ATHEARN, James L., Univ. of Montana, Sch. of Bus. Admin., Missoula, Mont.
ATHERTON, Wallace N., Univ. of California, Dept. of Econ., Davis, Calif. 95616
ATKINS, Paul M., 199 Inwood Ave., Upper Montclair, N.J.
ATKINSON, Glen, Univ. of Oklahoma, Box 413 Faculty Exchange, Norman, Okla.
ATKINSON, L. Jay, 3709 Tollgate Ter., Falls Church, Va. 22041
ATKINSON, Sterling K., Temple Univ., Philadelphia, Pa.
ATKINSON, Sue N., 29 Edgemarth Hill Rd., Westport, Conn.
ATKINSON, Thomas R., 29 Edgemarth Hill Rd., Westport, Conn.
ATKINSON, (Mrs.) Wilellyn M., 7610 Tollgate Ter., Falls Church, Va. 22041
ATLAS, (Mrs.) Liane W., 2254 48th St., N.W., Washington 7, D.C.
ATLAS, Martin, 2254 48th St., N.W., Washington 7, D.C.
ATLEE, John S., 432 Garden City Dr., Monroeville, Pa. 18832
ATTAR, Abdul-Wahab, 440 S. Oxford St., Los Angeles, Calif.
ATTERBERRY, Phil R., 11412 Luxmanor Rd., Rockville, Md. 20852
ATTIGA, Ali A., Ministry of Plan. & Dev., Tripoli, Libya
ATWATER, Thomas V., Jr., 89 Main St., Concord, Mass. 01742

ATWELL, Frank P., Bell Tel. Co. of Pennsylvania, 1 Parkway, Philadelphia 2, Pa.
ATWOOD, Edward C., Jr., 203 W. Preston St., Lexington, Va. 24450
AUBLE, Arthur G., Box 600, Tripoli, Libya
AUBREY, Henry G., 560 Riverside Dr., Apt. 14J, New York, N.Y. 10027
AUCHTER, Edmund L., 4972 E. Leisure, Fresno, Calif. 93702
AUDET, Reynald, 815 ave. de Bienville, Quebec 6, Que., Canada
AUER, Felix L., 9 Delbarton Dr., Madison, N.J. 07940
AUERBACH, Irving M., 3 Sheridan Ave., West Orange, N.J.
AUERBACH, (Mrs.) Kay J., USOM-AD/PE, APO San Francisco 96301
AUERBACH, (Mrs.) Norma L., 1960 Williamsbridge Rd., Bronx 61, N.Y.
AUERBACH, Paul R., 67-40 Parsons Blvd., Flushing, N.Y. 11365
AUERBACH, Robert D., 6930 South Shore Drive, Chicago, Ill. 60649
AUGUST, James D., 2009 N. Daniel St. #103, Arlington, Va. 22201
AUGUSTA, Joseph H., 430 M St., S.W., N-100, Washington, D.C. 20024
AULT, Bertram, 2717 Shore Dr., Merrick, N.Y. 11566
AURAY, Robert R., 56 W. Saddle River Rd., Waldwick, N.J. 07463
AUSTIN, Douglas V., Fed. Res. Bank of Cleveland, Res. Dept., Box 6387, Cleveland, Ohio 44101
AUSTIN, Ellis T., Fresno State Col., Fresno, Calif. 93726
AUSTIN, John S., 1611 Adams St., Madison, Wis.
AUSTIN, Philip E., Casselton, N.D. 58012
AUSTIN, Ruben V., Dept. of Econ. and Bus., Univ. of Delaware, Newark, Del. 19711
AUSTIN, Russel, Lord, Abbett and Co., 63 Wall St., New York 5, N.Y.
AUTEN, Gerald E., 922 N. Altadena, Royal Oak, Mich. 48067
AUTEN, John H., 3603 N. Abingdon St., Arlington, Va. 22207
AUXIER, C. Carson, 2204 Forest Glen Rd., Silver Spring, Md. 20910
AVELLA, William R., 674 Town Line Rd., Hauppauge, L.I., N.Y. 13076
AVERITT, George R., 341 Hickory St., Michigan City, Ind. 46360
AVERITT, Robert T., 108 South St., Northampton, Mass. 01060
AVINGER, Robert L., Jr., 1815 Forest Rd., Durham, N.C.
AWAD, Elias M., 1309 Maple Ave., Evanston, Ill. 60201
AWADALLA, Salib G., 14, Nessim Girgawy St., Old Cairo, Cairo, Egypt, U.A.R.
AWAR, Sami A., 11122 Montana Ave., Los Angeles 14, Calif.
AWH, Yeen Bock, Dept. of Com., Nicholls State Col., Thibodaux, La 70301
AXELSON, Ivar, 3590 Crystal View Ct., Miami, Fla. 33133
AXFORD, Herbert M., 1719 Heron Rd., Ottawa 8, Ont., Canada
AXILROD, Eric, 2401 Pennsylvania Ave., Apt. 15-B-36, Philadelphia 30, Pa.
AXINN, June M., 722 Wyndale Rd., Jenkintown, Pa. 19046
AYAL, Eliezer B., USAID/PO/NPA, APO San Francisco 96528
AYARS, Albert L., Sch. Dist. #81-Admin. Bldg., 825 W. Trent Ave., Spokane, Wash. 99201
AYDINOGLU, Ismail H., Min. of Finance, Ankara, Turkey
AYRE, Josephine, 5217 Reno Rd., N.W., Washington, D.C. 20015
AYRE, Michael B., 221 W. 13th St., New York, N.Y. 10011
AYRE, Peter I., "Lavender", Longbury Hill, Nr. Storrington, Sussex, England
AYRES, Clarence E., Univ. of Texas, Austin, Tex.
AYERS, Harold E., Tennessee Poly. Inst., Box 42A, Cookeville, Tenn. 38501
AYRES, Ruth W., Pennsylvania State Univ., 120 Home Econ. South, University Park, Pa. 17774
AZAD, Nematollah, Room 309, Econ. Res. Dept., Central Bank of Iran, Teheran, Iran
AZAR, Wasef Y., Nat. Income Sec., Dept. of Statis., Amman, Jordan
AZEVEDO, Ross E., Cornell Univ., New York State Sch. of Ind. and Labor Rel., Box 17, Ithaca, N.Y. 14850
AZHARI, Mr., Nat. Inst. of Admin., Consultation Section, Djalan Segara 10, Djakarta, Indonesia
AZHARI, Abdul, American Oil Company, Mktg. Res. Dept., P.O. Box 6110-A, Chicago, Ill. 60680

BABA, Koichi, Econ. Planning Agency, Kasumigaseki Chiyoda Ku, Tokyo, Japan
BABCOCK, Guilford C., 494 Cragmont Ave., Berkeley 8, Calif.
BABIAN, Haig, Inst. of Econ. Affairs, New York Univ., 475 Fifth Ave., New York 17, N.Y.
BABILOT, George, San Diego State Col., San Diego, Calif.
BABKA, Kenneth D., 777 Cleveland Dr., Apt. 1, Buffalo, N.Y. 14225
BABSON, Harold C., Jr., 625 Chestnut St., Manchester, N.H. 03101
BABSON, Roger W., Babson's Reports, Inc., Washington St., Weslesley Hills, Mass. 02181
BABURAM, Alfred R., 8 Belvoir Ct., Bel Air, E.C.D., British Guiana
BABYLON, John R., 227 C St., S.E., Washington, D.C. 20003
BACH, Clarence D., 3109 Garbett Ave., McKeesport, Pa.
BACH, George L., Grad. Sch. Ind. of Tech., Pittsburgh 13, Pa.
BACHELOR, Robert W., 89 A Shadow Lane, West Hartford, Conn. 06110
BACHMAN, Kenneth L., USDA, Econ. Res. Serv., For. Dev. and Trade Div., Washington, D.C. 20250
BACHMURA, Frank T., Indiana Univ., Dept. of Econ., Bloomington, Indiana
BACK, Nathaniel A., 5200 N. 30th St., Arlington, Va.
BACK, William B., Res. Dev. Econ. Div., Econ Res. Service, USDA, Washington, D.C. 20250
BACKES, Edward J., 9 Kaine Ter., Albany, N.Y. 12208
BACKMAN, Jules, 59 Crane Rd., Scarsdale, N.Y.
BACON, Dorothy C., Smith Col., 115 Elm St., Northampton, Mass.
BACON, Marvin A., 4400 Leland, Chevy Chase 15, Md. 21620
BADGER, Ralph E., Box 608, East Orleans, Mass. 02643
BADGLEY, L. Durward, 40 Knollwood Rd, Short Hills, N.J. 07078
BADRE, Albert Y., 725 S. Summit St., Iowa City, Iowa
BAER, Werner, Vanderbilt Univ., Dept. of Econ., Nashville, Tenn.
BAER, Wilbur, 2148 Wightman St., Pittsburgh, Pa. 15217
BAERNCOPF, David A., Univ. of Oregon, Sch. of Bus. Admin., Eugene, Ore. 97403
BAERRESEN, Donald W., 215 West Cristobal, San Clemente, Calif. 92672
BAERWALD, F., Grad. Sch. of Arts and Sci., Fordham Univ., New York 58, N.Y.
BAEZ, Ximena, 9 de Octubre #1172, Quito, Ecuador
BAFF, Stanley, T-112-E Northington, Tuscaloosa, Ala. 35404
BAGAMERY, Frank, Bluefield State Col., Div. of Bus., Bluefield, W.Va. 24701
BAGGERLY, Earl W., Jr., Norris and Hirshberg, Inc., 1550 Bank of Georgia Bldg., Atlanta, Ga. 30303
BAGGOTT, Nancy L., 515 N. Oakland Ave., Carbondale, Ill. 62901
BAGLEY, Edgar S., Kansas State Univ., Econ. Dept., 346 Waters Hall, Manhattan, Kans.
BAGLEY, William C., Jr., Univ. of Puerto Rico, College of Soc. Sci., Rio Piedras, P.R. 00745
BAHL, Roy W., Jr., West Virginia Univ., Col. of Com., Morgantown, W.Va. 26506
BAHN, Ralph, 151 Fleetwood Ter., Silver Spring, Md.
BAHN, Samuel H., 18 E. 95th St., New York, N.Y. 10028

BAHR, Richard C., Box 1131, Station B, Nashville, Tenn.
BAHRAL, Uri, 33 Ramban Road, Jerusalem, Israel
BAIER, Martin, 5206 W. 98th Ter., Shawnee Mission, Kans. 66207
BAILER, Lloyd H., 112-20 178th Pl., Jamaica, N.Y. 11433
BAILEY, Earl N., Univ. of Texas, Bus. and Econ. Bldg., Rm. 102, Austin, Tex.
BAILEY, Martin J., Inst. of Defense Analyses, 400 Army-Navy Dr., Arlington, Va. 22202
BAILEY, Richard M., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif.
BAILEY, Robert S., Westminster Col., New Wilmington, Pa. 16142
BAILEY, Robert W., 2746 Lawndale Ave., Evanston, Ill.
BAILEY, Victor B., Box 88, Wrightsville Beach, N.C. 28480
BAILEY, William M., Univ. of Maryland, Dept. of Econ., College Park, Md.
BAILEY, William R., 3023 S. Columbus St., Arlington, Va. 22206
BAILY, Nathan A., 5516 Greystone St., Chevy Chase, Md. 20015
BAIN, George F., 1060 Cedar Grove Blvd., Oakville, Ont., Canada
BAIN, Joe S., 560 Woodmont Ave., Berkeley, Calif. 94708
BAIN, Trevor, Univ. of Arizona, Dept. of Econ., Tucson, Ariz. 85721
BAIR, J. Arnold, 143 E. Broad St., New Holland, Pa. 17557
BAIRD, William M., Wittenberg Univ., Springfield, Ohio
BAIREY, W. Zev, Apartado 10-756, Mexico 10, D.F., Mexico
BAIRSTOW, (Mrs.) Frances, McGill Univ., Ind. Rela. Centre, 1009 Sherbrooke St., W., Montreal, Que., Canada
BAKER, C. B., Dept. of Agric. Econ., Univ. of Illinois, Urbana, Ill.
BAKER, Charles C., Jr., 3243 N. Abingdon St., Arlington 7, Va.
BAKER, Elizabeth F., 601 W. 113 St., New York 25, N.Y.
BAKER, George P., Harvard Bus. Sch., 202 Morgan Hall, Soldiers Field, Boston, Mass. 02163
BAKER, Harold B., 1700 W. Third Ave., Flint, Mich. 48502
BAKER, James C., 11412 Allview Dr., Beltsville, Md. 20705
BAKER, Maurice E., Dept. of Ag. Economics, Rutgers Univ., College of Agric., New Brunswick, N.J.
BAKER, Miner H., Seattle First National Bank, P.O. Box 3586, Seattle 24, Wash.
BAKER, (Mrs.) Nancy H., 28 Delaware Ave., Dumont, N.J. 07628
BAKER, Olin D., c/o Midwest Inst. Bus. Admin., P.O. Box 190, Eureka, Kans. 67045
BAKER, Russell G., Putnam Green, Apt. 30-H, Greenwich, Conn.
BAKER, William H., Univ. of Tennessee, Dept. of Bus. Admin., Martin, Tenn. 38237
BAKERMAN, Theodore, Mercy Hospital, Pittsburgh, Pa. 15219
BAKKE, E. Wight, 2 Hillhouse Ave., New Haven, Conn.
BAKKEN, Henry H., Univ. of Wisconsin, Col. of Agric., Madison 6, Wis.
BAKONY, Abbie Jane, Box #1539, Chicago, Ill. 60690
BAKONY, Leo I., Econ. Council of Canada, P.O. Box 527, Station B., Ottawa, Ont., Canada
BALABANIS, G. P., 1926 Columbia Pike, Apt. 9, Arlington, Va. 22204
BALABANIS, Homer P., 45 California Ave., Arcata, Calif. 95521
BALABKINS, Nicholas, Lehigh Univ., Econ. Dept., Bethlehem, Pa.
BALANTRAPUS, Rao, 19 S. 37th St., Philadelphia, Pa. 19104
BALASSA, Bela A., Yale Univ., Dept. of Econ., 37 Hillhouse Ave., New Haven, Conn.
BALBACH, Anatol B., San Fernando Valley State Col., Div. of Bus. and Econ., Northridge, Calif.
BALBACH, Rachel B., Div. of Bus. and Econ., San Fernando Valley State Col., Northridge, Calif.
BALCEREK, John P., 2038 Cornell Rd., Cleveland, Ohio 44106
BALCO, George J., 20 Harold St., Cos Cob, Conn. 06807
BALDERSTON, C. Canby, "Penncrest," 45 Rosetree Rd., Media, Pa. 19063
BALDO, Frank V., John Carroll Univ., Sch. of Bus., Cleveland, Ohio 44118
BALDWIN, Armand-Jean, St. Vincent College, Latrobe, Pa. 15650
BALDWIN, Charles L., Econ. Consultants, Inc., 10203 Santa Monica Blvd., Los Angeles, Calif. 90067
BALDWIN, Claude D., 2034 48th St., N.W., Washington 7, D.C.
BALDWIN, Edward F., 995 W. Lucas, Beaumont, Texas 77706
BALDWIN, George B., 6609 Virginia View Ct., Washington 16, D.C.
BALDWIN, Robert E., Univ of Wisconsin, Dept. of Econ., Madison, Wis. 53706
BALDWIN, Stephen E., Univ. of Washington, Dept. of Econ., Seattle, Wash.
BALDWIN, William L., Dept. of Econ., Dartmouth College, Hanover, N.H. 03755
BALESTRA, Pietro, Route de Champriond 6, Fribourg, Switzerland
BALICKA, Sophya M., A.I.D., APO New York 09254
BALKIN, Steven M., 18435 Westhaven, Southfield, Mich. 48076
BALKO, George A., Jr., Clark Univ., Worcester 10, Mass.
BALL, David S., Vanderbilt Univ., Dept. of Econ., Nashville 5, Tenn.
BALL, (Mrs.) Irene O., 96 S. State Rd., Briarcliff Manor, N.Y. 10510
BALL, Robert B., Austin Col., Sherman, Tex. 75090
BALL, Robert L., 5803 26th Ave., S.E., Washington, D.C. 20031
BALLABON, Maurice B., 105-10 62nd Rd., Forest Hills, N.Y. 12068
BALLAINE, Wesley C., Univ. of Oregon, Sch. of Bus. Admin., Eugene, Ore. 97403
BALLANCE, Robert H., Univ. of Tennessee, Econ. Dept., Knoxville, Tenn.
BALLANTINE, John W., 82 Library Pl., Princeton, N.J. 08540
BALLANTINE, Thomas M., Penton Publishing Co., 1213 W. 3rd St., Cleveland 13, Ohio
BALLANTYNE, Arnold P., Dept. of Econ., USAF Academy, Colo.
BALLAS, Angelos C., 1145 Madison St., Annapolis, Md. 21403
BALLES, John J., Mellon National Bank and Trust, Mellon Sq., Pittsburgh 30, Pa.
BALLESTEROS, Marto A., 10112 Ashburton Lane, Bethesda, Md. 20034
BALLESTEROS, Paulino A., 1128 Exeter, Reading, Pa. 19604
BALLIET, Lee S., P.O. Box Q, Austin 12, Tex.
BALLINGER, Roy, Econ. Res. Serv., U.S. Dept. of Agric., Washington 25, D.C.
BALLOT, Michael H., 100-F Escondido Village, Stanford, Calif. 94305
BALLOU, George T., Standard Oil Co., of California, Econ. Dept., 225 Bush St., Rm. 705, San Francisco, Calif. 94120
BALOFF, Nicholas, Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
BALSLEY, Howard L., Texas Tech. Col., Dept. of Marketing, Lubbock, Tex. 79409
BALYS, Stanley K., 1509 Hamilton Ave., N.W., Grand Rapids, Mich.
BAME, Jack J., Rutgers Univ. Sch. of Bus., 26 James Street, Newark 2, N.J.
BAMFORD, Fred E., Dept. of Econ. and Soc., Clark University, Worcester 10, Mass.
BANCROFT, Everett C., Colgate Univ., Econ. Dept., 83 Hamilton St., Hamilton, N.Y. 13346
BANCROFT, Gertrude, 3615 O St., N.W., Washington 7, D.C.
BANDA, A. Frederic, Univ. of Hartford, Dept. of Econ., P.O. Box 1948, Hartford 1, Conn.
BANDALOUKAS, Claude B., 33a Kefallinias St., Athens 8, Greece
BANDEEN, R. A., Canadian Nat. Rwys., Res. Dept., P.O. Box 8100, Montreal, Que., Canada

BANDERA, Vladimir N., Boston Col., Dept. of Econ., Chestnut Hill, Mass. 02167
BANDOR, Frank, 524 W. 123rd St., Apt. 4-W, New York, N.Y. 10027
BANKS, Charles G., 1 Avenue Athol, Canterbury, E. 7, Victoria, Australia
BANKS, J. Eugene, 880 5th Ave., New York, N.Y. 10021
BANKS, Robert L., 900 17th St., N.W., Washing-N.Y. 10021
BANNER, Gilbert, Univ. of Tennessee, Col. of Bus. Admin., Dept. of Econ., Knoxville, Tenn.
BANNER, Paul H., 2909 Rittenhouse St., N.W., Washington, D.C. 20015
BANNINK, R., Horionstraat 24, Voerendaal (L.), The Netherlands
BANNON, William J., Jr., 518 West Ave., Pawtucket, R.I. 02860
BAQUERO, Jose A., Catholic Univ. of America, Sch. of Educa., International Education, Washington, D.C. 20017
BARAN, Evelyn, 13039 Sky Valley Rd., Los Angeles 49, Calif.
BARAN, Victor J., 305 Beverly Dr., Alexandria, Va.
BARANSON, Jack, World Bank, Econ. Div., 1818 H St., N.W., Washington, D.C. 20006
BARATH, Desire, Univ. of Detroit, 4001 W. McNichols, Detroit 21, Mich.
BARATZ, Morton S., Dept. of Econ., Bryn Mawr Col., Bryn Mawr, Pa. 19016
BARATZ, Rubin Z., 4601 Henry Hudson Pkwy., New York 71, N.Y.
BARBASH, Jack, 1836 Keyes Ave., Madison 5, Wis.
BARBER, Clarence L., Univ. of Manitoba, Dept. of Econ., Winnipeg, Man., Canada
BARBER, E. Lloyd, 8216 Fenway Rd., Bethesda, Md. 20034
BARBER, Mildred S., 2475 Virginia Ave., Potomac Plaza, Washington 7, D.C.
BARBER, Richard J., Senate Antitrust Subcomm., 412 Old Senate Office Bldg., Washington, D.C. 20510
BARBER, William J., Wesleyan Univ., John E. Andrus Center for Pub. Affairs, Dept. of Econ. and Soc. Sciences, Middletown, Conn.
BARBIER, Hans-D, Eichendorffstrasse 10, 6602 Dudweiler, Saar, Germany
BARBION, Paul E., 1640 32nd St., N.W., Washington, D.C. 20007
BARBOSA, Antonio P., Rua Almirante Antonio Pessanha 3, Bairro do Restelo, Lisbon 3, Portugal
BARBOUR, Edmund, 3801 W. Greenwood Pl., Denver, Colo. 80236
BARCHFIELD, J. W., Olympic Col., Dept. of Bus. and Econ., Bremerton, Washington
BARCKLEY, Robert E., 6884 50th St., San Diego, Calif. 92120
BARCUS, Norman, Michigan Employ. Security Comm., 7310 Woodward Ave., Detroit, Mich. 48202
BARDARO, John S., 119 Alhambra St., San Francisco, Calif. 94123
BARG, Benjamin, United Nations, Grand Central Station, P.O. Box 20, New York 17, N.Y.
BARGER, Harold, Dept. of Econ., Columbia Univ., New York 27, N.Y.
BARGER, James W., 1519 LaPorte Ave., Ft. Collins, Colo. 80521
BARGES, Alexander, 1452 S. Beverly Dr., Los Angeles 35, Calif.
BARISH, Norman N., 7 Carthage Rd., Scarsdale, N.Y.
BARKAI, H., The Hebrew Univ., Dept. of Econ., Jerusalem, Israel
BARKAN, Harold, 1048 Nostrand Ave., Brooklyn 25, N.Y.
BARKAS, Benjamin W., 7711 Revere St., Philadelphia, Pa. 19152
BARKER, C. Austin, 2 Hickory Dr., Rye, N.Y. 10580
BARKER, Carl, Associated Fund, Inc., 506 Olive St., St. Louis, Mo. 63101
BARKER, James P., State Univ. Col., Cortland, New York
BARKIN, David P., 1947 Yale Sta., New Haven, Conn. 06520
BARKIN, Solomon, American Embassy, OECD, Manpower, APO 777 New York 09777
BARLEV, Joshua, 23821 Condon, Oak Park, Mich. 48237
BARLOON, Marvin J., Dept. Bus. and Econ., Western Reserve Univ., Cleveland 6, Ohio
BARLOW, Mary L., 2048 Hillside Dr., Falls Church, Va. 22043
BARLOW, Robert F., Whittemore Sch. of Bus. and Econ., Univ. of New Hampshire, Durham, N.H. 03824
BARLOW, Robin, Dept. of Econ., Univ. of Michigan, Ann Arbor, Mich.
BARLOWE, Raleigh, 907 Southlawn, East Lansing, Mich.
BARMAN, Lalit K., Cooper Tire and Rubber Co., Data Processing Dept., Findlay, Ohio 45840
BARNA, R. Robert, 2742 Ewald Circle, Detroit, Mich. 48238
BARNARD, Billings D., 82 Fisher Pl., Carmel, Calif.
BARNARD, Jerald R., State Univ. of Iowa, Bur. of Bus. and Econ. Res., Iowa City, Iowa
BARNDS, William J., 1804 N. Harvard St., Arlington 1, Va.
BARNEA, Joseph, 71-11 Juno St., Forest Hills, N.Y. 11376
BARNES, Arthur N., 9116 N. Ewing, Skokie, Ill. 60203
BARNES, Horace R., 1518 Clearview Ave., Lancaster, Pa.
BARNES, Irston, 58 N. Branford Rd., Wallingford, Conn.
BARNES, James L., 5913 Otis Ave., Tampa, Fla. 33604
BARNES, Leo, 473 West End Ave., New York 24, N.Y.
BARNES, Robert R., 310 Meadow Lane, Lancaster, Pa.
BARNES, William F., Jr., 8330 Delcrest Dr., Apt. 3B, University City, Mo. 63124
BARNETT, Harold J., Washington Univ., Dept of Econ., 226 McMillan Hall, St. Louis, Mo. 63130
BARNETT, Irving, 402 Washington Ave., Chestertown, Md. 21620
BARNETT, John S., 270 Jay St., Brooklyn 1, N.Y.
BARNETT, (Mrs.) Martha V., 402 Washington Ave., Chestertown, Md. 21620
BARNETT, Robert A., 25691 Hendrie Blvd., Huntington Woods, Mich. 49748
BARNEY, Frederick W., 1004 Bauman, Royal Oak, Mich. 48073
BARNGROVER, Charles, Col. of Bus. Admin., Univ. of Cincinnati, Cincinnati 21, Ohio
BARNHART, (Mrs.) Cynthia-Ann, 54 S. State Rd., Briarcliff Manor, N.Y.
BARNUM, Howard N., II, 913 9th St., Bldg. 57, Albany, Calif. 94710
BARON, Denis G., United States Embassy, APO New York 09777
BARON, Harold, 7444 S. Euclid Ave., Chicago, Ill. 60649
BARON, Leonard A., 19-M Ridge Rd., Greenbelt, Md. 20770
BARON, Philip, 5 Monroe Pl., Brooklyn 1, N.Y.
BARONE, Sam, 8432 Atherton St., St. Louis 3, Mo.
BARR, Edward E., 428½ Clinton St., Brooklyn 1, N.Y.
BARR, James L., 200 Kingsley Ave., Palo Alto, Calif.
BARR, John R., 81 Ridge St., North Sydney, NSW, Australia
BARR, Robert J., Col. of Bus. Admin., Marquette Univ., Milwaukee 3, Wis.
BARRACLOUGH, Solon, Oficina Regional de La Fao para America Latina, Casilla 10095, Santiago, Chile
BARRAZA, Luciano, 102-L Eagle Heights, Madison, Wis.
BARRES, Stephen J., California State Col. at Fullerton, Div. of Bus. Admin. and Econ., Fullerton, Calif.
BARRETT, Charles F., 110 W. Winspear Ave., Buffalo 14, N.Y.
BARRETT, Harry L., Jr., Plain Rd., Wayland, Mass. 01778
BARRETT, Larry B., 702 S. Division, Ann Arbor, Mich.
BARRETT, Martin R., 180 West End Ave., New York, N.Y. 10023

BARRETT, Michael J., Univ. of Colorado, Sch. of Bus., 204 Bus. Annex, Boulder, Colo. 80302
BARRETT, (Mrs.) Nancy S., Star Route #2, Warrlinda Village, La Plata, Md. 20646
BARRETT, Thomas, 381 Princeton St., San Francisco, Calif. 94134
BARRIGER, John W., Missouri-Kansas-Texas RR Co., 420 Gimblin Rd., St. Louis, Mo. 63147
BARRINGTON, Laurence W. L., 130 Flagg St., Worcester, Mass. 06109
BARRO, Robert J., 210 Holden Green, Cambridge, Mass. 02138
BARRO, Stephen M., c/o The RAND Corp., 1700 Main St., Santa Monica, Calif.
BARRON, James C., Pennsylvania State Univ., Dept. of Agric. Econ., University Park, Pa. 16802
BARRY, D. Marshall, 1133 Oak Ave., Evanston, Ill. 60202
BARRY, Robert A., Col. of William and Mary, Williamsburg, Va. 23185
BARRY, Robert D., 3432 Connecticut Ave. N.W., Washington, D.C. 20008
BARSNESS, Richard W., Northwestern Univ., Sch. of Bus., Evanston, Ill. 60201
BARSS, Lawrence W., Assoc. for Inter. Res., 8 Eliot St., Cambridge, Mass. 02138
BARTEL, Richard D., 560 Riverside Dr., Apt. 8-A, New York, N.Y. 10027
BARTEL, Robert J., North Broadway, Upper Nyack, N.Y.
BARTELL, Ernest, Princeton Univ., Woodrow Wilson Sch., Princeton, N.J. 08540
BARTELL, H. Robert, Jr., U.S. Savings and Loan League, 221 N. LaSalle St., Chicago, Ill. 60601
BARTFELD, Charles I., 15 W. 84th St., New York, N.Y. 10024
BARTH, (Mrs.) Helen A., 3821 Newark Street, N.Y., Washington, D.C. 20016
BARTH, Michael C., 55 W. 82nd (4-A), New York, N.Y. 10024
BARTH, Peter S., 1824 Northwest Ct., A, Columbus, Ohio
BARTH, Richard C., c/o Univ. of Maryland, APO New York 09403
BARTHEL, John, Stetson Univ., DeLand, Fla.
BARTHOLET, James K., 508 Bancroft St., Santa Clara, Calif.
BARTHOLOMEW, Donald C., Univ. of Missouri, Dept. of Econ., 5100 Rockhill Rd., Kansas City, Mo.
BARTLETT, Hale C., Univ. of Maryland, Dept. of Bus. Admin., College Park, Md. 20740
BARTLETT, Roland W., Univ. of Illinois, 315 Mumford Hall, Urbana, Ill.
BARTOLOME, Virgilio, 1025 Michigan Ave., Washington 17, D.C.
BARTOLOTTO, Ben A., 5854 Beck Ave., North Hollywood, Calif. 91601
BARTON, Richard F., Univ. of Kansas, Sch. of Bus., 311 Summerfield Hall, Lawrence, Kans. 66045
BARTON, Sam, Box 6015 T.C. Station, Denton, Texas
BARTSCH, William H., Hamilton House, Flat 5, 81 Southampton Row, London W.C. 1, England
BARUCCI, Piere, Via Volterrana, 14, Galluzzo, Florence, Italy
BARZEL, Yoram, Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
BASCH, Antonin, 2 Tudor City Pl., Apt. 7-GS, New York, N.Y.
BASCOM, Wynne B., Babson Inst., Babson Park 57, Mass.
BASEVI, Giorgio, Via Acquarone 43, Genoa, Italy
BASI, Raghbir S., Dept. of Bus. Admin., Kent State Univ., Kent, Ohio
BASIL, Brother, St. Marys Col., Winona, Minn.
BASKIN, Carlisle W., Randolph-Macon Col., Ashland, Va. 23005
BASMANN, Robert L., Purdue Univ., Herman C. Krannert Grad. Sch. of Ind. Admin., Lafayette, Ind. 47907
BASS, Elizabeth, 257 W. 11th St., New York 14, N.Y.
BASS, Frank M., Sch. of Ind. Mgt., Purdue Univ., Lafayette, Ind.
BASS, Henry L., 235 W. 15th St., Apt. 2C, New York 11, New York
BASS, James, 812 W. Washington, Ann Arbor, Mich. 48103
BASSETT, Harold S., System Sciences Corp., 5718 Columbia Pike, Falls Church, Va. 22041
BASSETT, Lowell R., 2204 Pueblo Dr., Lafayette, Ind.
BASSETT, Marjorie, Int. Encyclopedia of Science, 640 Fifth Ave., New York 19, N.Y.
BASSIE, V. Lewis, 708 La Sell Dr., Champaign, Ill.
BASSO, Alexander J., 3332 Curtis Dr., #202, Hillcrest Heights, Md. 20023
BASTI, Abdul Z., Univ. of Colorado, Dept. of Econ., Boulder, Colo. 80304
BAT, Bui Cong, Nat. Bank of Vietnam, 17, Ben Chuong Duong, Saigon, Vietnam
BATCHELDER, Alan B., Box 127, Gambier, Ohio 43022
BATCHELDER, Ronald W., 630 Martinelli St., Watsonville, Calif.
BATCHELOR, Hugh, Bd. of Broadcast Governors, Dept. of Econ. Res., 48 Rideau St., Ottawa 2, Ont., Canada
BATCHELOR, Joseph A., Dept. of Econ., Indiana Univ., Bloomington, Ind.
BATEMAN, Fred, Jr., Indiana Univ. Grad. Sch. of Bus., Bus. Econ. and Public Policy, Bloomington, Ind. 47405
BATES, Clyde T., Georgetown Col., Georgetown, Ky. 40324
BATES, Rex J., 415 Signal Hill Rd., Barrington, Ill. 60010
BATIE, Clarence M., Box 2044, Prairie View, Texas 78661
BATOR, Francis M., The White House, Room 361, Exec. Office Bldg., Washington, D.C. 20506
BATTIS, Robert A., Dept. of Econ., Trinity Col., Hartford 6, Conn.
BATTS, (Mrs.) Nathalie C., 20-15 24th St., Apt. 2B, Astoria, L.I., N.Y. 11105
BAUDE, Walter, Col. of Bus. Admin., Univ. of Cincinnati, Cincinnati 21, Ohio
BAUDER, Russell, 1108 S. Glenwood, Columbia, Mo.
BAUDUY, Guy, 80 First Ave., #14-G, New York, N.Y. 10009
BAUER, Alan, 501 E. 87th St., New York, N.Y. 10028
BAUER, Bertrand N., 5300 South Shore Dr., Chicago, Ill. 60615
BAUER, Larry L., 104 Chamerlain, Raleigh, N.C. 27607
BAUER, Milton F., Dept. of Pol. Econ., Univ. of Alberta, Edmonton, Alberta, Canada
BAUGE, Kenneth L., Michigan State Univ., Dept. of Soc. Science, East Lansing, Mich.
BAUGH, Russell, Dept. of Econ., Oklahoma State Univ., Stillwater, Okla.
BAUGHMAN, Ernest, Fed. Res. Bank, Res. Dept., P.O. Box 834, Chicago, Ill. 60690
BAUM, Milton S., 711 Santa Rita Way, Sacramento 25, Calif.
BAUME, Alfred H., Ford Motor Co., Finance Staff, The American Rd., Dearborn, Mich.
BAUMGART, Paul A., Econ. Res. Dept., Safeway Stores, Inc., P.O. Box 660, Oakland, Calif. 94604
BAUMGARTNER, H. W., 1442 Hanshaw Rd., Ithaca, N.Y. 14850
BAUMGARTNER, Mrs. H. W., 1442 Hanshaw Rd., Ithaca, N.Y. 14850
BAUMOL, William, Princeton Univ., 227 Dickinson Hall, Princeton, N.J. 08540
BAUSCH, Thomas A., c/o J. Alvert Bausch, 61 S. Main St., Batavia, N.Y.
BAWDEN, D. Lee, Univ. of Wisconsin, Dept. of Agric. Econ., Madison 6, Wis.
BAXLEY, William H., 811 Leonie Circle, Jacksonville, Fla. 32211
BAXTER, Nevins D., Univ. of Pennsylvania, Wharton Sch., Dept. of Fin., Philadelphia, Pa. 19104
BAY, John W., Univ. of Maine, Dept. of Econ., Portland, Me.
BAYARD, Franck, 1815 Varnum St., N.W., Washington, D.C. 20011
BAYER, Arthur A., 2705 E. Grand River Ave., Lansing, Mich.
BAYLOR, Aly A., 3144 Wheeling Ave., Manhattan Heights, El Paso, Texas
BAYO, Luis, a/c Manuel Tamez D., S.A., Nino Perdido No. 395, Mexico 12, D. F., Mexico

BAZAN, Henry J., 81 Raylo St., Chicopee, Mass. 01013
BEACH, Allen E., 3032 Stephenson Pl., N.W., Washington, D.C. 20015
BEACH, Earl F., McGill Univ., Stephen Leacock Bldg., Montreal, 2, Que., Canada
BEADLES, Nicholas A., Dept. of Econ., Univ. of Georgia, Athens, Ga.
BEADLES, William T., Illinois Wesleyan Univ., Dept. of Econ., Bloomington, Ill. 61701
BEAL, Williams, 2308 19th St., N.W., Washington, D.C. 09676
BEALES, Alan K., 551 Jean St., Oakland, Calif. 94610
BEALL, John Wood, Route 2, Endicott, N.Y.
BEALS, Ralph E., Northwestern University, Dept. of Econ., Evanston, Illinois
BEAN, Louis H., 3714 N. Randolph St., Arlington, Va. 22207
BEAN, Richard N., 3731 University Way, Apt. 101, Seattle, Wash. 98105
BEAR, Donald V., Univ. of California, San Diego, La Jolla, Calif. 92038
BEARD, Thomas R., Louisiana State Univ., Col. of Bus. Admin., Econ. Dept., Baton Rouge, La.
BEARD, Winston C., Baylor Univ., Hankamer Sch. of Bus., Waco, Texas 76706
BEARE, John B., Mount Allison Univ., Dept. of Econ., Sackville, N.B., Canada
BEASLEY, Kenneth W., 217 S. Lincoln 7, Bloomington, Ind. 47401
BEATON, Hugh C., 2439 O'Farrell St., San Francisco, Calif. 94115
BEATTIE, D. L., 1012 Connaught Ave., Moose Jaw, Sask., Canada
BEATTY, Sister M. Alexine, Aquinas School for Women, 303 Adams St., Milton, Mass. 01529
BEAUMONT, (Mrs.) Marion S., 561 Baughman Dr., Claremont, Calif. 91711
BEAUMONT, Richard A., 420 E. 85th St., New York 28, N.Y.
BEAVES, Peter D., Dept. of Econ., Loras Col., Dubuque, Iowa
BEAZER, William F., Inst. for Defense Analyses, 400 Army-Navy Dr., Arlington, Va. 22202
BEBERFALL, Elmer H., 67-25 161st St., Flushing 65, N.Y.
BECATTINI, Giacomo, Seminario Economia Politica, Facolta di Economia e Commercio, Via Curtatone 1, Florence, Italy
BECHDOLT, Burley V., Jr., 3300 Balzac St., Alhambra, Calif. 91803
BECHTOL, Paul T., Jr., Colorado Col., Colorado Springs, Colo.
BECK, James W., Univ. of California, Los Angeles, WDPC/IBM, Los Angeles, Calif. 90024
BECK, Morris, 90 W. Cedar St., Livingston, N.J. 07039
BECK, Robert G., Univ. of Alberta, Dept. of Econ., Edmonton, Alta., Canada
BECK, Robert J., 2858 S. 95th St., West Allis, Wis. 53227
BECKER, A. P., Univ. of Wisconsin-Milwaukee, Milwaukee, Wis. 53211
BECKER, Abraham, 305 W. 28th St., New York, N.Y. 10001
BECKER, Abraham S., c/o RAND Corp., 1700 Main St., Santa Monica, Calif.
BECKER, Charles M., 4214 Chatham Circle, Apt. 4, Las Vegas, Nev. 89109
BECKER, Donald P., 7334 Oak Park Ave., Van Nuys, Calif. 91406
BECKER, (Mrs.) Eugenia M., 2207 Prince St., Berkeley, Calif. 94705
BECKER, Gary S., 127 Brambach Rd., Scarsdale, N.Y.
BECKER, Joseph M., Saint Louis Univ., 221 N. Grand Blvd., St. Louis, Mo. 63103
BECKER, Nathan M., 44 Overlook Ave., West Orange, N.J. 07052
BECKER, W. G., Col. of Bus. Admin., Arizona State Univ., Tempe, Ariz.
BECKETT, Grace, P.O. Box 386, Urbana, Ill. 61801
BECKETT, W. Allan, 663 Yonge St., Toronto, Ont., Canada
BECKHART, Benjamin H., 24 Thelberg Rd., Poughkeepsie, N.Y. 12601
BECKLER, Bernard S., 8413 Victory Lane, Potomac, Md.
BECKLIN, Jay R., Indiana Univ., Bus. Econ. and Public Policy, Bloomington, Ind. 47405
BECKMAN, H. David, P.O. Box 4521, Tech. Station, Lubbock, Texas 79406
BECKMAN, Theodore, Col. of Com. and Admin., Ohio State Univ., Columbus, Ohio
BECKMANN, Martin J., Brown Univ., Dept. of Econ., Providence, R.I. 02912
BECKSTEAD, Robert W., P.O. Box 1353, University Park, N.M. 88070
BECKWITH, Burnham P., 403 Heliotrope St., Corona del Mar, Calif. 92625
BEDARD, John R., St. Mary's Col., Powerhouse Apts., Winona, Minn. 55987
BEDNARIK, Mojmir K., 30 Springdale Ave., White Plains, N.Y.
BEDROSIAN, Sarah G., 3518 E. Lowe Ave., Fresno, Calif. 93702
BEDSWORTH, William E., 4403 E. 5th Ave., Anchorage, Alaska 99504
BEEBE, Douglas T., Purdue Univ., Dept. of Econ., West Lafayette, Ind. 47907
BEECH, George R., 26 S. Belmont Circle, Oneonta, N.Y. 13820
BEECROFT, Eric, Univ. of Western Ontario, Middlesex Col., London, Ont., Canada
BEEMAN, William J., New Slocum Heights, Bldg. B-28, Apt. 5, Syracuse, N.Y. 13210
BEESON, Glen, 312 Hay St., Pittsburgh, Pa.
BEETHAM, Robert C., 136 Willow St., Brooklyn 1, N.Y.
BEGERT, William J., Jr., 5502 Carousel Circle, Garden Grove, Calif. 92641
BEHLING, Burton N., 5031 Glenbrook Rd. N.W., Washington 16, D.C.
BEHLOW, Robert R., 3221 Cathedral Ave., N.W., Washington, D.C. 20008
BEHMAN, (Mrs.) Sara, 964 Tulare Ave., Berkeley, Calif. 94707
BEHR, Michael, Univ. of Minnesota, Dept. of Econ., Duluth, Minn. 55812
BEHRENS, (Mrs.) Anna J. W., 8600 Lemon Ave. Apt. 9, La Mesa, Calif. 92041
BEHRENS, Carl F., 8600 Lemon Ave. Apt. 9, La Mesa, Calif. 92041
BEHRENS, Henry W., 144 7th St., Cresskill, N.J. 07626
BEHRMAN, Jere R., Univ. of Pennsylvania, Wharton Sch., Dept. of Econ., Philadelphia, Pa. 10194
BEIN, Donald H., A. O. Smith Corp., Data Systems Div., P.O. Box 584, Milwaukee, Wis 53201
BEINHAUER, Myrtle, Western Michigan Univ., Kalamazoo, Mich.
BEISHLINE, J. R., Alpine Rd., Greenwich Conn.
BEKKER, Konrad, American Embassy, APO, San Francisco 96346
BELANGER, Gerard, 715 Choquette, St. Hyacinthe, Que., Canada
BELANGER, M., 376 Est Du Roi, Quebec, Canada
BELCHER, Algernon S., South Carolina State Col., Orangeburg, S.C. 29115
BELCHER, David W., San Diego State Col., Div. of Bus. Admin., San Diego, Calif. 92115
BELCHER, Donald R., 550 Prospect St., Westfield, N.J.
BELCHER, Marjorie S., 333 E. 41st Street, New York, N.Y. 10017
BELCHER, Owen D., 969 Kenny St., Mobile, Ala. 36606
BELEKY, Louis P., 32-05 75th St., Jackson Hgts., Queens, N.Y. 11372
BELITSKY, A. Harvey, W. E. Upjohn Inst. for Employ. Res., 1101 17th St., N.W., Washington, D.C. 20036
BELL, Albert L., Dept. of Commerce, Franklin and Marshall Col., Lancaster, Pa.
BELL, Bernard R., Surveys and Res. Corp., 1010 Vermont Ave., N.W., Washington 5, D.C.
BELL, (Mrs.) Carolyn S., Wellesley Col., Wellesley, Mass. 02181
BELL, David E., 5801 Bent Branch Rd., Washington 16, D.C.
BELL, Duran, Jr., Univ. of California, Div. of Soc. Sci., Irvine, Calif. 92650
BELL, Frederick W., Federal Res. Bank of Boston, Res. Dept., Boston, Mass.
BELL, Harry H., UNCTAD, Palais des Nations, Geneva, Switzerland

BELL, James W., 1745 Chicago Ave., Evanston, Ill.
BELL, John F., 605 West Delaware Ave., Urbana, Ill.
BELL, Laurence J., P.O. Box 60, Bala-Cynwyd, Pa. 19004
BELL, Lewis C., Western Kentucky State Col., Tax Res. Center, Bowling Green, Ky. 42102
BELL, Philip W., Dept. of Econ., Haverford Col., Haverford, Pa. 19041
BELL, Richard M., 236 Baltic St., Brooklyn 1, N.Y.
BELL, Roger M., 3950 S. Pine Center, Orchard Lake, Mich. 49953
BELL, Spurgeon, 210 E. 30th St., New York 16, N.Y.
BELL, William J., 337 Linden Ave., Johnstown, Pa.
BELLA, Salvatore J., Col. of Com., Notre Dame Univ., Notre Dame, Ind. 46556
BELLEI, Danilo, Via Ridolfi 50, Modena, Italy
BELLEMORE, Douglas H., 152 Mountain Ave., Summit, N.J. 07901
BELLER, Andrea H., 357 E. 201st St., New York, N.Y. 10458
BELLER, Irv, AFL-CIO, 815 16th St., Washington 6, D.C.
BELLI, Pedro, 2632 Etna St., Berkeley 4, Calif.
BELLICO, Russell, 9 Willard Ave., Thompsonville, Conn.
BELLINGER, Patricia, 827 N. Waller Ave., Chicago, Ill. 60651
BELLSTEDT, Christoph J., Kettelerstr. 33, 611 Dieburg, Hessen, Germany
BELMONT, William R., Central Bank of Nigeria, P.M.B. 12194, Lagos, Nigeria
BELOTTE, Charles A., 2705 15th St., Troy, N.Y. 12180
BELOTTI, Lorenzo M., Dept. of Econ., Univ. of Santa Clara, Santa Clara, Calif.
BELOVICZ, Meyer W., 143-12 Halsey Dr., West Lafayette, Ind.
BELOZERCOVSKY, Norberto, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
BELSER, Joseph, 3262 Corinth Ave., Los Angeles 66, Calif.
BELSHAW, Michael, Box 268A RFD 1, New Milford, Conn. 06776
BELSLEY, David A., 22 W. Lyme Rd., Hanover, N.H. 03755
BELTH, Joseph M., 4131 Arlington Rd., Bloomington, Ind. 47401
BELTRAN, Pedro G., Apartado 485, Lima, Peru
BENABDI, Mohamed L., 11 Rue Audre Chenier, (N.D.D. Afrique), Alger, Algeria
BENATAR, Lewis O., 46 Edgewood Rd., Hartsdale, N.Y. 10530
BENAVIE, Arthur, San Francisco State Col., Dept. of Econ., 1650 Holloway Ave., San Francisco, Calif. 94132
BENDAVID, Avrom, 5716 Ager Rd., Apt. 2, West Hyattsville, Md. 20782
BEN-DAVID, Moshe, Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich.
BENDER, Edward J., III, 2302 Salutaris, Cincinnati, Ohio 45206
BENDER, Filmore E., Univ. of Maryland, Dept. of Agric. Econ., College Park, Md.
BENEDICT, Murray R., Giannini Found., Univ. of California, Berkeley, Calif.
BENEGAS, Hector, Economics Research Center AID, 1126 E. 59th St., Chicago, Ill. 60637
BENEWITZ, Maurice C., 261 Thompson Shore Rd., Manhasset, L.I., N.Y.
BENHAM, Lee, Apt. 80-B, Escondido Village, Stanford, Calif.
BENISHAY, Haskel, State Univ. of New York at Buffalo, Bus. and Econ., Main Street, Buffalo, N.Y.
BENJAMIN, Arthur, 5446 Old Court Rd., Randallstown, Md. 21133
BENJAMIN, Charles H. W., Univ. of New Hampshire, Whittemore Sch. of Bus. and Econ., Morill Hall, Durham, N.H.
BENJAMIN, Harold, 245 Rockingstone Ave., Larchmont, N.Y. 10538
BENJAMIN, Ruth, 1330 Massachusetts Ave., N.W., Washington, D.C. 20005
BEN-MOSHE, Jacob, 112 Pomona Ave., Newark, N.J.
BENNETT, Charles A., 48-51 43rd St., Woodside, N.Y. 11377
BENNETT, Howard, 2526 Laurel Lane, Wilmette, Ill. 60091
BENNETT, Jack F., 141 Taconic Road, Greenwich, Conn. 06833
BENNETT, James P., 3701 16th St., N.W., Apt. 209, Washington, D.C.
BENNETT, John D., Col. of Forestry, Dept. of Forestry Econ., Syracuse, N.Y.
BENNETT, John T., 3801 Towanda Rd., Alexandria, Va. 22303
BENNETT, Paul A., R.D. 3, Box 1180, Harrisburg, Pa. 17112
BENNETT, Philip A., 150 N. Day St., Orange, N.J.
BENNETT, Ralph J., Aquinas Col., Dept. of Econ., 1607 Robinson Rd., S.E., Grand Rapids, Mich. 49506
BENNETT, Richard E., Ohio State Univ., 1775 S. College Rd., Columbus, Ohio 43210
BENNETT, Robert L., Univ. of Maryland, Dept. of Econ., College Park, Md. 20742
BENNETT, Rollin F., Lambs Lane, Cresskill, N.J. 07626
BENNETT, Virgil E., Bennett and Co., 200 Beaver St., Beaver, Pa. 15009
BENNETT, Walter H., Research Analysis Corp., Econ. and Costing Div., McLean, Va. 22101
BENNETT, William B., 3318 Rose Lane, Falls Church, Va. 22042
BENNETT, William B., Union College, Dept. of Econ., Schenectady, N.Y. 12308
BENNETT, William R., P.O. Box 1896, University, Ala. 35486
BENNINGTON, William, Delaware River Port Authority, Div. of Port Development, P.O. Box 1949, Camden, N.J. 08101
BENO, Jan, 831 Fairview Ave., Washington, D.C. 20012
BENOIT, Emile, 39 Claremont Ave., New York 27, N.Y.
BENOIT, (Mrs.) Etta F., 39 Claremont Ave., New York 27, N.Y.
BEN-PORATH, Yoram, 372 Beacon St., Somerville, Mass.
BENSON, Charles S., 147 Arlington Ave., Berkeley, Calif. 94707
BENSON, George C. S., 300 E. 9th St., Claremont, Calif.
BENSON, John N., 204 King St. E., Apt. 2, Kingston, Ont., Canada
BENSON, Purnell, 21 Maple Ave., Madison, N.J.
BENSTON, George J., 5705 S. Maryland Ave., Chicago, Ill. 60637
BENTES, Joao F., Gustavo Sampaio 126/102, Rio de Janeiro, Brazil
BENTON, Don, Ridge Road, Box 355, Telford, Pa. 18969
BENZ, George, Central State Col., Edmond, Okla. 73034
BENZO, Jose M., Stratton Rd., Stratton Apts. # C-4, Williamstown, Mass. 01267
BERANEK, William, Pennsylvania State Univ., Col. of Bus. Admin., University Park, Pa. 16802
BERCHEM, Rutger H. E., Schulstrasse 33, 5101 Vaalserquartier Uber Aschen, Germany
BEREND, Karl H. B., 5205 Mt. Vernon Hgwy., Mt. Vernon Ter., Alexandria, Va. 22309
BERETVAS, Andor, 6832 S. Paxton Ave., Chicago, Ill. 60649
BERG, Norman A., Harvard Bus. Sch., Boston 63, Mass.
BERG, Sanford V., 10215 37th Pl., S.W., Seattle, Wash. 98146
BERG, Warren G., Luther College, Dept. of Econ. and Bus. Admin., Decorah, Iowa 52101
BERGE, B. Stanley, 2433 Simpson St., Evanston, Illinois
BERGER, Arnold, Case-W.R.U. Study, 2009 Adelbert Rd., Cleveland, Ohio 44106
BERGER, Florence B., 3733 Laurel Ave., Brooklyn 24, N.Y.
BERGER, Samuel, 40 Tehama St., Brooklyn, N.Y. 11218
BERGER-VOESENDORF, Alfred V., 1916 17th St., N.W. Apt. 214, Washington, D.C. 20009
BERGERON, John A., 2 Elsmere Ave., Methuen, Mass. 01844

BERGLAND, Allan E., Northern Arizona Univ., Col of Bus. Admin., Flagstaff, Ariz. 86001
BERGLUND, Milton E., 1822 Brett Rd., Apt. 18, Pittsburgh, Pa.
BERGMAN, James R., 49 Meadow Lane, Lawrence, N.Y. 11559
BERGMANN, (Mrs.) Barbara B., Univ. of Maryland, Dept. of Econ., College Park, Md.
BERGOLD, Roy T., Jr., 14348 Sanderson, Dolton, Ill. 60429
BERGREEN, Sholem, 55 Old Meadow Rd., West Hartford, Conn. 06090
BERGSMAN, Joel, USAID/B ADDP, APO New York 09676
BERGSON, Abram, Harvard Univ. Econ. Dept., Littauer Center, Cambridge, Mass. 01922
BERGSTEN, C. Fred, 6129 Leesburg Pike, Apt. 920, Falls Church, Va. 22041
BERGSTROM, Erik E., 644 Guinda, Palo Alto, Calif. 94301
BERGSTROM, Theodore C., Serra House, Serra St., Stanford, Calif.
BERINGER, Christoph, O.E.C.D. Financial Policies Div., 2, rue André Pascal, Paris 16e, France
BERKE, Leonard M., 6524 Elgin Lane, Bethesda, Md. 21811
BERKI, Sylvester E., Cornell Univ., Dept. of Econ., Ithaca, N.Y.
BERKMAN, Sigmund, 3914 Virgilia St., Chevy Chase, Md. 20015
BERKOWITZ, Monroe, Dept. of Econ., Rutgers Univ., New Brunswick, N.J.
BERLE, Elihu M., 4320 Chestnut St., Philadelphia, Pa. 19104
BERLIN, Lawrence H., 9911 Old Spring Rd., Kensington, Md. 20795
BERLINER, Joseph S., Dept. of Econ., Brandeis Univ., Waltham, Mass. 02081
BERMAN, Dale, Goodbody and Co., 2 Broadway, New York, N.Y. 10004
BERMAN, Edward B., 8 McKinley Rd., Marblehead, Mass. 01945
BERMAN, Eric, 612 S. 11th St., Laramie, Wyo.
BERMAN, Lawrence D., 427 S. Henderson, Apt. 13, Bloomington, Ind. 47403
BERMAN, Louise, 1432-F Arch, Berkeley, Calif.
BERMAN, Stanley M., U.S. Dept. of Labor, Washington, D.C. 20210
BERMUDEZ, Jorge, Box 2251, San Juan, P.R. 00903
BERNARD, Normand R. V., B. of Governors of The Fed. Res. System, Washington 25, D.C.
BERNARD, Richard C., Jr., 310 Riverside Dr., New York, N.Y. 10025
BERNARDO, Lester L., 69 Lanning Ave., Trenton, N.J. 08618
BERNARDO, Roberto M., 2631-A Durant, Berkeley, Calif. 94704
BERNEY, Robert E., Arizona State Univ., Dept. of Econ., Tempe, Ariz, 85281
BERNFIELD, Frederick M., 1111 Arlington Blvd., Apt. M908, Arlington, Va. 22209
BERNHARD, (Mrs.) Carolyn G., 5907 Bradley Blvd., Bethesda, Md. 20014
BERNHARD, Leroy F., 212 W. Washington St., Chicago, Ill. 60606
BERNHARD, Richard C., Univ. of Utah, Dept. of Econ., Salt Lake City 12, Utah
BERNHARDT, Irwin, 5 Forber Ter., Pittsburgh 17, Pa.
BERNHARDT, Joshua, 1346 Conn. Ave. Rm. 604, Washington 6, D.C.
BERNOLAK, Imre, 2308 Hillary Ave., Ottawa 8, Ont., Canada
BERNSTEIN, Blanche, 2500 Q St. N.W., Washington 7, D.C.
BERNSTEIN, Edward M., 1821 23rd St., N.W., Washington, D.C. 20008
BERNSTEIN, Herbert M., Washington and Jefferson Col., Dept. of Econ., Washington, Pa. 15301
BERNSTEIN, Irving, Inst. of Ind. Rela., Univ. of California, Los Angeles 24, Calif.
BERNSTEIN, Joseph, 13609 S. Spinning Ave., Gardena, Calif. 90249
BERNSTEIN, Leonard A., 380 N. Niagra Ave., Lindenhurst, L.I., N.Y. 11757
BERNSTEIN, Martin, 1020 Grand Concourse, Apt. 15V, New York 51, N.Y.
BERNSTEIN, Michael J., 5427 Carlton Way, Los Angeles 27, Calif.
BERNSTEIN, Peter L., 1161 York Ave., New York 21, N.Y.
BERNSTEN, Bernard P., 3002 Trinity Dr., Belair-at-Bowie, Md. 20715
BERNSTEN, (Mrs.) Elizabeth L., 3002 Trinity Dr., Belair-at-Bowie, Md. 20715
BERNT, H. H., 46 Park Pl., Geneva, N.Y. 14456
BERRA, Lubio R., 98 Seminary Ave., Yonkers, N.Y. 10704
BERRIDGE, William A., R.D. 3, Great Barrington, Mass. 01230
BERRY, Charles H., Brookings Institution, 1775 Massachusetts Ave. N.W., Washington 36, D.C.
BERRY, Dale A., Dept. of Econ., Drake Univ., Des Moines 11, Iowa
BERRY, George, Dept. of Finance, Texas Tech. Col., Lubbock, Tex.
BERRY, R. Albert, 52 Hillhouse Ave., New Haven, Conn.
BERRY, Ralph E., Jr., 40 John St., Newton, Mass. 02159
BERRY, Richard M., 2706 Babbitt Lane, Bowie, Md. 20715
BERRY, Robert E., Dept. of Econ., Miami Univ., Oxford, Ohio 45056
BERRY, Thomas S., Univ. of Richmond, Box 5, Richmond, Va. 23173
BERTRAM, Rudolf F., 520 Locust Hill Dr., Knoxville, Tenn. 37920
BESEN, Stanley M., Rice Univ., Dept. of Econ., Houston, Tex. 77001
BESSE, Clifford S., Xavier Univ., Cincinnati, Ohio 45207
BEST, Robert A., 3817 Cathedral Ave., Washington, D.C. 20016
BESTERS, Hans, Gutenbergstrasse 7, 463 Bochum-Querenburg, Germany
BETCKE, Ernest R., 355 Atherton Hall, University Park, Pa. 16802
BETH, John L., Jr., Nazareth Col. of Kentucky, Nazareth, Ky. 40048
BETHKE, Robert H., Discount Corp. N.Y., 58 Pine St., New York 5, N.Y.
BETHKE, William, 762 Hyslip Ave., Westfield, N.J. 07090
BETHLEN, Francis R., State Univ. Col., Plattsburgh, N.Y.
BETTIS, Lee W., 3260 Sawtelle Blvd., Apt. 2, Los Angeles, Calif. 90066
BETTMAN, James R., 572 Yale Sta., New Haven, Conn. 06520
BETTS, James W., c/o Dun and Bradstreet, Inc., P.O. Box 4063, Honolulu, Hawaii
BETTY, Sam M., 63 S. McGregor Ave., Spring Hill, Ala. 36275
BETZ, George W., Fed. Res. Bank of St. Louis, Res. Dept., P.O. Box 442, St. Louis, Mo. 63166
BETZ, H., Univ. of Alberta, Dept. of Polit Econ., Calgary, Alta., Canada
BETZ, Robert T., 6130 Faircrest Dr., Cincinnati, Ohio 45224
BEUTHE, Michel V., 911 Sherman, Evanston, Ill. 60202
BEUTLER, Renate, Rotenberg 31, 355 Marburg/Lahn, Germany
BEVINS, Robert J., Kansas State Univ., Dept. of Econ., Manhattan, Kans. 66504
BEYDA, Eileen M., Univ. of Colorado, Sch. of Bus., Boulder, Colo.
BEYER, John C., 66 Grove St., Wellesley, Mass
BEYERS, William B., 4807 Beach Dr., S.W., Seattle 16, Wash.
BEZA, Sterie, Int. Monetary Fund, 19th and H Sts., N.W., Washington 25, D.C.
BEZANSON, Anne, 55 Block St., Abington, Mass. 02018
BHALLA, Bharat, Bank of New York, Investment Res. Dept., 48 Wall St., New York 15, N.Y.
BHATIA, Mohinder S., Clapp and Mayne, Inc., USAID, P.O. Box "J", Balboa, Canal Zone
BHATIA, Sarvan K., Armstrong State Col., Savannah, Ga. 31404
BHATTACHARYA, Dwijaraj, Univ. of Toledo, Econ. Dept., Toledo, Ohio 43606
BHOURASKAR, D. M., Box 20, Grand Central P.O., New York, N.Y.
BIACCHI, Lawrence L., 6823 Riverdale Rd. Apt. D-202, Riverdale, Md. 20840
BIALKIN, Kenneth J., 1 Chase Manhattan Plaza, New York 5, N.Y.

BIANCHI, Andres, Cienes, Casilla 10015, Santiago, Chile
BICARD, Mario, 1A C.P. 222, San Salvador, El Salvador
BICKEL, Herbert J., Tex. Eastern Transmission Corp., P.O. Box 2521, Houston, Tex.
BICKLEY, John H., P.O. Box 82, Skokie, Ill. 60078
BICKSLER, James L., 1151 Noel Dr., Menlo Park, Calif.
BIEDERMAN, H. R., Dept. 01-14, Lockheed Aircraft Corp., Burbank, Calif.
BIEDERMAN, Paul S., 330 3rd Ave., Apt. 16-C, New York, N.Y. 10010
BIELER, Thomas A., 301 Woodlawn Pike, Apt. F 10, Knoxville, Tenn. 37920
BIELEY, (Mrs.) Peggy M., Bieley Wagner and Associates, Inc., 1515 N. S. 7th St., Miami, Fla. 33125
BIEN, Bettina H., Found. for Econ. Educa., Inc., Irvington-on-Hudson, N.Y.
BIERI, Hermann G., Lutertalweg 9, Bolligen, Switzerland
BIERMAN, Harold, Jr., Grad. Sch. of Bus. Admin., Cornell Univ., Ithaca, N.Y.
BIERWAG, G. O., Dept. of Econ., Univ. of Oregon, Eugene, Ore.
BIESDORF, Heinz B., Cornell Univ., Col. of Home Econ., Dept. of Textiles and Clothing, Ithaca, N.Y.
BIGGS, Robert M., Univ. of Toledo, Dept. of Econ., Toledo 6, Ohio
BIGLER, Eugene, 427 E. School St., Kent, Ohio
BIGLER, William R., 86 Highland Ave., Columbus, Ohio
BIGNOLI, Patricia J., 328 W. 44th St., New York, N.Y. 10036
BIGOSINSKI, Jerry T., 439 Crest Dr., Northvale, N.J. 07647
BILAS, Richard A., Dept. of Econ., Univ. of Southern California, Los Angeles 7, Calif.
BILBIJA, Zarko G., Florida State Univ., Dept. of Econ., Tallahassee, Fla.
BILKEY, (Mrs.) Angelica, Univ. of Wisconsin, Sch. of Com., Madison, Wis.
BILKEY, Warren J., Univ. of Wisconsin, Sch. of Com., Madison, Wis.
BILLINGHAM, (Mrs.) Carol J. M., Birmingham Rd. R. 2, Alma, Mich. 48801
BILLINGS, Arthur G., 33 S. 22nd St., Terre Haute, Ind. 47803
BILLINGS, Clarence D., 4-G University Heights, Columbia, Mo. 65201
BILLINGS, Donald B., 2833 Kalmia Lee Court, Apt. 301, Falls Church, Va. 22042
BILLINGS, J. A., Mankato State Col., Sch. of Bus., Dept. of Econ., Mankato, Minn. 56001
BILLINGS, Martin H., Michigan State Univ., Dept. of Agric. Econ., Wells Hall, East Lansing, Mich.
BILLINGS, R. Bruce, Univ. of Arizona, Col. of Bus. and Pub. Admin., Dept. of Econ., Tucson, Ariz. 85721
BILLINGSLEY, Ray V., Texas A. and M. Univ., Agric. Econ., College Station, Tex.
BILLINGTON, Wilbur T., Res. Dept., Fed. Res. Bank of Kansas City, Kansas City, Mo.
BILLMYER, David, 438 Second Ave., Albany, N.Y. 12209
BILLS, Mary J., 2401 Lake Dr., Allison Park, Pa. 15101
BILSBORROW, Richard E., 024 Lakeview, Mundelein, Ill. 60060
BINDER, Yevette M., 2775 E. 15th St., Brooklyn, 35, N.Y.
BING, Ralph A., One Stetson Ave., Mill Valley, Calif.
BINGHAM, R. Gordon, Jr., 1518 Oak, Evanston, Ill. 60201
BINGHAM, Robert C., Col. of Bus. Admin., Kent State Col., Kent, Ohio
BINGHAM, Tayler H., c/o Yowell, 139 Hillspoint Rd., Westport, Conn. 06882
BINGHAM, Thomas V., 1130 Lakeside Dr., Birmingham, Mich.
BINHAMMER, H. H., Dept. of Econ., Royal Military Col. of Canada, Kingston, Ont., Canada
BINIEK, Joseph P., 2620 S. Fern St., Arlington 2, Va.
BINKLEY, Howard L., c/o Pharmaceutical Mfrs. Asso., 1155 15th St., N.W., Washington, D.C. 20005
BINNEWIES, Rudolf M., 1027 Greenwood St., Evanston, Ill. 60202
BIRCH, Cecil M., 366 Sunset, Windsor, Ont., Canada
BIRCH, Eleanor M., Univ. of Iowa, Dept. of Econ., Phillips Hall, Iowa City, Iowa
BIRCH, John W., Univ. of Wyoming, Col. of Com. Ind., Laramie, Wyo. 82071
BIRCKMAYER, Harold D., State Univ. of New York, at Albany, Albany, N.Y. 12203
BIRD, Alan Ross, Rural Renewal and Dev. Inv., AED, RDED, ERS, US Dept. of Agric., Washington, D.C. 20250
BIRD, Francis H., 3539 Biddle St., Cincinnati 20, Ohio
BIRD, Michael, 1740 Athens, #1, Boulder, Colo.
BIRD, Michael D., 413 Hamilton Avenue, Silver Spring, Md. 20901
BIRD, Richard M., c/o Fundación Ford, Apartado Aereo 11528, Bogotá, Colombia
BIRD, Roger C., 809 Wilbur St., Easton, Pa.
BIRDSALL, William C., Woodstock Col., Woodstock, Md. 21163
BIRDZELL, Luther E., Jr., General Electric Co., 570 Lexington Ave., Rm. 3700, New York, N.Y. 10022
BIRDZELL, Ruth A., 1509 Delmont Ct., Urbana, Ill. 61801
BIRKENHEAD, Thomas B., 95 Dosoris Lane, Glen Cove, L.I. N.Y. 11542
BIRNBAUM, Eugene A., 6512 Elgin Lane, Bethesda 14, Md.
BIROWO, A. T., Box 147, Djakarta, Indonesia
BISCHOFF, Charles W., 367 Harvard St., Apt. 5, Cambridge 38, Mass.
BISCO, Barbara, 110 East End Ave., New York 28, N.Y.
BISCOE, Alvin B., Jr., Univ. of Florida, Col. of Bus. Admin., Dept. of Fin. and Ins., Gainesville, Fla. 32603
BISH, Robert L., Indiana Univ., Grad. Res. Center, Box 194, Bloomington, Ind. 47406
BISHOP, Charles E., North Carolina State Col. of Agric. Econ., Box 5368, Univ. of North Carolina, Raleigh, N.C.
BISHOP, George A., 35 Jennie Lane, Westport, Conn.
BISHOP, George W., Jr., Northern Illinois Univ., Dept. of Fin., De Kalb, Ill. 60115
BISHOP, John H., 510 E. Catherine, Ann Arbor, Mich. 48104
BISHOP, Philip W., Smithsonian Inst., U.S. Nat. Museum, Dept. of Arts and Mfg., Washington, D.C. 20560
BISHOP, Robert L., Massachusetts Inst. of Tech., 52-371, Cambridge, Mass. 01922
BISHR, Mahmoud F., Oscaleta Rd., South Salem, N.Y.
BISKUP, Reinhold, Franzstrasse 9, 5 Cologne-Lindenthal, W. Germany
BISSELL, Richard M., Jr., 22 Mountain Rd., Farmington, Conn. 06032
BISSEY, William K., R.R. 5, Columbus, Ind. 47201
BITNER, Claude A., Texas A. and M. College, Dept. of Econ., College Station, Tex.
BITTERMANN, Henry J., 6931 33rd Street, N.W., Washington, D.C. 20015
BITTERMANN, (Mrs.) Kathleen S., 6931 33rd Street, N.W., Washington, D.C. 20015
BITTMAN, Richard A., 3208 Coronado Ave., Farmington, N.M. 87401
BIVENS, Gordon E., Dept. of Econ., Univ. of Wisconsin, 3203 N. Downer Ave., Milwaukee 11, Wis.
BIXBY, (Mrs.) Eleanor R., 105 Walker Lane, Needham, Mass. 02159
BIXBY, Willis W., 39 Whiskey Bottom Rd., Laurel, Md. 20810
BLACK, Albert G., 3011 College Ave., Berkeley, Calif. 94705
BLACK, Edward R., 151 E. 83rd St., New York 28, N.Y.
BLACK, Fred P., 2228 Land Ave., Laramie, Wyo.
BLACK, Guy, Exec. Office Bldg., Rm. 395, Washington, D.C.
BLACK, Harry, 8938 N. Nashville, Morton Grove, Ill. 60053
BLACK, J. Roy, Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101

BLACK, James G., 2610 River Park Dr., Columbus 21, Ohio
BLACK, (Mrs.) Jeannetta W., 6655 MacArthur Blvd., Washington, D.C. 20016
BLACK, Joseph B., Jr., Miami Univ., Sch. of Bus. Admin., Dept. of Fin., Oxford, Ohio 45056
BLACK, Kelly J., 5633 Autry, Lakewood, Calif. 90712
BLACK, Robert B., 6655 MacArthur Blvd., Washington, D.C. 20016
BLACK, Robert P., Fed. Reserve Bank, Res. Dept., Richmond, Va.
BLACK, Robert R., Univ. of Akron, 302 E. Buchtel Ave., Akron 4, Ohio
BLACK, Stanley, 23 Court St., New Haven 11, Conn.
BLACK, William E., 2726 Argyle Rd., Montgomery, Ala. 36111
BLACKBURN, James M., 6941 Ave. H, Houston, Tex. 77011
BLACKBURN, John O., 208 Pineview Rd., Durham, N.C.
BLACKHURST, Richard G., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
BLACKMAN, Herbert N., 7004 Wilson Lane, Bethesda, Md. 20034
BLACKMAN, James H., Cherokee Circle, Chapel Hill, N.C.
BLACKMAN, John L., Jr., 1106 N. Pleasant St., North Amherst, Mass. 01059
BLACKMORE, John, Univ. of Minnesota, Inst. of Agric., St. Paul, Minn. 55101
BLACKSIN, Jack, 920 Snure Road, Silver Spring, Md.
BLADEN, Vincent, Faculty of Arts, Univ. of Toronto, Toronto 5, Ont., Canada
BLAINE, James C. D., Univ. of North Carolina, Sch. of Bus. Admin., Chapel Hill, N.C.
BLAIR, Andrew R., 2760 Grand Concourse, Bronx 58, N.Y.
BLAIR, Calvin P., Univ. of Texas, B.E.O.B. 724, Austin 12, Tex.
BLAIR, John M., Star R. 3, La Plata, Md. 20646
BLAIR, Roger D., Michigan State Univ., 1102-C Univ. Village, East Lansing, Mich.
BLAKE, Archie, 225 Garden Rd., Columbus, Ohio 43214
BLAKE, Charles H., Jr., Central Washington State Col., Ellensburg, Wash. 98926
BLAKE, William H., Jr., 4712 Ravensworth Rd., Annandale, Va.
BLAKEY, Roy G., 615 Ocean Ave., Apt. B, Santa Monica, Calif.
BLALOCK, (Mrs.) Dorothy B., Western New Mexico Univ., Silver City, N M. 88061
BLANCATO, Alfred F., 189-19 50th Ave., Fresh Meadows, N.Y. 12736
BLANCHARD, David G., 423 E. 4th St., Bloomington, Ind.
BLANCO, Alberto, Casilla 923, Santiago de Chile, Chile
BLANCO, Cicely W., 4436 Burlington Pl., N.W., Washington, D.C. 20016
BLANK, David M., 135 Old Farm Rd., Pleasantville, N.Y. 10570
BLANK, Uel, Agric. Extension Service, Inst. of Agric., 190 Coffey Hall, St. Paul, Minn. 55101
BLASE, Melvin G., Univ. of Missouri, Mumford Hall, Columbia, Mo.
BLASER, Arthur F., Jr., American Embassy Box 360, APO New York 09080
BLASS, Walter P., 2394 Whittier Ave., Westfield, N.J.
BLASZ, Murray, 2611 W. 2nd St., 3-B, Brooklyn, N.Y. 11223
BLAU, Gerda J., F.A.O., Viale delle Terma di Caracalla, Rome, Italy
BLAUER, (Mrs.) Rosalind, 2780 Darlington Pl., #24, Montreal 26, Que., Canada
BLAUG, Mark, Inst. of Educa. London Univ., Malet St., London W.C. 2, England
BLEDSOE, William A., 141 Valley Rd., San Carlos, Calif.
BLEE, Francis J., Smith Kline and French Laboratories, 1500 Spring Garden St., Philadelphia 1, Pa.
BLEICH, Edmund, 642 Gilbert Ave., Pearl River, N.Y. 10965
BLICKSILVER, Jack, Georgia State Col., 33 Gilmer St., S.E., Atlanta 3, Ga.
BLISS, Charles A., Harvard Bus. Sch., Soldiers Field, Boston 63, Mass.
BLISS, John Alden, East Greenbush, New York, N.Y.
BLISS, Perry, State Univ. of New York at Buffalo, Sch. of Bus. Admin., Buffalo 14, N.Y.
BLITCH, Charles P., Old Dominion Col., Sch. of Bus., Norfolk, Va. 23508
BLITMAN, Samuel, 6600 Luzon Ave., N.W., Washington 12, D.C.
BLITSTEIN, Allen, 4323 Centennial Hall, Univ. of Minnesota, Minneapolis, Minn. 55455
BLITZ, Rudolph, Dept. of Econ., Vanderbilt Univ., Nashville, Tenn.
BLITZSTEIN, Marvin, 2659 Elbridge St., Philadelphia 49, Pa.
BLOCH, Ernest, 180 Park Row, New York, N.Y. 10038
BLOCH, Ernest, 1515 47th St., Brooklyn, N.Y. 11219
BLOCH, Henry S., 757 3rd Ave., New York 17, N.Y.
BLOCH, Herman D., 16 Sachem St., East Rockaway, L.I., N.Y. 11518
BLOCK, Herbert, 6000 Green Tree Rd., Bethesda, Md. 21811
BLOCK, Howard R., Lawrence Col., Dept. of Econ., Appleton, Wis. 54910
BLOCKI, Robert A., 500 Peshtigo Ct., Chicago, Ill.
BLODGETT, Forrest C., 2nd Region, ARADCOM, Richards-Gebaur AFB, Mo.
BLODGETT, Ralph H., Dept. of Econ., Univ. of Florida, Gainesville, Fla.
BLODGETT, Terrell D., Univ. of Texas, Dept. of Econ., Austin, Tex. 78712
BLOMBERG, David J., 11 Seaman St., Madison, N.J. 07940
BLOMSTROM, Robert L., Michigan State Univ., 408 Eppley Center, East Lansing, Mich.
BLONDEEL, Etienne E., Univ. of Natal, Econ Dept., King George V Ave., Durban, S. Africa
BLOOD, Dwight M., Div. of Bus. and Econ. Res., Box 3275, Univ. Sta., Laramie, Wyo. 82071
BLOOM, Adele, 24 Riverside Dr., New York 23, N.Y.
BLOOM, Clark, Ford Found., Box 2379, Beirut, Lebanon
BLOOM, Edward, 279 Quentin Rd., Brooklyn 23, N.Y.
BLOOM, Jay D., Hasbrouck Apts. 22-B, Church St., New Paltz, N.Y. 12561
BLOOM, Max R., Col. of Bus. Admin., Syracuse Univ., Syracuse 10, N.Y.
BLOOM, Nathan, 23 Milton Grant Dr., Blauvelt, N.Y. 10913
BLOOMBERG, L. N., Bur. of Budget, Exec Office Bldg., Washington 25, D.C.
BLOOMFIELD, Arthur I., Dept. of Econ., Univ. of Pennsylvania, Philadelphia 4, Pa.
BLOUGH, Roy, 450 Riverside Dr., Apt. 82, New York 27, N.Y.
BLOUNT, Gerald R., Jr., 5605 North 35th. Rd., Arlington, Va. 22207
BLOWERS, Charles W., TVA, Div. of Forestry Dev., Norris, Tenn. 37918
BLUM, James L., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48104
BLUM, Lester, Dept. of Econ., Colgate Univ., Hamilton, N.Y. 13346
BLUM, Robert, 3707 Williams Lane, Chevy Chase, Md. 20015
BLUMBERG, Aryeh, 5801 S. Dorchester Ave., Apt. 7B, Chicago, Ill.
BLUMEL, Joseph C., Div. of Soc. Sciences, Portland State College, P.O. Box 751, Portland 1, Ore.
BLUMENFELD, Arthur A., 6017 Redondo Ct., N.W., Albuquerque, N.M.
BLUMENTHAL, W. Michael, c/o U.S. Mission, Geneva, Switzerland
BLUMNER, Sidney M., Chico State Col., Dept. of Econ., Chico, Calif. 95927
BLURTON, Michael A. S., 1944-A Orchard St., Urbana, Ill.
BLYN, George, Econ. Dept., Rutgers Univ., Camden, N.J.
BLYN, Martin R., Fed. Res. Bank of Cleveland, Cleveland, Ohio 44124
BOALS, Gordon P., 3201 36th St., N.W., Washington, D.C. 20016
BOAN, Jack A., Univ. of Saskatchewan, Regina Campus, Regina, Sask., Canada

BOARMAN, Patrick M., Bucknell Univ., Dept. of Econ., Lewisburg, Pa. 17837
BOATWRIGHT, John B., Humble Oil and Refining Co., P.O. Box 2180, Houston, Texas 77001
BOATWRIGHT, John W., 1025 Vermont Ave., N.W. Suite 1102, Washington, D.C. 20005
BOBBA, Rao V., 160 Claremont Ave., Apt. 2-J, New York, N.Y. 10027
BOBBITT, Pearl B., 1135 Potomac Ave., Hagerstown, Md. 21740
BOBER, Mandell M., 1922 Wood St., Sarasota, Fla.
BOBER, Stanley, 926 Ivy St., Pittsburgh 32, Pa.
BOBERSKI, Irving V., 201 E. Walton Pl., Chicago, Ill. 60611
BOBO, James R., Louisiana State Univ. in New Orleans, Col. of Bus. Admin. Lake Front, New Orleans, La. 70122
BOCCIA, Angel, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
BOCK, Gerhard, Lessingstr. 8, 8570 Pegnitz, West Germany
BOCK, Robert L., State Univ. Col., Dept. of Econ and Polit. Sci., Oswego, N.Y.
BODANSKY, Harry, 9618 Dewmar Lane, Kensington, Md. 20795
BODDY, David, Univ. of Glasgow, Dept. of Soc. and Econ. Res., 12, Bute Gardens, Glasgow W. 2, Scotland
BODDY, Francis, Grad. Sch., Univ. of Minnesota, 321 Johnston Hall, Minneapolis 14, Minn.
BODE, Karl F., 2900 S. Grant St., Arlington, Va.
BODENHAMER, O. Lee, P.O. Box 302, Cambridge, Mass. 01922
BODENHORN, G. D. Diran, Ohio State Univ., Dept. of Econ., Columbus, Ohio 43210
BODIG, Richard D., 249 E. 48th St., New York 17, N.Y.
BODKIN, Ronald G., Univ. of Western Ontario, Dept. of Econ. and Soc., London, Ont., Canada
BODNER, David E., 105-15 66th Rd., Forest Hills, Queens, N Y. 11375
BOEHMLER, Erwin W., Investment Bankers Asso. of Amer., 425 13th St. N.W., Washington, D C. 20004
BOEHNE, Edward G., 413 N. Clark St., Bloomington, Ind. 47401
BOETTLER, Herbert F., 8036 Delmar Blvd., St. Louis 30, Mo.
BOGAR, Bernerd, Dept. of Econ., Ohio University, Athens, Ohio 45701
BOGGS, Richard L., 421 Bayberry Ave., Naperville, Ill. 60540
BOGUMILL, John P., American Embassy, APO New York 09662
BOGUSLAW, Robert, 5004 King Richard Dr., Annandale, Va. 22203
BOH, Lim Tay, Univ. of Singapore, Econ. Dept., Bukit Timah Rd., Singapore 10, Malaya
BOHAN, Francis J., Jr., 5504 Kempton Dr., Springfield, Va. 22151
BOHATIUK, Nicholas G., Univ. of Virginia, James Wilson Dept. of Econ., Charlottesville, Va. 22903
BOHI, Douglas R., Box 331 C.S., Pullman, Wash.
BOHLMAN, Herbert W., 1301 37th St., Des Moines 11, Iowa
BOHM, Peter J. G., Tennisvagen 60, Rotevro, Sweden
BOHMER, Peter G., 1311 Chicago Ave., Apt. 410, Evanston, Ill. 60201
BOHN, Carl, 331 Bement Ave., Staten Island, 10, N.Y.
BOHR, Kenneth A., 501 Meridian St., Falls Church, Va.
BOIE, John, 1010 W. Green St., Rm. 328, Urbana, Ill.
BOLAND, Dennis E., 1523 Lincolnshire Dr., Apt. 2, Champaign, Ill.
BOLAND, Gertrude C., California State Poly. Col., Soc. Sci. Dept., Kellogg Campus, Pomona, Calif.
BOLAND, James P., 12 Riverdale Rd., Concord, Mass. 01742
BOLAND, Lawrence A., Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee, Wis. 53211
BOLAND, Vincent F., Jr., Dept. of Economics, Univ. of Arizona, Tucson, Ariz.
BOLANOS-GUILLEN, Demetrio, Univ. of the Americas, Rm. 16, Carretera Mexico, Toluca, Mexico 10, D.F., Mexico
BOLCH, Ben W., Univ. of North Carolina, Econ. Dept., Chapel Hill, N.C.
BOLDEN, Norman Ray, 879 Park Pl., Brooklyn 16, N.Y.
BOLES, Ellen Z., 201 E. 21st St., New York, N.Y. 10010
BOLICH, Leon, 1926 Lawrence St., N.E., Washington 18, D.C.
BOLINO, August C., 11411 Lund Pl., Kensington, Md. 20705
BOLLINGER, W. L., Col. of Idaho, Caldwell, Idaho 83605
BOLLMAN, Frank H., Univ. of California, Giannini Found. of Agric. Econ., Berkeley, Calif.
BOLSTER, Richard L., 212 Prynnwood Rd., Longmeadow, Mass. 01106
BOLTEN, Steven, 63 E. 9th St., New York, N.Y. 10003
BOLTON, Roger E., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
BOMBACH, G., Feierabendstrasse 47, Basel, Switzerland
BOMBELLES, Joseph T., 3605 Northcliffe Road, University Heights 18, Ohio
BOMBERA, Ben B., Col. of Comm., De Paul Univ., 25 E. Jackson Blvd., Chicago, Ill. 60604
BOMFIM, Ronaldo F. de Sa, Rua Marcilio Dias 66, Manaus, Amazonas, Brazil
BONACCORSO, Joseph T., 20 Monroe St., Apt. LJ-7, New York, N.Y. 10002
BONANNO, Jack, 1384 39th Ave., San Francisco 22, Calif.
BONAPARTE, Michael H., 1130 N. Lake Ave., Pasadena, Calif.
BONAPARTE, Tony H., 32-21—95th St., East Elmhurst, N.Y. 12423
BONBRIGHT, James C., 29 Claremont Ave., New York 27, N.Y.
BOND, David E., Univ. of Western Ontario, Dept. of Econ., London, Ont., Canada
BOND, Floyd A., Grad. Sch. of Bus. Admin., Univ. of Michigan, Ann Arbor, Michigan
BONDE, Ruth L., Fisk Hall Rm. 314, Northwestern Univ., Evanston, Ill.
BONESS, A. James, Jr., 2401 Calvert St., N.W., Apt. 719, Washington, D.C. 20008
BONGARD, Gerald J., Pacific Tel. and Tel., 140 New Montgomery St., San Francisco, Calif.
BONHAM, John M., 5125 E. 27th Pl., Tulsa 14, Okla.
BONIFIELD, William C., Wabash Col. Econ. Dept., Crawfordsville, Ind. 47933
BONIN, Joseph M., Auburn Univ., Dept. of Econ., Auburn, Ala. 36830
BONNELL, Allen T., 11 Single Lane, Wallingford, Pa. 19086
BONNELL, Elwyn T., 6705 Loring Ct., Bethesda, Md. 20034
BONNELL, (Mrs.) Virginia C., 6705 Loring Ct., Bethesda, Md. 20034
BONNEN, James T., 1732 Linden St., East Lansing, Mich. 48823
BONNER, Gordon R., Univ. of Delaware, Robinson Hall, Newark, Del. 19711
BONNEY, Donald R., 5205 Isabell, Peoria, Ill. 61614
BONNICK, Gladstone G., Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee, Wis. 53211
BONNINGTON, Robert L., Univ. of Iowa, Col. of Bus. Admin., Iowa City, Iowa 52240
BONOMO, Vittorio, Brown Univ., Dept. of Econ., Providence, R.I. 02912
BONSER, Charles F., Indiana Univ., Grad. Sch. of Bus., Bur. of Bus. Res., Bloomington, Ind.
BONSOR, Thomas W., Eastern Washington State Col., Econ. Dept., Cheney, Wash.
BONWICH, William T., 3674 Lindell Blvd., St. Louis, Mo. 63108
BOOCHEVER, Louis C., American Embassy, APO New York 09695
BOOKBINDER, Albert I. A., 475 Bieling Rd., Elmont, N.Y.
BOOKER, Gene S., 2121 Frederick, Kalamazoo, Mich.
BOOMS, Bernard H., 317 N. Craig St., Suite 10, Pittsburgh, Pa. 15213

BOONE, Gladys, Sweet Briar Col., Sweet Briar, Va. 24595
BOORAEM, Jonathan W., 15 Cutting Ave., Worcester 6, Mass.
BOORMAN, John T., Univ. of Southern California, Dept. of Econ., Los Angeles, Calif. 90007
BOOTE, Maurice, Trent Univ., Peterborough, Ont., Canada
BOOTH, Charles R., 1722 Theodan Dr., Pittsburgh 16, Pa.
BOOTH, Donald R., 1302 Merona Pl., Anaheim, Calif.
BOOTH, John T., Fed. Res. Bank of San Francisco, Res. Dept., 400 Sansome, San Francisco, Calif. 94120
BOOTH, S. Lees, Nat. Consumer Fin. Asso., 1000 16th St., N.W., Washington 6, D.C.
BOPP, Karl R., Fed. Res. Bank, 925 Chestnut St., Philadelphia 1, Pa.
BORCHARDT, Harvard G., 4129 Woodbine St., Chevy Chase, Md. 20015
BORCHERDING, Thomas E., 3038 Feltz Ave., Cincinnati, Ohio 45211
BORCHERT, Frank R., Jr., Case Inst. of Tech., Inst. Plan., University Circle, Cleveland, Ohio
BORCICH, John M., Univ. of Washington, Dept. of Econ., Seattle, Wash.
BORDEAUX, Arthur F., Jr., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich. 48823
BORDEN, Howard C., 518 Linda Vista, Atherton, Calif. 93422
BORDEN, Neil H., 325 Morgan Hall, Soldiers Field, Boston 63, Mass.
BORDNER, Betty J., 111-B Huron Towers, 2200 Fuller Rd., Ann Arbor, Mich. 48105
BOREHAM, Gordon F., 2360 Ryan Dr., Ottawa 5, Ont., Canada
BOREK, Theodore, 1526 W. Avalon Dr., Phoenix 15, Ariz.
BORENSTEIN, Israel, 50 F Chemin de la Gradelle, Geneva, Switzerland
BORETSKY, Michael, 401 Pine Spring Rd., Falls Church, Va.
BORJAS, Armando, Apartado 2299, Caracas, Venezuela
BORKIN, Joseph, 802 Ring Bldg., Washington, D.C. 20036
BORNEMANN, Alfred, 151 Engle St. Apt. C5, Englewood, N.J.
BORNHOFEN, John O., 34 Woodridge Lane, Sea Cliff, N.Y. 11579
BORNSTEIN, Morris, Dept. of Econ., Univ. of Michigan, Ann Arbor, Mich.
BOROFSKY, N. R., General Motors Corp., 1775 Broadway, New York 19, N.Y.
BORRELL-NIVERA, José R., Rosal 44, Barcelona 4, Spain
BORTH, Daniel, Jr., 237B Himes Hall, Louisiana State Univ., Baton Rouge, La.
BORTHWICK, John M., 1325 E. 143rd St., E. Cleveland, Ohio 44033
BORTS, George H., Econ. Dept., Brown Univ., Providence 12, R.I
BORUS, Michael E., Michigan State Univ., Labor and Ind. Rela. Center, East Lansing, Mich.
BOSCHAN, (Mrs.) Charlotte F., 181 New Hempstead Rd., New City, N.Y. 10956
BOSCHAN, Paul, 181 New Hempstead Rd., New City, N.Y. 10956
BOSCH-DOMENECH, Antonio, Vrgell. 53, Barcelona 11, Spain
BOSCHER, Robert W., Horseshoe Dr.—East Hill, Poughkeepsie, N.Y.
BOSLAND, Chelcie C., Dept. of Econ., Brown Univ., Providence, R.I.
BOSS, Irwin S., 188-02 64th Ave., Apt. 6-L, Flushing, N.Y. 11365
BOSSCHER, Robert A., 7376 Arlington Blvd., Falls Church, Va. 22042
BOSSE, Daniel B., Southern Illinois Univ., Lovejoy Library Bldg., Edwardsville, Ill. 62025
BOSSEN, Marianne, Lakehead Univ., Port Arthur, Ont., Canada
BOSSMANN, Eva, c/o Prof. K. Schmidt, Sembritzkistr., 31a, 1 Berlin 41, Germany
BOSSONS, John D., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh, Pa. 15213
BOSSY, George H., 16 E. 96th St., New York 28, N.Y.
BOST, Howard L., Medical Center, Univ. of Kentucky, Lexington, Ky.
BOSTAPH, Samuel H., 4905 Vega Ct. East, Fort Worth, Tex. 76133
BOSTWICK, William M., Jr., 2665 Shasta Rd., Berkeley 8, Calif.
BOSWELL, Jerry D., Tulip Tree House, Apt. 402, Bloomington, Ind. 47403
BOTERO-PARAMO, Omar, Inter-Amer. Dev. Bank, Office 416, Washington, D.C. 20577
BOTSAS, Eleutherios N., Lafayette College, Dept. of Econ., Easton, Pa. 18042
BOTT, Ernest G., 111 Jacquelyn Dr., Savannah, Ga. 31406
BÖTTCHER, Bodo, Stresemannällee 19, 6 Frankfurt-main Sud 10, Germany
BOTTINI, Domenick, 780 Concourse Village West, Bronx, N.Y. 10451
BOTWIN, Harvey J., Millstone River Apts., Apt. D-16, Princeton, N.J. 08540
BOUCHER, Armand, 1460 Glastonbury, Ann Arbor, Mich. 48103
BOUCHER, G. P., Canada Dept. of Agric., Admin. Branch, Econ. Div., Ottawa, Ont., Canada
BOUCHERIE, Wilhelm, Hafengrenzweg 7, Munster 1/W, Western Germany
BOUDREAU, Thomas J., 2950 Jean-Guyon St. Apt. 108, Sillery, Quebec 6, P.Q., Canada
BOUDREAUX, Edward A., Jr., 2340 Grant St., Berkeley, Calif. 94703
BOUEY, G. K., Res. Dept., Bank of Canada, Ottawa, Ont., Canada
BOUJEMAA, Habib, Amn, Ecore, Menzel Abdelrahman (Bizute), Tunisia
BOULDING, Kenneth E., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48104
BOUMA, J. L., Verzetstrijderslaan 54, Groningen, The Netherlands
BOURKNEY, R. A., 515 S. Court St., Platteville, Wis. 53818
BOURNE, Howard A., Dept. of Econ., Wayne Univ., Detroit 1, Mich.
BOURNEUF, Alice, 328 Quinofequin Rd., Waban, Mass. 02568
BOURQUE, Philip J., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
BOUTWELL, Wallace K., Jr., 3314 S. 28th St., Apt. 302, Alexandria, Va. 22302
BOUVIER, Emile, Univ. of Sherbrooke, Sherbrooke, Que., Canada
BOVE, Claude, Sch. of Educa., Fordham Univ., 302 Broadway, New York 7, N.Y.
BOVE, Roger E., 204 Holden Green, Cambridge, Mass. 02138
BOVET, Eric D., 42 Curtis St., Alexandria, Va. 22308
BOWDEN, E. V., Robert R. Nathan Asso., Inc., Trust Terr. Headquarters, U.S. Trust Terr. Pacific Is., Saipan, Mariana Islands 96950
BOWDEN, (Mrs.) Mary V., 343 E. 30th St., New York 16, N.Y.
BOWEN, Clarence R., 4353 W. Parker Ave., Chicago, Ill. 60639
BOWEN, Howard R., 102 Church St., Iowa City, Iowa 52240
BOWEN, John F., Ripon Col., Dept. of Econ., Ripon, Wis.
BOWEN, William G., Ind. Rela. Sec., P.O. Box 248, Princeton, N.J. 08540
BOWER, Blair T., Resources for the Future, 1755 Massachusetts Ave., N.W., Washington, D.C. 20036
BOWER, Joseph L., Harvard Univ. Grad. Sch. of Bus. Admin., George F. Baker Found., Soldiers Field, Boston, Mass. 02163
BOWER, Richard S., Amos Tuck Sch., Hanover, N.H. 03755
BOWERS, David A., Western Reserve Univ., Dept. of Econ., Cleveland, Ohio
BOWERS, Edison L., Ohio State Univ., 1775 S. College Rd., Columbus 10, Ohio
BOWERS, (Mrs.) Patricia F., 145 E. 16th St., New York, N.Y. 10003
BOWERS, Robert D., 461 G Kenwood Dr., Euclid, Ohio 43018
BOWERS, Robert S., 2909 Memory Lane, Kalamazoo, Mich.
BOWIE, Lloyd W., Little Rock Univ., Little Rock, Ark. 72204
BOWLBY, Roger L., 1136 Burton Rd., Knoxville, Tenn. 37919

BOWLEN, Bernard J., Bus. Res. Dept., Caterpillar Tractor Co., Peoria, Ill.
BOWLER, John Francis, 10 S. Mason Ave., Chicago, Ill. 60644
BOWLES, Samuel, 11 Holly Ave., Cambridge, Mass. 02138
BOWLES, W. Donald, Dept. of Econ., American Univ., Washington 6, D.C.
BOWLIN, Oswald D. Texas Tech. Col., Sch. of Bus. Admin., Lubbock, Tex.
BOWMAN, Arthur C., 120 Moss Dr., San Antonio, Texas 78213
BOWMAN, Dean O., 1120 Richman Knoll, Fullerton, Calif.
BOWMAN, Edward R., Massachusetts Inst. of Tech., Sloan Sch. of Mgt., Cambridge, Mass. 01922
BOWMAN, Henry W., 1004 Harrison St., Syracuse, N.Y. 13210
BOWMAN, John D., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
BOWMAN, Mary Jean, Dept. of Econ., Univ. of Chicago, Chicago, Ill. 60637
BOWMAN, R. Wallace, Loews Theatres, 1540 Broadway, New York, N.Y. 10036
BOWMAN, Raymond T., Bur. of Budget, Washington 25, D.C.
BOWMAN, Ward S., Jr., Yale Univ., Law Sch., New Haven, Conn.
BOWN, Charles C., Col. of Bus. Admin., Univ. of Washington, Seattle 5, Wash.
BOWSHER, Norman N., Fed. Res. Bank St. Louis, P.O. Box 442, St. Louis 66, Mo.
BOYAJIAN, Haig, 11 Ft. George Hill, New York, N.Y. 10040
BOYD, Archibald D., 20 Hexham Rd., Ottawa 14, Ont. Canada
BOYD, J. Hayden, Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
BOYD, John D., Univ. of British Columbia, Dept. of Econ., Vancouver 8, B.C., Canada
BOYD, John G., U.S. Tariff Comm., Washington, D.C.
BOYD, Robert K., Dept. of Bus. Admin., Pacific Union Col., Angwin, Calif. 94508
BOYDEN, Theodore C., Georgia State Col., Dept. of Econ., Atlanta, Ga. 30303
BOYES, Rodney L., Univ. of Oklahoma, Norman, Okla. 73069
BOYLE, Charles E., 28450 E. Kalong Circle, Southfield, Mich.
BOYLE, Doris D., Charleston Hall, Apt. C, 4506 N. Charles St., Baltimore, Md.
BOYLE, Gerald J., 1 Yarmouth Rd., Cranford, N.J. 07016
BOYLE, John J., 137 Blodgett Roy Dr., New Britain, Conn.
BOYLE, Stanley E., Fed. Trade Comm., Bur. of Econ., Washington, D.C. 20580
BOYNE, David H., Dept. of Agric. Econ., Michigan State Univ., E. Lansing, Mich.
BRABB, George J., Dept. of Acc., Fin. and Statis., Univ. of Washington, Seattle 5, Wash.
BRACKETT, Joseph A., 4516 Van Ness St. N.W., Washington 16, D.C.
B'RACZ, Istvan, P.O. Box 352, Cathedral Sta., New York 25, N.Y.
BRADBURD, Arnold W., 7829 S. East End Ave., Chicago, Ill. 60649
BRADBURY, R. W., Col. of Bus. Admin., Univ. of Florida Gainesville, Fla. 32601
BRADEN, George D., 570 Lexington Ave., Rm. 401, New York 22, N.Y.
BRADEN, Samuel E., Dept. of Econ., Indiana Univ., Bloomington, Ind.
BRADFIELD, James, Ohio Univ., Dept. of Econ., Athens, Ohio 45701
BRADFORD, Charles H., 5409 N. 31st St., Arlington, Va. 22207
BRADFORD, Colin I., Jr., 306 W. 105th St., Apt. 8, New York 25, N.Y.
BRADFORD, David F., Stanford Univ., Dept. of Econ., Stanford, Calif. 94305
BRADFORD, Donald, 990 Grove St., Evanston, Ill. 60201
BRADFORD, Frederick A., R. D., Hector, N.Y. 14841
BRADFORD, William C., 1812 Chicago Ave., Evanston, Illinois 60201
BRADLEY, Charles E., 5 Carderock Ct., Bethesda, Md. 20034
BRADLEY, Harry R., 20207 Glasgow Dr., Saratoga, Calif. 95070
BRADLEY, Michael E., 17 Game Farm Rd., Ithaca, N.Y.
BRADSHAW, J. O., U.S. Military Acad., Dept. of Soc. Sci., West Point, N.Y. 10996
BRADSHAW, Robert C., University of Maryland, Econ. Dept., College Park, Md. 20742
BRADSHAW, William L., Jr., 146 Wood St., Apt. 2, San Francisco, Calif. 94118
BRADSHER, Julian H., Sch. of Bus., Dept. of Econ., Oklahoma State Univ., Stillwater, Okla.
BRADY, Dorothy S., 326 S. 24th St., Philadelphia, Pa. 19103
BRADY, Peter F., 46-71 Parsons Blvd., Flushing, N.Y. 11355
BRADY, Ronald W., 714 Overbrook Dr., Columbus 14, Ohio
BRADY, Thomas F., 251 Summit Ave., Buffalo 14, N.Y.
BRADY, Thomas M., 6 Irving Ct., Orinda, Calif. 94563
BRAFF, Allan J., Univ. of New Hampshire, Whittemore Sch. of Bus. and Econ., Durham, N.H. 03824
BRAGG, George, North Amer. Aviation, 1700 E. Imperial Hwy., El Segundo, Calif.
BRAINARD, Harry G., Michigan State Univ., Econ. Dept.-Marshall Hall, East Lansing, Mich. 48823
BRAINARD, William C., Cowles Found., Box 2125 Yale Sta., New Haven, Conn.
BRAINERD, (Mrs.) C. P., North Ship Rd., Exton, Pa. 19341
BRAININ, David, State Capitol, Dept. of Fin., Sacramento 14, Calif.
BRAMER, Henry C., Rd. #1, Venetia, Pa. 15367
BRAMHALL, David F., 5100 Maple Park Ave., Baltimore, Md. 21207
BRAMS, Marvin R., 85 Naples Rd., Brookline, Mass.
BRAMWELL, Willis K., Jr., Commercial State Bank, Marshalltown, Iowa 50158
BRANCH, Ben S., Jr., Univ. of Texas, Dept. of Econ., Austin, Tex.
BRANCH, Maurice L., Albion Col., Albion, Mich. 49224
BRANCO, Raul, Univ. of Texas, Dept. of Econ., Austin 12, Tex.
BRAND, Horst, 11 Ft. George Hill, New York, N.Y. 10040
BRAND, Michael, Dept. of Econ. and Bus. Admin., Texas Western Col., El Paso, Tex.
BRAND, Norman P., 1539 Cory Drive, Dayton, Ohio 45406
BRAND, Willem, Krimkade 51, Voorschoten, The Netherlands
BRANDES, Joseph, 16-36 Raymond St., Fair Lawn, N.J. 07412
BRANDIS, R. Buford, Suite 840, 1120 Connecticut Ave., N.W., Washington 6, D.C.
BRANDIS, Royall, Univ. of Illinois, Dept. of Econ., 106 Commerce Annex, Urbana, Ill.
BRANDL, Donald L., 2916 North 74th St., Milwaukee, Wis. 53210
BRANDL, John E., 326 16th Ave., S.E., St. Cloud, Minn.
BRANDOW, George, Penn State Univ., Weaver Bldg., University Park, Pa. 17774
BRANDT, Alfred, 68-43 Juno St., Forest Hills, N.Y. 12068
BRANDT, Harry, 2722 Foster Ridge Rd., N.E., Atlanta, Ga. 30329
BRANDT, Karl, Stanford Univ., Hoover Inst. on War, Revolution and Peace, Stanford, Calif. 94305
BRANDT, Richard W., 1332 Highland Ave., Blacksburg, Va. 24060
BRANDWEIN, Robert, 11822 Kim Pl., Potomac, Md. 20677
BRANN, William, Medical Center, Univ. of Alabama, Birmingham, Ala.
BRANNEN, Ted R., Univ. of Houston, Col. of Bus. Admin., Houston, Tex.
BRANNON, Gerard M., 4813 N. 24th St., Arlington 7, Va.
BRANSON, Robert E., Dept. of Agric. Econ. and Soc., A and M Col. of Texas, College Station, Tex.
BRANT, Marvin R., USAID/RNA, APO 09668 New York

BRASH, Donald T., Australian Nat. Univ., Dept. of Econ., Canberra, Australia
BRASLOW, Laurence, Univ. of Maryland, Econ. Dept., College Park, Md.
BRASWELL, Robert B., 142 West End Ave., New York, N.Y. 10023
BRATT, Elmer C., Lehigh Univ., Bethlehem, Pa.
BRAUERS, Wim K. M., Birontlaan 97, Berchem-Antwerp, Belgium
BRAULT, Florent, Univ. of Ottawa, Laurier Ave., E., Ottawa, Ont., Canada
BRAUN, Kurt, 2904 Argyle Dr., Alexandria, Va.
BRAUN, Stanislaw, Room 2635A, United Nations, New York 17, N.Y.
BRAVO, Paul R., c/o Export Credits Ins. Corp., P.O. Box 655, Ottawa 4, Ont., Canada
BRAWER, Harry L., 16900 Chatsworth St., Granada Hills, Calif. 91344
BRAY, A. P., 24 Lester St., Revesby, Sydney, Australia
BRAY, David M., 4316 Rowalt Dr., Apt. 102, College Park, Md. 24740
BRAY, James O., 3305 Middlefield Rd., Palo Alto, Calif. 94306
BRAY, Robert T., 10483½ Eastborne Ave., Los Angeles, Calif. 90024
BRAY, William H., Jr., 6100 29th St., N.W., Washington 15, D.C.
BRAZEAL, Brailsford R., Morehouse Col., Atlanta, Ga.
BRAZELTON, W. Robert, Univ. of Missouri at Kansas City, Dept. of Econ., 5100 Rockhill Rd., Kansas City, Mo.
BRAZER, Harvey E., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
BRAZER, (Mrs.) Marjorie C., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
BRAZZEL, John M., Univ. of Missouri, Dept. of Econ., Columbia, Mo. 65202
BREAK, George F., Univ. of California, Dept. of Econ., Berkeley, Calif. 94720
BREAUX, Clarence T., Port au Prince, Dept. of State, Washington, D.C. 20521
BRECHER, Irving, Dept. of Econ., McGill Univ., Montreal, Que., Canada
BRECKENRIDGE, W. N., Box 143, Waterville, Me.
BRECKNER, Norman V., Center for Naval Anal., 1401 Wilson Blvd., Arlington, Va. 22209
BREDO, William, 13820 Page Mill Rd., Los Altos Hills, Calif.
BREEN, John J., 146 Hilary Dr., Providence, R.I. 02908
BREHENY, John R., RR 1, Nauvoo, Ill.
BREHM, Carl T., Jr., Kenyon Col., Dept. of Econ., Gambier, Ohio 43022
BREHM, William A., 521 E. Franklin Ave., Neenah, Wis.
BREIDENBACH, William E., 6008 Frederick St., Springfield, Va. 22150
BREIER, Frederick A., 3249 Jackson St., San Francisco 18, Calif.
BREINDL, Vac A., Port of Seattle, P.O. Box 1209, Seattle, Washington 98111
BREIT, William L., Univ. of Virginia, Dept. of Econ., Charlottesville, Va. 22903
BREITKREUTZ, Henry R., Louisiana State Univ., Dept. of Econ. and Fin., Lakefront, New Orleans, La. 70122
BREMER, C. Daniel, 2316 37th St., N.W., Washington, D.C. 20007
BREMS, Hans, Dept. of Econ., Col. of Com., Univ. of Illinois, Urbana, Ill.
BRENAN, Sister Mary, Mundelein Col., 6363 Sheridan Rd., Chicago, Ill. 60626
BRENNAN, Ciaran, 4 Lr. Churchtown Rd., Churchtown, Dublin 14, Ireland
BRENNAN, Edward W., Wharton Sch., Acct. Dept., Univ. of Pennsylvania, Philadelphia 4, Pa.
BRENNAN, George A., Sr., McFadden's Protective Agcy., Inc., Va. and Truckee Bldg., Carson City, Nev. 89701
BRENNER, Dick, 317 Sully St., Laramie, Wyo.
BRENTS, Thomas E., 6341 Inca Rd., Ft. Worth, Tex. 76116
BRESSLER, Barry, 730 E. 95th St., Brooklyn 36, N.Y.
BREST, Joel I., Fed. Res. Bank of New York, Res. Dept., New York, N.Y. 10045
BRETHOUWER, Melvin W., 7118 Wilson Lane, Bethesda, Md. 21811
BRETON, Albert, Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
BRETSCHNEIDER, George E., 3160 W. Denver Pl., Denver, Colo. 80211
BRETZFELDER, Robert B., 2748 Woodley Pl., N.W., Washington, D.C. 20008
BREU, Theodore M., 825-D, Grad. House, West Lafayette, Ind. 47907
BREWER, Charles E., Queens Col., Dept. of Econ., Charlotte, N.C. 28207
BREWER, David W., 109 Pioneer Dr., West Hartford, Conn. 06090
BREWER, John C., Kent State Univ., Kent, Ohio
BREWER, Michael F., 6817 Connecticut Ave., Chevy Chase, Md.
BREWSTER, Maurice R., 4499 Garmon Rd. N.W., Atlanta, Ga.
BREYER, Ralph F., Wharton Sch., Univ. of Pennsylvania, Philadelphia 4, Pa.
BRIANT, R. L., Dept. of Econ., The Univ., Leicester, England
BRICKNER, Arthur, Bankers Trust Co., P.O. Box 318, New York 15, N.Y.
BRICKNER, Dale G., Indiana Univ., Labor Educa. and Res. Center, Bloomington, Ind.
BRICKSIN, Roderick M., 1812 17th St., Apt. 15, Rock Island, Ill.
BRIDENSTINE, Merwyn G., Col. of Bus. Admin., Univ. of Arkansas, Fayetteville, Ark
BRIDGES, Benjamin, Jr., 3623 Greenway Pl., Alexandria, Va. 22302
BRIDGES, G. Sadler, Texas A and M Univ., Col. of Liberal Arts, Dept. of Econ., College Station, Tex.
BRIEF, Richard P., 26 Circle Dr., Hastings-on-Hudson, New York 13076
BRIEFS, Godfrey E., RM 13-128, Gen. Marketers Bldg., Detroit 2, Mich.
BRIEFS, Goetz A., 1413 44th St. N.W., Washington, D.C.
BRIEFS, Henry W., Georgetown Univ., Washington 7, D.C.
BRIERS, James L., 26601 Normandy Rd., Bay Village, Ohio 44210
BRIGGS, Royal J., 702 N. 21st St., Superior, Wis.
BRIGGS, Vernon M., Jr., Univ. of Texas, Dept of Econ., Austin, Tex.
BRIGHAM, Eugene F., Grad. Sch. Bus. Admin., Univ. of California, Los Angeles 24, Calif
BRIGHT, John J., c/o Econ. Dept., Monrovia, Liberia
BRIL, Avram, Box 536, Village Sta., Warren, Mich.
BRILL, Daniel H., 10414 Brookmoor Ct., Woodmoor, Silver Spring, Md
BRIMMER, Andrew F., Bd. of Gov. of the Fed Res. System, Washington, D.C. 20551
BRIMMER, Kenneth R., Room 2108, F.O.B. 4, Bur. of the Census, Washington 25, D.C.
BRINBERG, Herbert R., Amer. Can Co., 100 Park Ave., New York 17, N.Y.
BRINDENSTINE, Don C., San Diego State Col., Sch. of Bus., San Diego, Calif. 92115
BRINEGAR, George K., Dept. of Agric. Econ., Univ. of Illinois, 305 Mumford Hall, Urbana, Ill.
BRINK, Dale W., 470 Ivanhoe Dr., Fairborn, Ohio 45324
BRINKER, Paul A., Univ. of Oklahoma, Norman, Okla.
BRINKMAN, Richard L., 7905 S.W. Cedar St., Apt. 53, Portland, Ore.
BRINKMANN, George L., 5612 N. 27th St., Arlington, Va. 22207
BRINSER, Ayers, 225 Corrie Rd., Ann Arbor, Mich.
BRINTON, George M., Westwood Res., Inc., Suite 1130 Kirkeby Center, 10889 Wilshire Blvd., Los Angeles, Calif. 90024
BRISTOL, Ralph B., Jr., Room 4054, Main Treasury Bldg., Washington 25, D.C.
BRITE, Robert L., 547 Chester Pl., Univ. Heights, Piscataway, N.J. 08854
BRITT, Robert D., Smith Col., Dept. of Econ., Wright Hall, Northampton, Mass. 01060
BRITTAN, Margaret R., Univ. of Denver, Col.

of Bus. Admin., 1445 Cleveland Pl., Denver 2, Colo.
BRITTO, Ronald, Tufts Univ., Econ. Dept., Medford, Mass. 02052
BRITTON, Francis E. K., 209 W. 109th St., New York 25, N.Y.
BRITTON, Virginia, 3314 Gumwood Drive, Hyattsville, Md.
BROCK, Leslie V., 1801 Dearborn, Caldwell, Idaho 83605
BROCK, Samuel M., West Virginia Univ., Div. of Forestry, Morgantown, W.Va.
BROCKHOFF, Klaus, 8 Ravensteynstr., 54 Coblenz-Pfaffendorf, Germany
BROCKIE, Melvin D., Cal. Inst. of Tech., Pasadena, Calif.
BRODA, Miguel Angel M., Cabildo 1713, 10 "2," Buenos Aires, Argentina
BRODE, John, 36 Parker St., Cambridge, Mass. 02138
BRODERICK, William D., 8238 Roselawn, Detroit, Mich.
BRODERSOHN, Mario S., Larrea 738, 29P., Dpto. 6, Buenos Aires, Argentina
BRODSKY, Leonard, 403 Washington St., Brookline, Mass. 02146
BRODSKY, Nathan, 2840 Lrocom Lane, Arlington, Va. 22207
BROEHL, Wayne G., Jr., 6 Kingsford Rd., Hanover, N.H. 03755
BROIDA, Arthur L., 207 Marthas Rd., Alexandria, Va.
BROKAW, P. L., 713 Timber Branch Dr., Alexandria, Va.
BROKAW, William C., Philadelphia Col. of Textile and Science, Schoolhouse Lane and Henry Ave., Philadelphia 44, Pa.
BROMLEY, James D., 100 Prospect Ave., North Kingstown, R.I.
BRONFENBRENNER, Martin, Carnegie Inst. of Tech., G.S.I.A., Pittsburgh 13, Pa.
BRONSON, David W., 2530 Drexel St., Vienna, Va. 22180
BROOKINS, Terry M., Dept. Bus. Admin., Prairie View A. and M. Col., Prairie View, Tex. 78661
BROOKS, David B., Resources for the Future, 1755 Massachusetts Ave., N.W., Washington 36, D.C.
BROOKS, Robert C., North Carolina State Univ., Dept. of Econ., Raleigh, N.C. 27607
BROOKS, Robert C., Jr., Vanderbilt Univ., Dept. of Econ. and Bus. Admin., Box 749, Nashville, Tenn. 37203
BROOKS, Robert D., 466 Lexington Ave., N. Y. Central System, New York 17, N.Y.
BROOKS, Thomas M., Univ. of Connecticut, Dept. of Family Econ. and Mgt., U-58, Sch. of Home Econ., Storrs, Conn. 06268
BROOKS, Verna I., 2311 N. Front St., Harrisburg, Pa.
BROTAN, Jiri, 37-26 87th St., Jackson Hgts. 72, N.Y.
BROTHERS, Dwight S., Rice Univ., Dept. of Econ. and Bus. Admin., Houston, Tex. 77001
BROUDE, Henry W., 13 W. Slope Lane, Hamden, Conn. 06517
BROUS, Ira J., 167 Smalley Rd., Syracuse, N.Y. 13210
BROUSSALIAN, Vartkes L., 7705 Persimmon Tree Lane, Bethesda, Md. 20034
BROUWER, Tony, 1652 Shagrhi-La, S.E., Grand Rapids 8, Mich.
BROVEDANI, Bruno, Viale SS. Pietro e Paolo, 1, Rome, Italy
BROWN, Alan A., 2936 West 131st Street, Gardena, California 90249
BROWN, (Mrs.) Anne M., Univ. of Illinois, Dept. of Econ., Urbana, Ill.
BROWN, Bernard, Dept. of Econ., Univ. of Houston, Houston 4, Tex.
BROWN, Byron B., Jr., Dept. of Econ., Wis. State Col., Eau Claire, Wis.
BROWN, Byron W., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
BROWN, Carl F., 23734 Brazil, Southfield, Mich. 48075
BROWN, Charles R., Federal Res. Bank of Chicago, Detroit Br., 130 W. Fort, Detroit, Mich.
BROWN, Courtney C., 4 Kent Rd. Cor. Ogden Rd., Scarsdale, N.Y.
BROWN, David G., Dept. of Econ., Univ. of North Carolina, Carroll Hall, Chapel Hill, N.C.
BROWN, David W., 709 E. 44th Street, North, Kansas City, Mo. 64116
BROWN, David W., Iowa Mission, US/AID Peru, U.S. Embassy, Lima, Peru
BROWN, Douglass V., 46 Griggs Rd., Brookline, Mass. 01506
BROWN, E. Cary, Massachusetts Inst. of Tech., Dept. of Econ., Cambridge, Mass. 01922
BROWN, Elmer J., Univ. of Arizona, Dept. of Econ., Tucson, Ariz.
BROWN, Emily C., 4327 Abbott Ave., S., Minneapolis, Minn. 55410
BROWN, Ernest J., Harvard Law Sch., Langdell Hall, Cambridge, Mass. 01922
BROWN, Francis J., De Paul Univ., 25 E. Jackson Blvd., Chicago, Ill. 60604
BROWN, Gardner, Jr., 1922 Franklin Ave., E., Seattle, Wash.
BROWN, Gilbert T., USOM-K, APO 96301, San Francisco
BROWN, Harry G., 403 S. Garth Ave., Columbia, Mo. 65201
BROWN, J. Douglas, Princeton Univ., 9 Nassau Hall, Princeton, N.J. 08540
BROWN, J. Michael, 4500 Walnut, Apt. 305, Philadelphia, Pa. 19139
BROWN, James A., Jr., Inst. for Defense Anal., 400 Army-Navy Dr., Arlington, Va. 22202
BROWN, James D., Jr., New York Univ., Grad. Sch. of Bus. Admin., 100 Trinity Pl., New York 6, N.Y.
BROWN, James R., 19 Cumberland Rd., West Hartford, Conn. 06119
BROWN, Joe E., Univ. of Toledo, Dept. of Econ., Toledo 6, Ohio
BROWN, Joseph D., Georgia Exp. Sta., Experiment, Ga.
BROWN, Josiah N., Gov. of Liberia, Bur. of Econ. Res., Monrovia, Liberia
BROWN, Keith C., 4706 Jason, Apt. 131, Dallas, Tex. 75206
BROWN, Lee, 9918 Rogart Rd., Silver Spring, Md.
BROWN, Leo C., Cambridge Center for Soc. Studies, 42 Kirkland St., Cambridge, Mass. 02138
BROWN, Lester R., 3716 Manor Rd., Chevy Chase, Md. 21620
BROWN, Linda R., 645 Neil Ave., Apt. 819, Columbus, Ohio 43215
BROWN, Murray, 3518 Newark St., N.W., Washington 8, D.C.
BROWN, Norman L., Virginia Poly. Inst., Blacksburg, Va.
BROWN, Pembroke H., 1104 W. Daniel St., Champaign, Ill. 61822
BROWN, Perry L., 1103 N. 9th St., Estherville, Iowa 51334
BROWN, Philip M., 3 Page St., Brunswick, Me. 04011
BROWN, Philip S., 1627 Connecticut Ave., Washington 9, D.C.
BROWN, (Mrs.) Phoebe R., 31-90 140th St., Flushing 54, New York
BROWN, Ralph, Jr., Yale Law Sch., 401A Yale Sta., New Haven, Conn.
BROWN, Raphael, 2941 S. Columbus St., Arlington 6, Va.
BROWN, Robert C., Stanford Res. Inst., Menlo Park, Calif.
BROWN, Robert E., Apartado 5954, Guayaquil, Ecuador
BROWN, Roger G., 40 Church St., Poultney, Vt. 05764
BROWN, Russell E., Lenoir Rhyne Col., Hickory, N.C. 28601
BROWN, Samuel L., 1525 44th St. N.W., Washington 7, D.C.
BROWN, Sidney, 210 Central Ave., Edison, N.J. 08817
BROWN, Theodore H., 25 Meadow Way, Cambridge, Mass. 01922
BROWN, Thomas M., 1121 W. Main St., Zanesville, Ohio 43701
BROWN, Truman B., Amherst Central High Sch., Soc. Studies Dept., 4301 Main St., Snyder, N.Y. 14226

BROWN, Weir M., American Embassy/USRO/F, APO 09777, New York
BROWN, Wendell V., Jr., 137 Clarendon St., Syracuse, N.Y. 13210
BROWN, William C., 170-22 Henley Rd., Jamaica, N.Y. 11432
BROWN, William C., Hampton Inst. Box 6007, Hampton, Va. 23368
BROWN, William F., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles, Calif. 90024
BROWN, William J., 597 Maple Ave., Teaneck, N.J. 07666
BROWNE, Basil, Univ. of Montana, Dept. of Econ., Missoula, Mont. 59801
BROWNE, E. Wayles, Jr., 3700 Oliver St., N.W., Washington, D.C. 20015
BROWNE, Martha S., 510 E. 23rd St., New York 10, N.Y.
BROWNE, Robert S., 214 Tryon Ave., Teaneck, N.J.
BROWNING, Edgar, Flint Hill, Virginia
BROWNING, O. E., 2704 N.W. First Ave., Gainesville, Fla. 32601
BROWNLEE, O. H., Sch. of Bus. Admin., Univ. of Minnesota, Minneapolis 14, Minn.
BROWNLIE, (Mrs.) Marian M., 28 E. 55th St., New York, N.Y. 10022
BROWNSTEIN, Howard N., 100-20 71 Rd., Apt. 604, Forest Hills, N.Y. 11375
BROWNSTEIN, Jerald J., 3922 Lankenau Rd., Philadelphia, Pa. 19131
BROZEN, Yale, Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
BRUBAKER, Sterling, 10700 Shelley Ct., Garrett Park, Md. 20766
BRUCE, Harry J., Spector Frt. System, Inc., 205 W. Wacker Dr., Chicago, Ill. 60606
BRUCE, Robert G., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh 13, Pa.
BRUCK, Nikolaus, 189 Steamboat Rd., Great Neck, N.Y.
BRUENING, Thomas M., 3310 27th St., Apt. 5, Washington, D.C. 20008
BRUHN, M. Carl, Dept. of Econ., Augustana Col., Rock Island, Ill.
BRUNGART, Robert R., American Embassy (USRO), 10811 Lombardy Rd., Silver Spring, Md.
BRUNHILD, Gordon, Dept. of Econ., Univ. of South Florida, Tampa 4, Fla.
BRUNNER, Edmund, Jr., The RAND Corp., Santa Monica, Calif.
BRUNNER, Karl, Dept. of Econ., Univ. of California, 405 Hilgard Ave., Los Angeles 24, Calif.
BRUNNER, Robert O., Social Security Admin., Res. and Stat., Baltimore, Md. 21235
BRUNO, Vincent J., 18275 Birchcrest Dr., Detroit, Michigan 48221
BRUNS, William J., Jr., c/o Project Manager, 3800 Union Bay Circle, N.E., Seattle, Wash. 98105
BRUNT, Maureen, Harvard Univ., Dept. of Econ., 1746 Cambridge St., Cambridge, Mass. 02138
BRUSCHKE, Heinrich H., St. Louis Univ., Dept. of Econ., 3674 Lindell Blvd., St. Louis, Mo. 63108
BRUSSELL, Eugene R., 11801 Rose Ave., Los Angeles, Calif. 90066
BRUTON, Henry J., Universidad de Chile, Inst. de Econ., Casilla 3861, Santiago, Chile
BRY, Gerhard, Shady Glen, West Orange, N.J.
BRYAN, Donald W., 330 Roosevelt Ave., Syracuse, N.Y. 13210
BRYAN, Leslie A., 1016 W. John St., Champaign, Ill.
BRYAN, Robert F., 600 Purchase St., Rye, N.Y. 10580
BRYAN, William, 291 Highfield Dr., Columbus, Ohio 43214
BRYAN, William R., Fed. Res. Bank, P.O. Box 442, St. Louis 66, Mo.
BRYANT, Harold L., 504 Timothy Ave., Danville, Ky. 40422
BRYANT, Keith, Jr., Howard Col., Birmingham, Ala. 35209
BRYANT, Paul F., 104-27 120th St., Richmond Hill, N.Y. 11419
BRYANT, Ralph C., Fed. Res. Bd., Div. of Int. Fin., Washington, D.C. 20551
BRYCE, Alexander D., Corpus Christi Col., Cambridge, England
BRYCE, Herrington J., 913 E. Adams, Syracuse, N.Y. 13210
BRYCE, Murray D., 47 Emerson Rd., Winchester, Mass. 01890
BRYDE, Thomas J., 4 Concord Ave., Larchmont, N.Y. 10538
BRYSKI, Anthony J., 16 Fairview Ave., Quakertown, Pa. 18951
BRYSON, Charles W., 2000 F. St. N.W., Washington 4, D.C.
BRYSON, James T., 46 Nassau Rd., Great Neck, N.Y. 11021
BRYSON, John A., 46 Nassau Rd., Great Neck, N.Y. 11021
BRYSON, Winfred O., Jr., 4514 St. Georges Ave., Baltimore 12, Mo.
BRZESKI, Andrzej, Univ. of California, Dept. of Econ., Davis, Calif. 95616
BRZYSKI, Stanley B., Malrick Rd., Troy, N.Y.
BUBECK, C. H., Chatsworth, Cliff Ter., Wales
BUCHAN, Leslie J., Sch. of Bus. and Pub. Admin., Washington Univ., St. Louis, Mo.
BUCHANAN, Briggs W., Jr., 83 Pond St., Westwood, Mass. 02090
BUCHANAN, James McG., Dept. of Econ., Univ. of Virginia, Rouss Hall, Charlottesville, Va.
BUCHHOLZ, Rogene A., The Wesley Found. of Pittsburgh, 5th and Bellefield, Pittsburgh 13, Pa.
BUCK, Donald T., Southern Connecticut State Col. Center for Econ. Educa., 501 Crescent St., New Haven, Conn.
BUCK, Hart, Toronto-Dominion Bank, Dept. of Econ. Res., King and Yonge Sts., Toronto 1, Ont., Canada
BUCK, Marion A., 12000 Edgewater Dr., Apt. 1402, Lakewood, Ohio 44107
BUCKALEW, Edward R., 6060 Cressentville Rd., Apt. N. D-11, Philadelphia, Pa. 19120
BUCKBERG, Albert, 925 25th St. N.W., Washington, D.C. 20037
BUCKINGHAM, Walter S., Jr., Drexel Inst. of Tech., Philadelphia, Pa.
BUCKLEY, Charles J., 225 Stephen Ave., Scranton 5, Pa.
BUCKLEY, Gerald, 442 E. 34th St., Brooklyn, N.Y. 11203
BUCKLEY, Louis F., 68-10 108th Ave., Forest Hills, N.Y. 12068
BUCKLEY, Richard J., Jr., 2811 S. 8th St., Arlington, Va. 22204
BUCKMAN, William B., Western and Southern Insurance Co., 400 Broadway, Cincinnati, Ohio
BUCKNAM, Roland F., 25 Parkwood St., Albany 8, N.Y.
BUCKWALTER, Henry G., 7016 Llanfair Rd., Upper Darby, Pa.
BUCKWELL, Lloyd J., Jr., Dept. of Econ. and Bus. Admin., Macalester Col., St. Paul 1, Minn.
BUCOVETSKY, M. W., 69 Douglas Dr., Toronto 5, Ont., Canada
BUCY, Marie E., 800 21st St. N.W., Washington, D.C. 20006
BUDA, Louis J., Jr., 96 Walnut St., Lynbrook, N.Y. 11563
BUDD, Edward C., Dept. of Econ., Penn. State Univ., Boucke Bldg., University Park, Pa. 17774
BUDDE, Norbert W., 6034 Kingsbury, St. Louis, Mo. 63112
BUDIN, Morris, 920 Cherry Lane, Vestal, N.Y. 13850
BUDINA, John W., Jr., 13 Weinel Dr., East St. Louis, Ill. 62203
BUDZEIKA, George, 34½ Van Corlear Pl., New York 63, N.Y.
BUDZINOWSKI, S., 16816 Stoepel, Detroit 21, Mich.
BUECHEL, Henry T., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
BUEHLER, Alfred, Wharton Sch., Univ. of Pa., Philadelphia 4, Pa.
BUEHLER, Vernon M., 2909 Valley Dr., Alexandria, Va.
BUENAGA, Ralph, Jr., 1757 Albany Ave., Brooklyn, N.Y. 11210
BUENING, Edward J., c/o Wis. Tel. Co., Rm. 1501 K, 722 N. Broadway, Milwaukee 2, Wis.
BUENO, Gerardo M., Nacional Financiera, S.A.,

Apt. No. 353 V. Carranza No. 25, Zona 1, Mexico, D.F.
BUFF, Francis F., Siena Col., Loudonville, N.Y. 13659
BUFF, Joseph A., 479 Loudon Rd., Loudonville, N.Y. 13659
BUGERA, John, 735 Splitlog Ave., Kansas City, Kans. 66101
BUGYIS, Andrew J., Montgomery Ward, Corp. Res. Dept. (B-2), 619 W. Chicago Ave., Chicago, Ill. 60607
BUKOVSKY, Alexis P., 3336 Broad Branch Ter., N.W., Washington, D.C. 20008
BULL, Elwyn M., 1705 Macon St., McLean, Va. 22101
BULL, (Mrs.) Vivian A., American Sch. of Oriental Res., Jerusalem, Jordan
BULLER, Orlan H., Kansas State Univ., Dept. of Econ., Manhattan, Kansas 66502
BULLOCK, J. Jay, 2465 St. Mary's Dr., Salt Lake City, Utah 84100
BULLOCK, Roy J., 8 N. Oakland St., Arlington, Va.
BUMA, Harold L., Bank of Amer., Dept. of Econ., Rincon Annex, P.O. Box 3415, San Francisco, Calif.
BUMAS, Lester O., 103-10 Queens Blvd., Forest Hills, N.Y. 12068
BUNCE, Edgar F., Jr., Mt. Harmony Rd., Bernardsville, N.J. 07924
BUNCH, Howard M., Southwest Res. Inst., 8500 Culebra Rd., San Antonio 6, Texas
BUNCHER, Irwin S., P.O. Box 50830, New Orleans, La. 70150
BUND, Henry, Res. Inst. of Amer., 589 Fifth Avenue, New York 17, N.Y.
BUNGER, Byron M., 1300 30th St., Apt. D-9-17, Boulder, Colo. 80302
BUNKA, Harvey C., Western Washington State Col., Bellingham, Wash. 98225
BUNTEN, John R., 4216 Shenandoah, Dallas, Tex. 75205
BUNTING, Robert L., Cornell Col. Dept. of Econ. and Bus., Mount Vernon, Iowa 52314
BURBRIDGE, R. Marc, Planning Res. Corp., 1875 Connecticut Ave. at T St., Washington, D.C. 20029
BURCH, Daniel W., 5119 N. 25th Rd., Arlington, Va. 22207
BURCH, Susan W., 231 C St., N.E., Washington, D.C. 20002
BURCROFF, Richard T., Jr., 6195 N.E. Radford Dr., Seattle, Wash. 98115
BURFORD, Roger L., Louisiana State Univ., Dept. of Bus. Fin. and Stat., Col. of Bus. Admin., Baton Rouge 3, La.
BURGER, Albert E., Jr., 206-3 Airport Rd., West Lafayette, Ind. 47906
BURGER, Alvin A., Drawer C, Capital Sta., Austin, Tex.
BURGER, David H., State Univ. of New York, Col. at Oswego, Oswego, N.Y. 13126
BURGER, (Mrs.) Marjorie K., State Univ. of New York, Col. at Oswego, Oswego, N.Y. 13126
BURGESS, Daniel F., 6405 Roberts Dr. S.E., Washington, D.C. 20031
BURGESS, Eugene W., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
BURGESS, Frederick M., Villanova Univ., Villanova, Pa. 19085
BURGESS, Leonard R., 404 Timber St., College Sta., Tex. 77840
BURGESS, Ralph E., 10201 Gary Rd., Potomac, Md. 20854
BURGESS, Robert W., 440 Pelham Manor Rd., Pelham, N.Y.
BURGESS, (Mrs.) Virginia F., 404 Timber St., College Station, Tex. 77840
BURGGRAF, (Mrs.) Shirley P., 11415 Hessler Rd., Apt. 1, Cleveland, Ohio 44106
BURHANS, N. C., 1806 Orchard St., Alexandria, Va.
BURK, Marguerite C., 1914 Arona St., St. Paul, Minn. 55113
BURK, Monroe, USAID APO, New York 09271
BURKE, James M., 6 Webley St., Allston, Mass. 02134
BURKE, John F., Jr., Eastern Illinois Univ., Charleston, Ill. 61920
BURKE, Thomas E., 3128 Lakeland Ave., Madison, Wis. 53704
BURKE, Thomas R., Georgetown Univ., Dept. of Econ., Washington, D.C.
BURKE, William M., 119 Sutro Hts. Ave., San Francisco 21, Calif. 94121
BURKETT, Evelyn B., Agric., Mech. and Normal Col., Bus. and Econ. Dept., Pine Bluff, Ark.
BURKHARD, Earl E., 1505 Archer Rd., New York 62, N.Y.
BURKHART, John, P.O. Box 72, Indianapolis, Ind. 46206
BURKHEAD, J. V., Dept. of Econ., Syracuse Univ., Syracuse, N.Y.
BURKUS, John, 6 Av. Francois-Besson, 5 et. No. 2, 1217 Meyrin, Geneva, Switzerland
BURLEY, Simon P., 214 Witherspoon St., Princeton, N.J. 08540
BURNET, Mary E., 132 Delray Rd., Rochester, N.Y. 14610
BURNETT, Howard J., Col. Center of the Finger Lakes, Corning, New York
BURNETT, Philip M., Indiana Univ. Library, Bloomington, Ind. 47401
BURNEY, Thomas G., 5 Hartford Ct., Worthington, Ohio 43085
BURNS, Arthur E., 4000 Massachusetts Ave., Washington 16, D.C.
BURNS, Arthur F., Apt. 3 H, 2 Tudor Pl. S., New York 17, N.Y.
BURNS, Arthur R., 70 La Salle St., New York, N.Y. 10027
BURNS, Edward J., Merrimack Col., North Andover, Mass.
BURNS, Eveline M., 70 La Salle St., New York, N.Y. 10027
BURNS, Joseph E., 764 Lawrence St., Apt. 2, Eugene, Ore. 97401
BURNS, Joseph M., Int. House, 1414 E. 59th St., Chicago, Ill. 60637
BURNS, Leland S., Div. of Res., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles 24, Calif.
BURNS, (Mrs.) Mary R., Route #1, Box 602, Excelsior, Minn. 55331
BURNSIDE, Malcolm, 36 Cambridge Dr., Berkeley Heights, N.J. 07922
BUROS, Allan, 14 Cabot St., Winchester, Mass., 01890
BURR, Susan S., 2800 Ontario Rd. N.W., Washington 9, D.C.
BURRESS, Glenn E., 21 Berkeley Pl., Montclair, N.J.
BURRINGTON, Leo I., 665 Lexington Ave., New York, N.Y. 10022
BURRIS, Edward C., Col. of Bus., Oklahoma State Univ., Stillwater, Okla.
BURROUGH, David L., 701 N. Bryan, Little Rock, Ark. 72205
BURROUGHS, Roy J., P.O. Box 256, Kensington, Md. 20795
BURROWS, Albert H., 1301 N. 3rd St., Marquette, Mich. 49855
BURSLER, Norman, c/o Robinson, 412 Riblett Lane, Wilmington, Del. 19808
BURSTEIN, A. C., Dept. of Com. and Ind. Dev., City of New York, 415 Madison Ave., New York, N.Y. 10017
BURSTEIN, Herbert, 160 Broadway, New York 38, N.Y.
BURSTEIN, Meyer L., Box 1282, Aspen, Colorado 81611
BURTLE, James L., c/o W. R. Grace and Co., 3 Hanover Square, New York, N.Y.
BURTON, Edwin T., III, Northwestern Univ., Dept. of Econ., Evanston, Ill. 60201
BURTON, John C., 404 Riverside Dr., New York, N.Y. 10025
BURTON, John F., Jr., Yale Univ., Econ. Dept., 602 SSS, New Haven, Conn. 06520
BURTON, Kenneth W., Cornell Univ., 49-I Hasbrouck Apts., Ithaca, N.Y.
BURTON, Richard M., 350 Commerce (W), Urbana, Ill.
BURTT, Everett J., Jr., 399 Clapboardtree St., Westwood, Mass. 02090
BUSBATE, Abdullah S., 11201 Euclid Ave., Cleveland, Ohio 44106
BUSCAGLIA, Adolfo E., 61-B Escondido Village, Stanford, Calif. 94305
BUSE, Rueben C., Col. of Agric., Dept. of

Agric. Econ., Univ. of Wisconsin, Madison 6, Wis.
BUSH, Paul D., Fresno State Col., Dept. of Econ., Fresno, Calif. 93726
BUSHARA, Munir L., Gulf South Res. Inst., Suite 414, Taylor Bldg., Baton Rouge, La. 70801
BUSHEY, A. Scott, Continental Casualty Co., Econ. Res. Dept., 310 S. Michigan Ave., Chicago, Ill. 60604
BUSHMAN, Ted, Box 46, Santa Maria, Calif. 93456
BUSHMAN, Tony, Redbud Hill, Apt. 801 Bloomington, Ind.
BUSHNELL, John A., Embassy-San Jose, Costa Rica, c/o Dept. of State, Washington, D.C.
BUSHNELL, Robert C., 171 E. College St., Oberlin, Ohio 44074
BUTCHER, Walter R., Washington State Univ., Dept. of Agric. Econ., Pullman, Wash.
BUTLER, Arthur, State Univ. of N.Y. at Buffalo, Dept. of Econ., Crosby Hall, Buffalo, New York 14214
BUTLER, (Mrs.) Darlene, 36 Myrtle Avenue, Cambridge, Mass. 01922
BUTLER, Hartman L., Jr., Duff, Anderson & Clark, 208 S. LaSalle St., Chicago, Ill. 60604
BUTLER, Joseph R., 1103 Lees Dr., Las Cruces, N.M. 88001
BUTLER, Robert A., RAND Corp., 1700 Main Street, Santa Monica, Calif.
BUTLER, Steward E., Univ. of Rhode Island, Dept. of Econ., Kingston, R.I. 02836
BUTLER, William F., c/o Chase Manhattan Bank, Dept. of Econ., New York, N.Y.
BUTT, Louise E., 3626 Van Ness St. N.W., Washington 8, D.C.
BUTT, S. McClellan, 3626 Van Ness St. N.W., Washington 8, D.C.
BUTTER, Irene H., 2339 S. Circle Dr., Ann Arbor, Mich.
BUTTERS, J. Keith, Harvard Univ., Grad. Sch. of Bus. Admin., Soldiers Field, Boston, Mass. 02163
BUTTRICK, John A., Sch. of Bus. Admin., Univ. of Minnesota, Minneapolis 14, Minn.
BUTTS, John H., P.O. Box 113, Ithaca, N.Y. 14867
BUXTON, Horace C., Jr., 517 Creek Crossing Rd., Vienna, Va. 22180
BUZZO, Joseph, 240 Liberty St., San Francisco, Calif. 94114
BYE, Raymond T., 506 Glenwood Ave., Moylan, Pa. 19065
BYE, Roger A., Box 94, Gilby, N.D.
BYERS, Norman F., Louisiana Poly. Inst., Ruston, La. 70271
BYKOSKI, Louis M., 105 Owsley Ave., Lexington, Ky.
BYL, Adhemar, Georgetown Univ., Econ. Dept., Washington, D.C.
BYMERS, Gwendolyn J., Col. of Home Econ., Cornell Univ., Ithaca, N.Y.
BYRD, Carol M., 2600 Benton Blvd., Kansas City 27, Mo.
BYRNE, Sister Antonia M., Col. of the Holy Name, 3500 Mountain Blvd., Oakland 19, Calif.
BYRNE, Donald R., 41468 Dale, Detroit, Mich. 48823
BYRON, Robert E., 250 Carlton Ave., Brooklyn 5, N.Y.
BYRON, William J., Ammendale Normal Inst., Beltsville P.O., Md.
BYUN, Hyung Yoon, Seoul Nat. Univ., Col. of Com., Seoul, Korea

CABAZA, Jose E., Jr., Box 7332, College Station, Tex. 77842
CABRAL, José M. de Morias, Rua Barata Salgueiro 29 3°D, Lisbon, Portugal
CABRERA, Kabana M., General Sanjurjo 40, Madrid 3, Spain
CACCAMO, Joseph P., 435 John St., Port Arthur, Ont., Canada
CADDY, M. Douglas, c/o N.A.M., 277 Park Ave., New York, N.Y. 10017
CADILLA-IQUINA, Carlos G., Gov. Dev. Bank for P.R., Box 4591, San Juan, P.R.
CADY, Elwyn L., 5100 Woodland Ave., Kansas City 10, Mo.
CADY, George J., Univ. of California, Riverside, Dept. of Econ., Riverside, Calif. 92507
CADY, Raymond D., 335-A Harvard St., Cambridge, Mass. 02139
CAFFE, Federico, Via A Cadlolo 42, Rome, Italy
CAGAN, Phillip D., Nat. Bur. of Econ. Res., 261 Madison Ave., New York, N.Y. 10016
CAGLARCAN, M. Erol, 1212 Kensington Rd., McLean, Va.
CAHILL, Donald A., 6822 Cloves Lane, Upper Darby, Pa.
CAHILL, James M., 1057 Esplanade, Pelham Manor, N.Y.
CAHILL, Neil, Creighton Univ., 2410 California St., Omaha, Neb. 68131
CAHN, Frances Triest, 111 E. 56th St., New York 22, N.Y.
CAILLOUET, A. Allen, 224 River Dr., East Palatka, Fla. 32031
CAIN, Glen G., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
CAIN, Leonard F., 1026 Newton St. N.E., Washington 17, D.C.
CAIN, Louis P., 9536 Ridgeway Ave., Evanston, Ill.
CAINE, Walter E., Tex. Eastern Trans. Corp., 320 Park Ave., New York, N.Y. 10022
CAIRNS, James P., Dept. of Econ., Royal Military Col., Kingston, Ont., Canada
CAIRO, Joseph P., 6716 Cornelius St., Philadelphia, Pa. 19138
CALAMITSIS, Evangelos A., Int. Monetary Fund, 19th and H Sts., N.W., Washington, D.C. 20431
CALCEDO, Orlando C., Apartado 181, Panama, Rep. of Panama
CALDARONE, Robert P., Center for Naval Anal., 1401 Wilson Blvd., Arlington, Va. 22209
CALDERWOOD, James D., Univ. of Southern Calif., Sch. of Bus. Admin., Los Angeles, Calif. 90007
CALDOR, Selina, 13125 Lutes Lane, Silver Spring, Md. 20906
CALDWELL, Edwin L., Conn. Bank and Trust, 1 Constitution Plaza, Hartford, Conn. 06103
CALE, Edward G., 6101 16th St., N.W., Apt. 708, Washington, D.C.
CALGAARD, Ronald K., Univ. of Kansas, Dept. of Econ., Lawrence, Kans.
CALHOUN, Charles P., 301 Harrison Ave., Harrison, N.Y. 10528
CALIFF, Joseph M., State Univ. Col., Potsdam, N.Y. 13676
CALIGARIS, Conrad P., Boston Col., Dept. of Econ., Chestnut Hill, Mass. 01012
CALKINS, Francis J., Western Reserve Univ., Sch. of Bus., Cleveland, Ohio 44106
CALKINS, Ralph N., 44 Fremont St., Bloomfield, N.J.
CALKINS, Robert D., The Brookings Inst., 1775 Massachusetts Ave. N.W., Washington 6, D.C.
CALKINS, (Mrs.) Susannah E., 1409 Lakeview Dr., Falls Church, Va. 22044
CALL, David L., Cornell Univ., 121 Savage Hall, Ithaca, N.Y.
CALL, Ivan T., Brigham Young Univ., Bus. Mgt. 381 JKB, Provo, Utah 84601
CALLAGHAN, J. F., Univ. of Windsor, Windsor, Ont., Canada
CALLANDER, Chester L., 211 Whitmoor Ter., Silver Spring, Md.
CALLARD, Charles G., c/o Ball Bros. Co., Inc., 1509 S. Macedonia, Muncie, Ind.
CALLEWAERT, Joseph, 7 Galileo Ct., Suffern, N.Y. 10901
CALLIAN, Vernon, 656 W. 178th St., New York, N.Y. 10033
CALMUS, Thomas W., 2424 Pennsylvania Ave. N.W., Apt. 317, Washington, D.C. 20037
CALTAGIRONE, Joseph A., 655 E. 14th St., Apt. 9-D, New York, N.Y. 10009
CALTON, Ivan D., State Col., Springfield, Mo.
CALVERT, Robert V., Box 178, University Park, N.M. 88070
CALVO, Elio R., Las Heras 1060, Resistancia, Chaco, Argentina
CAMERON, Carl R., 312 Wischman Ave., Oreland, Pa.
CAMERON, Helen A., 2244 Farleigh Rd., Columbus 21, Ohio

CAMERON, Kenneth, Jr., 11734 Montana Ave., Los Angeles 49, Calif.
CAMERON, Rondo E., Rockefeller Found., Casilla 2-P, Santiago 11, Chile
CAMMACK, T. Emerson, 1704 W. Green St., Champaign, Ill.
CAMMACK, William B., Box 105, Fishkill, N.Y. 12524
CAMMAROSANO, Joseph R., Fordham Univ., Dept. of Econ., Dealy Hall, Room 509, Bronx, N.Y. 10458
CAMPAGNA, Anthony S., Univ. of Vermont, Dept. of Com. and Econ., Burlington, Vt.
CAMPBELL, Burnham O., Univ. of California, Econ. Dept., Los Angeles 24, Calif.
CAMPBELL, C., Portland State Col., Portland 1, Ore.
CAMPBELL, Claude A., 3001 Windsor Way, Tallahassee, Fla. 32303
CAMPBELL, Colin D., 9 N. Park St., Hanover, N.H. 03755
CAMPBELL, David C., 327 Texas St., San Francisco, Calif. 94107
CAMPBELL, David R., 2614 39th St. N.W., Washington, D.C. 20007
CAMPBELL, Duncan R. G., 49 Tiverton Dr., Ottawa 5, Ont., Canada
CAMPBELL, Edward J., Drew Univ., Madison, N.J. 07940
CAMPBELL, Edward L., P.O. Box 1684, Greensboro, N.C. 27402
CAMPBELL, George C., Metropolitan Life Ins. Co., 1 Madison Ave., New York 10, N.Y.
CAMPBELL, Harrison S., 6728 Sausalito Ave., Canoga Park, Calif.
CAMPBELL, Keith O., Dept. of Agric. Econ., Univ. of Sydney, Sydney N.S.W., Australia
CAMPBELL, Kenneth J., 3-B Eastway, Greenbelt, Md. 20770
CAMPBELL, Persia, 137-27 Geranium Ave., Flushing, N.Y. 11355
CAMPBELL, (Mrs.) Rita, 26915 Alejandro, Los Altos Hills, Calif.
CAMPBELL, Robert, Dept. of Econ., Univ. of Oregon, Eugene, Ore.
CAMPBELL, Robert W., Dept. of Econ., Indiana Univ., Bloomington, Ind.
CAMPBELL, (Mrs.) Rosemary G., 9 N. Park St., Hanover, N.H. 03755
CAMPBELL, Roy K., 41-67 Judge Street Apt. 6N, Elmhurst 73, N.Y. 14059
CAMPBELL, Thomas C., Jr., Col. of Comm., West Virginia Univ., Morgantown, W.Va.
CAMPBELL, W. Glenn, 26915 Alejandro, Los Altos Hills, Calif.
CAMPBELL, William F., Rt. 6, Box 20, Charlottesville, Va.
CAMPOS, Hector, Econ. Res. Center, 1126 E. 59th St., Chicago, Ill. 60637
CAMPOS-SALAS, Octaviano, Jardines del Pedregal, de San Angel, Penas 600, Mexico 20, D.F., Mexico
CAMUS, Quirico S., Jr., Security Bank Bldg., First Nat. Credit Corp., Sta. Mesa, Manila, Philippines
CANADAY, Jon D., 482 Burnside Ave., Apt. B4, East Hartford, Conn. 06108
CANDELAS-AGOSTO, Jose B., 1702 Alaska St., Urb. San Gerardo, Rio Piedras, Puerto Rico
CANDILIS, Wray O., Amer. Bankers Asso., 90 Park Ave., New York, N.Y. 10016
CANELLAS, Andrew A., 6807 N. 30th Rd., Arlington, Va. 22213
CANFIELD, Alvah T., P.O. Box 132, University Sta., Syracuse 10, N.Y.
CANGELOSI, Vincent E., Univ. of Texas, B.E.O.B., 620 Col. Bus. Admin., Austin 12, Tex.
CANGIALOSI, Joseph S., 2336 Glenmont Circle, #106, Silver Spring, Md. 20902
CANN, Kenneth T., Indiana Univ., Southeastern Campus, Jeffersonville, Ind. 47130
CANN, Richard T., 512 Kingston Hall Ct., Hampton, Va.
CANNEGIETER, Cornelis A., American Univ. in Cairo, Dept. of Econ., 113 Sharia Kasr El-Aini, Cairo, Egypt
CANNER, Stephen J., 58 Woodland St., Worcester, Mass.
CANOYER, Helen G., St. Col. of Home Econ., Cornell Univ., Ithaca, N.Y.
CANTER, Irving B., 2508 Drexel, Detroit 15, Mich.
CANTOR, Arnold B., Int. Labor Off., Geneva, Switzerland
CANTOR, David J., Boston Univ., Dept. of Econ., 236 Bay State Rd., Boston, Mass. 02215
CANTRELL, Lang L., 855 N. Vermont Ave., Los Angeles 29, Calif.
CAPEHART, Bertis E., Amer. Iron and Steel Inst., 150 E. 42nd St., New York 17, N.Y.
CAPEL, Richard E., Univ. of Manitoba, Dept. of Agric. Econ., Winnipeg 19, Man., Canada
CAPLAN, Benjamin, Apt. W 901, 4201 Cathedral Ave. N.W., Washington 16, D.C.
CAPLAN, Edwin H., Univ. of Oregon, Sch. of Bus. Admin., Eugene, Ore.
CAPLES, Jeremiah, J., 467 Wayside Dr., San Antonio, Tex. 78213
CAPONE, Gesuele C., 1828 Eastern Pkwy., Brooklyn, N.Y. 11233
CAPRON, William M., 3205 Pickwick Lane, Chevy Chase, Md. 21620
CARASSO, Pierre A., 268 E. Broadway, New York 2, New York
CARBAJO, Alfonso, 1414 E. 59th St., Chicago, Ill.
CARBAUGH, Daniel C., Univ. of Missouri at Kansas City, 5100 Rockhill Rd., Kansas City, Mo. 64110
CARBERT, Leslie E., 843 Southampton Dr., Palo Alto, Calif.
CARBONARA, E. Vernon, 11 Canterbury Rd., New York Univ., Rockville Center, N.Y.
CARBONE, Anthony R., 62 Oak St., Reading, Mass. 01867
CARBONE, Thomas A., 1547 79th St., Brooklyn 28, N.Y.
CARDEN, George E., The Bank of Calif., N.A., Corp. Plan. Dept., 400 California St., San Francisco, California 94120
CARDEN, John G. D., 6055 E. Valley View Ct., Mentor, Ohio 44060
CARDEÑAS, Jorge, Apartado Nacional 3083, Bogota, Colombia
CARDENAS-PEREZ, Pablo, Carrera 7A No. 16-56, 7° piso, Bogota, Colombia
CAREY, James W., Communications, 119 Gregory Hall, Urbana, Ill.
CAREY, Omer L., Washington State Univ., Col. of Econ. and Bus., Pullman, Wash. 99163
CARGILL, Thomas F., Univ. of Califonia, Dept. of Econ., Davis, Calif. 95616
CARIGNAN, Robert E., Dept. of Soc. Sci., U.S. Military Academy, West Point, N.Y. 10996
CARIS, Theodore, Random House, Inc., 501 Madison Ave., New York, N.Y.
CARLE, Spencer, 3332 W. 109th St., Inglewood, Calif. 90303
CARLETON, Willard T., New York Univ., 100 Trinity Pl., New York 6, N.Y.
CARLILE, J. Philip, 2910 N. 24th St., Arlington 7, Va.
CARLIN, Alan P., 1700 Main St., Santa Monica, Calif. 90406
CARLIN, Edward A., 834 Rosewood Ave., East Lansing, Mich.
CARLIP, Alfred B., Harpur Col., Binghamton, N.Y.
CARLIP, (Mrs.) Vivian, Harpur Col., Binghamton, N.Y.
CARLISLE, Robert M., 2250 Dexter Rd., Ann Arbor, Mich.
CARLO, Carmen J., 906 St. Johns Pl., Brooklyn 16, N.Y.
CARLSEN, Eric H., Cornell Univ., Dept. of Econ., Ithaca, N.Y. 14850
CARLSON, George N., 909 S. Fifth St., Room 457, Champaign, Ill. 61822
CARLSON, Jack W., 500 Canterbury Lane, Alexandria, Va. 22314
CARLSON, John A., Purdue Univ., Dept. of Econ., Lafayette, Ind.
CARLSON, Keith M., Fed. Res. Bank, P.O. Box 442, St. Louis 66, Mo.
CARLSON, Merle T., 1702 Wirt Rd., Apt. 25, Houston, Tex. 47055
CARLSON, Norma W., 1212 Spruce St., Philadelphia, Pa. 19017
CARLSON, Ralph E., P.O. Box 551 (Sunrise Dr.), Blacksburg, Va. 24060

CARLSON, Reynold E., 333 E. 69th St., New York, N.Y. 10021
CARLSON, Robert S., Columbia GSB, Uris Hall No. 608, New York, N.Y. 10027
CARLSON, Ronald K., Northwest Airlines Compound #6, 33 Ugwsudani-Machi, Shibuyw-ku, Tokyo, Japan
CARLSON, T. L., Western Michigan Univ., Dept. of Econ., Kalamazoo, Mich.
CARLSON, Valdemar, Antioch Col., Yellow Springs, Ohio 45387
CARLSSON, Robert J., Box 302, Norwood, N.J. 07648
CARMAN, D. G., Southern Methodist Univ., Dept. of Econ., Finchen Bldg., Room 38, Dallas, Tex.
CARMEL, Alan S., 305 Hudson St., Winnipeg 19, Man., Canada
CARMICHAEL, William D., Cornell Univ., Grad. Sch. of Bus. and Pub. Admin., Ithaca, N.Y. 14850
CARMONE, Frank J., Jr., 2213 Glen Spring Lane, Broomall, Pa. 19008
CARNEIRO, Jose G., 210 Apartment 804, Rua Das Laranjeiras, Rio De Janeiro, Brazil
CARNES, Hugh B., Sch. of Bus. Admin., Tulane Univ., New Orleans 18, La.
CARNEY, David E., c/o I.D.E.P., B. P. 3186 Dakar, Republic of Senegal, W. Africa
CARNEY, James D., Jr., 1554 Unionport Rd., Bronx, N.Y. 10462
CARNEY, (Mrs.) Marna K., 3220 Cornell Ave., Dallas 5, Tex.
CAROL, Arthur, 1150 12th St., N.W., Washington, D.C. 20005
CAROLE, Charles H., 52-30 39th Dr., Apt. 9-G, Woodside, N.Y. 11377
CAROVANO, John M., Hamilton Col., Dept. of Econ., Clinton, N.Y. 13323
CARPENTER, Cecil C., Route No. 6, Greendale Rd., Lexington, Ky. 40505
CARPENTER, Claude S., 1609 8th Pl., McLean, Va. 22101
CARPENTER, (Mrs.) Joanne R., 1643 Lincoln Ave., San Jose, Calif.
CARPENTER, Kenneth E., York Univ., Dept. of Econ., Toronto, Ont., Canada
CARR, Charles R., Purdue University, Sch. of Ind. Mgt., Krannert Bldg., Lafayette, Ind.
CARR, Clarence E., 36 Watson Blvd., Pine Bluff, Ark. 71601
CARR, D. W., 56 Sparks St., Room 802, Ottawa 4, Ont., Canada
CARR, Harold N., North Central Airlines, 6201 34th Ave., S., Minneapolis 50, Minn.
CARR, Hobart C., New York Univ., Dept. of Banking and Fin., Washington Sq., New York, N.Y.
CARR, John A., Jr., 11346 Cherry Hill Rd., Apt. 204, Beltsville, Md. 20705
CARR, Robert A., Fresno State Col., Maple and Shaw Ave., Fresno, Calif. 93726
CARRICK, Paul M., Jr., Gen. Electric Co.-TEMPO, Drawer QQ, Santa Barbara, Calif.
CARRIER, Joseph E. R., 184 East Creek Dr., Menlo Park, Calif.
CARRIER, Ronald E., 5177 Princeton Ave., Memphis, Tenn.
CARRION-SERNA, Ascensio, Dept. Academico De Contabilidad, I.T.E.S.M., Sucursal De Correos "J", Monterrey, N.L., Mexico
CARROLL, Adger B., North Carolina State Univ., Agric. Econ. Dept., Room 16 Patterson Hall, Raleigh, N.C.
CARROLL, Charles A., Jr., 1062 Lyn Rd., Bowling Green, Ohio 43402
CARROLL, Clifford, Gonzaga Univ., Spokane, Wash. 99202
CARROLL, F. A., 842 Myrtlea Lane, Houston 24, Tex.
CARROLL, Frank S., 3615 Chesapeake St., N.W., Washington, D.C. 20008
CARROLL, John J., Ford Found. Calcutta Project, 477 Madison Ave., New York 22, N.Y.
CARROLL, Joseph L., 306 Adams Ave., State College, Pa. 16801
CARROLL, Norman E., Rosary Col., River Forest, Ill.
CARROLL, Philip M., 207 S. Nike Site, Ellsworth AFB, S.D. 57706
CARROLL, Richard R., King Col., Dept. of Econ., Bristol, Tenn.
CARROLL, Sidney L., 48 Irving St., Cambridge, Mass. 02138
CARROLL, Thomas F., Inter-Amer. Dev. Bank, 808 17th St. N.W., Washington 25, D.C.
CARROTHERS, Gerald A. P., Univ. of Pennsylvania, 203 Fine Arts Bldg., Philadelphia, Pa. 19104
CARSEL, (Mrs.) Sophie R., 447 Orange St., Oakland 10, Calif.
CARSON, C. Wesley, 2020 F St., N.W., Washington, D.C. 20006
CARSON, (Mrs.) Carol S., 2020 F St., N.W., Washington, D.C. 20006
CARSON, John J., 1620 Farrington Ave., Apt. 8, Unionville, Conn. 06085
CARSON, Leslie O., Augustana Col., Dept. of Bus. Admin. and Econ., Sioux Falls, S.D. 57102
CARSON, Richard L., Carleton Univ., Dept. of Econ., Ottawa 1, Ont., Canada
CARSON, William J., Magnolia Villa Apts., Walton Way, Augusta, Ga. 30904
CARSTENS, Richard, 12 Laveta Pl., Nyack, N.Y. 10960
CARTER, Anne P., 202 Brattle St., Cambridge, Mass. 01922
CARTER, Charles F., Univ. of Lancaster, Bailrigg House, Lancaster, England
CARTER, Doyle E., 16518 Diana Lane, Houston, Tex. 77058
CARTER, E. Eugene, 844 S. Ash, Wichita, Kans. 67211
CARTER, John P., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
CARTER, Lee, R. W. Booker and Assoc., Inc., 215 N. 11th St., St. Louis, Mo. 63101
CARTER, Nicholas G., 32 Salisbury Rd., Newton 58, Mass.
CARTER, Roger N., Indiana Univ., 3-6 Hoosier Courts, Bloomington, Ind. 47401
CARTER, W. Harrison, 21 Hillside Circle, Storrs, Conn. 06268
CARTER, Walter F., 615 University Park, Rochester, N.Y. 14620
CARTER, Weld, S., 125 Oakwood Dr., Antioch, Ill. 60002
CARTER, William A., Robert Frost Lane, Etna, N.H. 03750
CARTES-DUARTE, Carlos E., Oliva 420, Asuncion, Paraguay
CARTWRIGHT, Philip W., Dept. of Econ., Univ. of Washington, Seattle 5, Wash.
CARUCCI, John V., 1320 Rutger St., Utica 3, N.Y.
CARUSO, Phillip P., Michigan State Univ., Dept. of Econ., Marshall Hall, East Lansing, Mich.
CASE, Harold C. M., 612 W. Ohio St., Urbana, Ill.
CASE, Leland Stewart, 2207 Sherman Ave., Evanston, Ill. 60201
CASELLA, Cono, C. W. Post Campus, A. T. Roth Grad. Sch. of Bus. Admin., Greenvale, N.Y 11548
CASEY, John J., Jr., 2119 N. Rolfe St., Arlington, Va. 22209
CASEY, Michael T., 625 E. 14th St., New York 9, N.Y.
CASEY, William L., Jr., 189 Central St., Apt. 19, Holliston, Mass. 01746
CASH, Doris C., West Georgia Col., Econ Dept., Carrollton, Ga. 30117
CASH, Webster C., Fisk Univ., Dept. of Econ., Nashville, Tenn. 37208
CASILLAS-CELIS, Luis R., 2001 N. Adams St., Apt. 601, Arlington, Va. 22201
CASIRAGHI, Riccardo, Viale Brigate Partigiane 12/10, Genoa, Italy
CASSADY, Ralph, Jr., Univ. of California, Bur. of Bus. and Econ. Res., 4284 GBA, Los Angeles, Calif. 90024
CASSEN, Robert H., London Sch. of Econ., Houghton St., London W.C. 2, England
CASSIDY, (Mrs.) Carmel M., 12406 Sarah Lane, Bowie, Md. 20715
CASSIDY, Terence W., 109 E. 65th Street, Kansas City 13, Mo.
CASSIDY, William J., 109 E. 65th Street, Kansas City 13, Mo.

CASSIMATIS, Peter J., 19 Lorraine Dr., Eastchester, N.Y. 12060
CASSON, John J., 305 W. 18th Street, New York, N.Y. 10011
CASSTEVENS, Jerry D., 229 Johnson Ave., Mather AFB, Calif. 95655
CASTANEDA, Rolando, 297 Mansfield St., Apt. D, New Haven, Conn. 06511
CASTILLO, Eugenio, Lamartine 719 Apt. 1, Mexico 5, D.F., Mexico
CASTLE, Emery N., Oregon State Univ., Corvallis, Ore. 97331
CASTLE, R. Woodrow, P.O. Box 3152 (CRB), Johnson City, Tenn. 37603
CASTLE, Stephen S., Rutgers, The State Univ., Sch. of Bus., 18 Washington Pl., Newark, N.J. 07102
CASTLES, Ira C., Delta State Col., Cleveland, Miss. 38732
CASTRO, Amado A., P.O. Box 1504, Manila, Philippines
CASTRO, Barry, 113 Columbia Heights, Brooklyn 1, N.Y.
CATALDO, Lawrence J., Reservoir Rd., Meredith, N.H. 03253
CATANESE, Anthony V., 34 N. McKinley Ave., Apt. 102, Athens, Ohio 45701
CATLIN, Warren B., 268 Main St., Brunswick, Me. 04011
CAUAS, Jorge, San Francisco 1869, Santiago, Chile
CAULEY, James B., 315 Cherry St., West Homestead, Pa. 15120
CAULEY, Troy J., Dept. of Econ., Indiana Univ., Bloomington, Ind.
CAULFIELD, Henry P., Jr., 6625 31st N.W., Washington 15, D.C.
CAUTHORN, Robert C., Arizona Ind. Comm., 1616 W. Adams St., Phoenix, Ariz. 85007
CAVALCANTI, Clovis de Vasconcelos, R. Jose Luiz de Silveira Barros 117, Espinheiro Recife, (P), Brazil
CAVANAUGH, Lawrence F., Jr., Florida State Univ., Box 572, Tallahassee, Fla. 32306
CAVANAUGH, Raymond H., 4730 Centre Ave., Pittsburgh, Pa. 15213
CAVAZOS, Amado, H.B. Zachry Co., P.O. Box 21130, San Antonio, Tex. 78821
CAVE, Roy C., 467 Kentucky Ave., Berkeley 7, Calif.
CAVERT, William L., 1443 Grantham St., St. Paul 8, Minn.
CAVES, Richard E., Dept. of Econ., Harvard Univ., Cambridge, Mass. 01922
CAVIN, James P., 7605 Glennon Dr., Bethesda, Md. 21811
CAWTHORNE, Delmas R., Miami Univ., Sch. of Bus. Admin., Oxford, Ohio 45056
CAYAN, Peter J., State Univ., Agric. and Tech. Col., Delhi, N.Y. 13753
CAYZEDO, Al, Univ. of Cincinnati, 320 McMicken Hall, Dept. of Econ., A and S, Cincinnati, Ohio 45221
CEDERLUND, Albert, 12 Kenwood St., Chelmsford, Mass. 01824
CELIS, Miguel, Cent. Res. Bk., Dept. of Econ. Res., la. Calle Pte., San Salvador, El Salvador
CELL, Donald C., 2054 Anthony Ave., Bronx 51, N.Y.
CELLARIUS, P., Verlendge Houtweg 3, Laren (N.H.) Netherlands
CEPHALAS, Anthony P., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
CERF, J. Georg, 207 Ledyard Ave., Fayetteville, N.Y. 13066
CERNOHOUS, Zdenek, c/o F. J. Herda, 738 13th Ave., So., St. Cloud, Minn. 56301
CESAL, Lon, Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa
CESSARIO, Ralph, 103-34 42nd Ave., Corona, N.Y. 11368
CETIN, Hikment, Cinar Sokak No. 130, Yenimahalle, Ankara, Turkey
CHACE, James E., Jr., 721 N.E. 5th Pl., Gainesville, Fla. 32601
CHACKO, George K., 6801 Barr Rd., Washington, D.C. 20016
CHACKO, K. C., 72/48 Chanakypuri, New Delhi, India
CHADBOURNE, Walter W., 104 Hilltop Rd., Hilltop Manor, Wilmington 3, Del.
CHADWICK, Leonard E., San Diego State Col., San Diego, Calif. 92115
CHAFFEE, Donald M., Jr., Univ. of California, Dept. of Econ., Davis, Calif. 95616
CHAFFIN, Robert L., Gulf South Res. Inst., Suite 414, Taylor Bldg., Baton Rouge, La. 70801
CHAIRNOFF, Hugh, Fed. Res. Bk. of Philadelphia, Res. Dept., Philadelphia, Pa. 19101
CHAITIN, Anthony, 1518 Central St., Evanston, Ill. 60201
CHALECKI, Richard B., 4626 N. Knox Ave., Chicago, Ill.
CHALEFF, (Mrs.) Frieda, 195 Devon Rd., Westwood, N.J. 07675
CHALGHIAN, Charles, 35290 Moravian Dr., Fraser, Mich. 48026
CHALK, Alfred F., Texas A. and M. Univ., Faculty Exchange Box 216, College Sta. Tex. 77843
CHALLAH, Muwaffak C., P.O. Box 150, Damascus, Syria
CHALMERS, James A., 3040 Williamsburg Rd., Ann Arbor, Mich.
CHAMBERLAIN, (Mrs.) Ethel L., 2534 Beverly Ave., Santa Monica, Calif. 90405
CHAMBERLAIN, (Mrs.) Mariam, 6 LeGrand Rd., North Haven, Conn.
CHAMBERLAIN, (Mrs.) Mary Neal, 317 Logan Rd., Bethel Park, Pa. 15102
CHAMBERLAIN, Neil W., 6 LeGrand Rd., North Haven, Conn.
CHAMBERLIN, Edward H., 4 Channing St., Cambridge, Mass. 01922
CHAMBERLIN, Vell B., 3 Canoe Brook Dr., Princeton Junction, N.J. 08550
CHAMBERS, Edward J., Univ. of Washington, Col. of Bus. Admin., Seattle 5, Wash.
CHAMPOUX, Raymond J., 11 North Ave., Melrose, Mass. 02176
CHAN, Fletcher C., 16 Joice St., San Francisco, Calif. 94108
CHAN, Le Dinh, Faculty of Law, 17 Duy Tan, Saigon, Vietnam
CHANCE, William A., 519 Broad St., Warrenburg, Mo. 64093
CHANCES, Ralph J., Bates Col., Div. of Soc. Sci., Lewiston, Me.
CHAND, Mahesh, Allahabad Univ., Dept. of Econ., House No. 378, Plot No. 163—Mumfordgunj, Allahabad 2, U.P., India
CHANDA, Rajinder N., 175 Governor St., Providence, R.I.
CHANDLER, Cleveland A., Morgan State Col., P.O. Box 469, Baltimore, Md. 21212
CHANDLER, Lester V., 88 Westerly Rd., Princeton, N.J. 08540
CHANDLER, Margaret K., Columbia Univ., Grad. Sch. of Bus., Uris Hall, New York, N.Y. 10027
CHANDLER, Richard G., Michigan Col. of Tech., Houghton, Mich. 49931
CHANDRASEKARIAH, M. C., Deputy Director of Employment, 40 Infantry Rd., Bangalore 1, India
CHANDRAWANTNA, Pradit, Ministry of Communication, Rajdamnern Ave., Bangkok, Thailand
CHANDROSS, Robert H., 3Y Magie Apts., Faculty Rd., Princeton, N.J. 08540
CHANG, Alfred Z., 3935 Blackstone Ave., Bronx, N.Y. 19463
CHANG, Chien Min, United Col., Dept. of Bus. Admin., 9A Bonham Rd., Hong Kong
CHANG, Ching Gwan, Apt. 625, 4000 Tunlaw Rd. N.W., Washington 7, D.C.
CHANG, Chung Kyun, Min. of Construction, Housing Div., Seoul, Korea
CHANG, Donald M., 10226 Day Ave., Kensington, Md. 20795
CHANG, James L. Y., Virginia Military Inst., Dept. of Econ., Lexington, Va.
CHANG, John K., 713 Henry St., Apt. 208, Ann Arbor, Mich. 48104
CHANG, K. Laurence, Dept. of Econ., Western Reserve Univ., Cleveland 6, Ohio
CHANG, Kia Ngau, Hoover Inst., Stanford Univ., Stanford, Calif. 96128
CHANG, Mabel L., 501 W. 123rd St., Apt. 5-C, New York 27, N.Y.

CHANG, Pei-Yang, 215 W. 92nd St., New York, N.Y. 10025
CHANG, Robert C., 35 Hanover St., Bridgeport, Conn. 06604
CHANG, Se Moon, 711 S. Marion St., Carbondale, Ill.
CHANG-SOON, Yoo, 244-92, Huam-dong, Yongsan-ku, Seoul, Korea
CHANG-YOL, Lee, Korea Univ., Anam Dong, Songbuk-ku, Seoul, Korea
CHANOWITZ, Gerson N., Aerospace Ind. Asso., 1725 DeSales St., N.W., Washington, D.C. 20036
CHANT, John F., Dept. of Political and Econ. Sci., Queens Univ., Kingston, Ont., Canada
CHANTAGUL, Preeda, Ministry of Nat. Dev., Tech. and Plan. Off., Bangkok, Thailand
CHAO, James, Central Methodist Col., Bus. and Econ. Dept., Fayette, Mo. 65248
CHAO, Joseph T. C., 518 W. Broadway, Granville, Ohio 43023
CHAO, Kang, Univ. of California, Dept. of Econ., Berkeley, Calif. 94720
CHAPIN, Gene L., Duke Univ., Dept. of Econ., Durham, N.C. 27706
CHAPIN, Ned, 1190 Bellair Way, Menlo Park, Calif. 94026
CHAPLIN, Robert N., 4440 Laguna Pl., No. 207, Boulder, Colo. 80302
CHAPMAN, Gordon R., 707 A St., N.E., Washington, D.C. 20002
CHAPMAN, J. Frank, S. Carolina Nat. Bk., P.O. Box 300, Columbia, S.C. 29202
CHAPMAN, Jan L., 2309 55th St., Lubbock, Tex.
CHAPMAN, (Mrs.) Jane R., 707 A St., N.E., Washington, D.C. 20002
CHAPMAN, (Mrs.) Janet G., 217 McCully St., Pittsburgh, Pa. 15216
CHAPMAN, John H., Jr., West Virginia Univ., Regional Res. Inst., Morgantown, W. Va. 26506
CHAPMAN, John M., Columbia Univ., Grad. Sch. of Bus., New York 27, N.Y.
CHAPPELLE, Daniel E., 4433 N. E. Failing St., Portland, Oregon 97213
CHAREAU, Walter J., 2653 N. Moreland Blvd., Shaker Heights, Ohio 44120
CHARLE, Edwin G., Jr., Ohio Univ., Dept. of Econ., Athens, Ohio 45701
CHARLES, Albert W., 5410 Riveria Way, Torrance, Calif.
CHARLES, K. J., Dept. of Econ. and Soc., University of Manitoba, Winnipeg, Canada
CHARLES, Raymond A., Bond Dept., Prudential Ins. Co. of Amer., Newark 1, N.J.
CHARLTON, Richard E., Jr., Schoolcraft Col., 18600 Haggerty Rd., Livonia, Mich. 48151
CHARLTON, Richard T., 1100 Terminal Sales Bldg., Portland 5, Oregon
CHAROUS, Arthur A., Sears Roebuck and Co., D/702R, Div. of Econ. Res., 2000 Spring Rd., Oak Brook, Ill. 60523
CHARTENER, William H., 4 North Ct., Port Washington, N.Y. 11050
CHASE, Arnold E., Bur. of Labor Statis., U.S. Dept. of Labor, Washington 25, D.C.
CHASE, Henry W., 34 E. Putnam Ave., Greenwich, Conn. 06830
CHASE, John W., 9447 Mary Glen Dr., Crestwood, Mo. 63126
CHASE, Richard X., 7-U Research Rd., Greenbelt, Md.
CHASTAIN, E. D., Jr., Auburn Univ., Dept. of Econ., Auburn, Ala. 36830
CHATOV, Robert, 2222 Fuller Rd., Ann Arbor, Mich.
CHAU, John Tin-wing, US AID, APO 152, San Francisco 96352
CHAU, (Mrs.) Maria L., US AID, APO 152, San Francisco 96352
CHAU, Yau Pik, Illinois Wesleyan Univ., Bloomington, Ill.
CHAUDHRY, Mahinder D., Royal Military Col. of Canada, Kingston, Ont., Canada
CHAUDHURI, Tarapada, Indian Aid Mission, Katmandu, Nepal
CHAUMONT, Paul, Societe Generale de Belgique 3, Montagne du Parc, Brussels, Belgium
CHAVES, Fernando, 3325 Pensa Dr., Falls Church, Va. 22041
CHAVEZ, Carlos, Edificio Herrera, Guatemala, Guatemala
CHAWLA, Muhammad R., 12/113 Maqbolabad, Alamgir Rd., Karachi 5, Pakistan
CHAWNER, Lowell J., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
CHEIT, Earl F., Univ. of California, 201 Calif. Hall, Berkeley 4, Calif.
CHEN, C. T., 4550 Connecticut Ave., Apt. 302, Washington, D.C. 20008
CHEN, Chung Wei, Xerox Corp., P.O. Box 1540, Webster #12, Rochester, N.Y. 14603
CHEN, David Y., P.O. Box 1877, University Park, N. Mex. 88070
CHEN, Dean Tin, 3316 10th St., East Moline, Ill. 61244
CHEN, Fu-Sen, Dept. of Econ., Univ. of Manitoba, Winnipeg, Man., Canada
CHEN, Gordon K. C., Univ. of Massachusetts, Amherst, Mass.
CHEN, Henry C., Col. of Bus. Admin., Univ. of Houston, Houston 4, Tex.
CHEN, Kuan I., State Univ. of New York at Albany, Albany, N.Y. 12203
CHEN, Samuel C., 715 Lake Blvd., Bemidji, Minn. 56601
CHEN, Yung-Ping, Univ. of California, Econ. Dept., Los Angeles, Calif. 90024
CHENERY, Hollis B., 1010 Memorial Dr., Cambridge, Mass. 02138
CHENEY, Robert J., Boston Col., Chestnut Hill, Mass. 01012
CHENG, Chu-yuan, Center for Chinese Studies, Lane Hall, The Univ. of Michigan, Ann Arbor, Mich. 48104
CHENG, Hang-Sheng, Iowa State Univ., Dept. of Econ., Ames, Iowa
CHENG, Pao Lun, Sch. of Bus. Admin., Univ. of Mass., Amherst, Mass.
CHENKIN, Alvin, 103 E. Tryon Ave., West Englewood, N.J. 08092
CHENOWETH, R. T., U.S. Military Academy, West Point, N.Y. 10996
CHEN-YOUNG, Paul L., 6501 14th St., N.W., Washington, D.C. 20012
CHERIAN, Edward J., Logistics Mgt. Inst., 4900 Massachusetts Ave., N.W., Washington, D.C. 20016
CHERINGTON, Charles R., 94 Brattle St., Cambridge, Mass. 01922
CHERINGTON, Paul W., 9 Coolidge Hill Rd., Cambridge, Mass. 01922
CHERNEFF, Robert V., 6341 N.E. Radford Dr., Seattle, Wash. 98115
CHERNICK, Jack, Inst. of Mgmt., Winants Hall, Rutgers Univ., New Brunswick, N.J.
CHERNICK, Sidney E., c/o Econ. Council of Canada, P.O. Box 527, Ottawa, Ont., Canada
CHERNOFSKY, (Mrs.) Ruth G., 1420 Carroll St., Brooklyn 13, N.Y.
CHERRINGTON, Homer V., 1 Strouds Run Rd., Athens, Ohio
CHERRY, Richard T., Div. of Bus. Admin., Agric. and Mechanical Col., College Sta., Tex.
CHERRY, Robert W., U.S. Dept. of Agric., Dairy Div., C and MS, Washington, D.C. 20250
CHERRY, Ronald L., 1830 Mifflin St., Huntingdon, Pa. 16652
CHERRY, Russell C., Jr., Texas Agric. and Mechanical Univ., Col. of Agric., College Sta., Tex.
CHESKIN, Irving, 189 Cleveland Dr., Croton-On-Hudson, N.Y. 10520
CHESKY, Edward J., Amer. Consulate, Kadnna, Nigeria
CHESLER, Herbert A., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh 13, Pa.
CHESSLER, David, 573 New Jersey Ave., Brooklyn, N.Y. 11207
CHESTER, Arthur D., 317 Summit St., Marquette, Mich. 49855
CHESTER, Buford, McCall Corp., McCall St., Dayton, Ohio
CHETTY, V. K., Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee, Wis. 53211
CHEUNG, Ng-Sheong, California State Col., Dept. of Econ., Long Beach, Calif. 90804
CHEW, David, Univ. of Singapore, Econ. Dept., Bukit Timah Rd., Singapore 10, Malaya
CHEYNEY, William J., 1411 K St., N.W., Suite 702, Washington 5, D.C.
CHHEDA, Kokila G., 134 Princeton, Apt. 3, Buffalo, N.Y. 14226

CHI, Wan-Fu, Dept. of Econ., Southern Illinois Univ., 701 W. Mill, Carbondale, Ill.
CHIANG, Alpha C., Univ. of Connecticut, Dept. of Econ., Storrs, Conn. 06268
CHIANG, You Keng, Wilkes Col., Wilkes-Barre, Pa.
CHICK, Victoria, Univ. Col., Dept. of Polit. Econ., Gower St., London W.C. 1, England
CHICKLIS, Theodore C., 2515 Forest Glen Rd., Silver Spring, Md.
CHILD, Arthur J. E., 402 Bate Crescent, Saskatoon, Sask., Canada
CHILD, Frank C., Econ. Dept., Univ. of California, Davis, Calif. 95616
CHILD, Robert W., 17332 Sunburst St., Northridge, Calif.
CHILDERS, Howard R., Jr., 921 W. Ildereen St., Springfield, Mo. 65804
CHILDS, Frank E., U.S. Naval Postgraduate Sch., Dept. of Econ., Monterey, Calif. 93940
CHILDS, Gerald L., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
CHILDS, James B., 1221 Newton St., N.E., Washington, D.C. 20017
CHILTON, Werner L., 21919 Snow Ave. C 4, Dearborn, Mich.
CHIN, Rockwood, Wheaton Col., Dept. of Econ., Norton, Mass. 02766
CHINGOS, E. George, Chingos and Sons, 818-20 America Ave., New York 1, N.Y.
CHINITZ, Benjamin, 6308 Kenhowe Dr., Bethesda, Md. 20034
CHIPMAN, John S., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
CHIRMAN, Michael, Direction du Plan, BP 365, Ouagadougou, Upper Volta, Africa
CHIROVSKY, Nicholas L., 292 Eastern Pkwy., Newark 6, N.J.
CHISHOLM, John W., Louisiana State Univ., Col. of Com., Baton Rouge 3, La.
CHISHOLM, Roger K., Grad. Sch. of Bus., 339 E. Chicago Ave., Chicago, Ill. 60611
CHISM, Neal A., 304 S. 52nd St., Lincoln, Nebr. 68510
CHISWICK, Barry R., 610 W. 115th St., Apt. 4, New York, N.Y. 10025
CHITTLE, Charles R., Bowling Green State Univ., Dept. of Econ., Bowling Green, Ohio 43402
CHITWOOD, John C., Jr., 1127 W. 76th Ter., Kansas City, Mo.
CHIU, John Shih-Yao, Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
CHO, Byung T., Mississippi State Univ., Col. of Bus. and Ind., P.O. Box 661, State College, Miss. 39762
CHO, Jae Hong, 1815 Winston Dr., South Bend, Ind. 46635
CHO, Le Van, 36 Huynh Thuc Khang, Saigon, Vietnam
CHOATE, Ronald F., R. R. #2, Carterville, Ill. 62918
CHOGUILL, Charles L., 134 Abbey Ter., Drexel Hill, Pa. 19026
CHOI, Joo Whan, 425 8th Ave., S.E., Minneapolis, Minn. 55414
CHOI, Kyou E., Louisiana State Univ., Dept. of Econ., New Orleans, La. 70122
CHOI, Paul W., 553 E. 25th St., Paterson, N.J.
CHON, Myong Che, P.O. Box 2542, Stanford, Calif. 96128
CHOO, Hakchung, Clark Univ., Dept. of Econ., Worcester, Mass. 01610
CHOU, Frederick W., 1220 N. Pierce St., Arlington, Va. 22209
CHOU, Shun-Hasin, Dept. of Econ., Univ. of Pittsburgh, Pittsburgh 13, Pa.
CHOU, Tao-hsiung, IBM World Trade Corp., 2401-05 Hilton Hotel, 2A Queens Rd. C., Hong Kong
CHOU, Victor, Fin. Div. Box 512, Allis Chalmers Mfg., Milwaukee 1, Wis.
CHOU, Yu-Min, Univ. of Minnesota, Morris, Div. of Soc. Sci., Morris, Minn.
CHOUGH, Soon, Univ. of New Hampshire, Whittemore Sch., Durham, N.H. 03824
CHOW, Gregory Chi-chong, Thomas J. Watson Res. Center, P.O. Box 218, Yorktown Heights, N.Y. 10598
CHRETIEN, Paul J., Chevy Chase, 8807 Altimont Lane, Washington, D.C.
CHRISMAN, R. G., Eastern Kentucky State, Richmond, Ky.
CHRIST, Carl F., Dept. of Polit. Econ., Johns Hopkins Univ., Baltimore 18, Md.
CHRIST, Roger D., 489 North St., Elgin, Ill.
CHRISTELOW, Allan, 3 Toquam Rd., New Canaan, Conn.
CHRISTELOW, (Mrs.) Dorothy B., 3 Toquam Rd., New Canaan, Conn.
CHRISTENSEN, Kenneth C., 245 Market St., Rm. 214, San Francisco 6, Calif.
CHRISTENSEN, Laurits R., Univ. of California, Inst. of Bus. and Econ. Res., Berkeley, Calif. 94720
CHRISTENSON, Carroll L., 213 S. Bryan Ave., Bloomington, Ind.
CHRISTENSON, Charles J., Harvard Bus. Sch., Soldiers Field, Boston, Mass. 02163
CHRISTENSON, Donald D., Univ. of Wichita, Wichita, Kans. 67208
CHRISTGAU, Catherine E., 1545 18th St., N.W., Apt. 911, Washington, D.C. 20036
CHRISTIAN, David B., 1759 Penfield Rd., Penfield, N.Y. 14526
CHRISTIAN, James W., 1328 Wilson, Ames, Iowa
CHRISTIAN, Virgil L., Jr., Univ. of Kentucky, Col. of Com., Lexington, Ky.
CHRISTIAN, W. E., Jr., 5010 Willow Bend Blvd., Houston 35, Tex.
CHRISTIANSEN, Martin K., Univ. of Minnesota, T.S.H., St. Paul Campus, St. Paul 1, Minn.
CHRISTIE, George A., Jr., 251 Lynn Dr., Franklin Lakes, N.J. 07417
CHRISTIE, George N., 65 Nassau Rd., Great Neck, N.Y.
CHRISTIE, H., British Embassy, Washington, D.C.
CHRISTNER, George E., Jr., Beasley Industries, Inc., 975 W. Third Ave., Columbus, Ohio 43212
CHRISTOFFEL, William G., 201 La Forte Ave., Syracuse 7, N.Y.
CHRISTOPHER, Albert, Univ. of Maine in Portland, 96 Falmouth St., Portland, Me.
CHRISTOPHER, Benjamin B., White Stone, Va. 22578
CHRISTOU, George C., North Carolina State Univ., Dept. of Agric. Econ., Raleigh, N.C.
CHRISTY, George A., North Texas State Univ., Sch. of Bus. Admin., Denton, Tex.
CHRON, Gustav N., 7110 N. McAlpin Ave., Chicago, Ill. 60646
CHRYSSOPOULOS, Harry J., Mitropoleos 48, Athens 127, Greece
CHU, Finley M., Ouachita Baptist Col., Dept. of Econ., Arkadelphia, Ark.
CHU, Kwang-wen, 683 Gayley Ave., Apt. 14, Los Angeles, Calif. 90024
CHUCKER, Harold, Minneapolis Star, 425 Portland Ave., Minneapolis, Minn. 55415
CHUDSON, Walter A., 11 Pryer Lane, Larchmont, N.Y. 10538
CHUMLEY, (Mrs.) Toledo W., 5846 Sherrier Pl., N.W., Washington, D.C. 20016
CHUNG, A. M., 120 Deerfield Rd., Broomall, Pa. 19008
CHUNG, Joseph H. S., L'Univ. de Sherbrooke, Sherbrooke, Que., Canada
CHUNG, Joseph S., Illinois Inst. of Tech., Dept. of Bus. and Econ., Chicago, Ill. 60616
CHUNG, Pham, Univ. of New Mexico, Dept. of Econ., Albuquerque, N. Mex.
CHUNG, William Kuei-yong, Denison Univ., Dept. of Econ., Granville, Ohio 43023
CHUNG, Yong Bong, 5060 W. 12th St., Los Angeles, Calif. 90019
CHUNG, Young-Iob, 1132 Monocacy St., Bethlehem, Pa. 18018
CHURCHILL, Anthony A., Inter. Bk., 1818 H St., N.W., Washington, D.C. 20433
CHURCHILL, Gilbert A., Jr., Banta Apts. F-125, Bloomington, Ind.
CHURCHILL, Mary E., 615 E. 14th St., Apt. 8A, New York, N.Y. 10009
CHUTE, Aaron H., 5000 Shoal Creek Blvd., Austin, Tex. 78756
CHYTROWSKI, Allan M., 66-01 Burns St., Apt. 2-BB, Forest Hills, N.Y. 11375

CIESLA, Casimir, 314 Twining Rd., Oreland, Pa. 19075
CINOSI, Nicholas, 126 N. Montgomery St., Walden, N.Y.
CIOCCO, Frank P., 86-45 St. James Ave., Elmhurst, Queens, N.Y. 11373
CIOLFI, Orlando, 390 East 153rd St., Bronx, N.Y. 10455
CIOPPA, Joseph, 144 Swannekin Rd., Blauvelt, N.Y. 10913
CIOSEK, Carl P., 7911 Roswell Dr., Shrevewood, Falls Church, Va. 22043
CIRESE, Robert C., 910 Bancroft Way, Berkeley, Calif. 94710
CIST, Frank, Box 325, Brewster, Mass. 02631
CLAGUE, Christopher, 124 Sherman St., Cambridge, Mass. 02140
CLAGUE, Ewan, 3821 Woodley Rd., Washington, D.C. 20016
CLAIBORN, Edward L., DFE, U.S. Air Force Academy, Colo. 80840
CLAIR, Robert D., P.O. Box 155, Allendale, N.J. 07401
CLARAMUNT, Ana M., Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
CLARE, Kenneth, Westwood Res., Inc., Suite 1130 Kirkeby Center, 10889 Wilshire Blvd., Los Angeles, Calif. 90024
CLARENBACH, Fred A., 5009 Risser Rd., Madison, Wis. 53705
CLAREY, William A., 1109 W. Moss, Peoria, Ill.
CLARK, Carolyn, 2317 C Carleton St., Berkeley, Calif. 94704
CLARK, Charles T., Box 7668, Univ. Sta., Austin, Tex. 78712
CLARK, Clifford D., New York Univ., Grad. Sch. of Bus. Admin., 100 Trinity Pl., New York, N.Y. 10003
CLARK, Clifford H., Rocky Mountain Col., Dept. of Econ. and Bus., Billings, Mont.
CLARK, David H., Univ. of Maine, Sch. of Bus. Admin., Orono, Me. 04473
CLARK Eugene, 414 Garfield St., Pullman, Wash.
CLARK Floyd B., South Sta. 5766, College Station, Tex.
CLARK, Hamilton L., 8600 Lorraine Dr., Hinsdale, Ill. 60521
CLARK, Harold F., 718 Ridgemont Ave., San Antonio, Tex.
CLARK, M. Gardner, Cornell Univ., New York State Sch. of Ind., Labor Rela., Ithaca, N.Y.
CLARK, O. Morrell, 1303 37th St., Ogden, Utah 84403
CLARK, Paul G., Cluett Dr., Williamstown, Mass. 01267
CLARK, Peter B., 56 Pearl St., Cambridge, Mass. 01922
CLARK, Peter B., Univ. of California, Dept. of Econ., Los Angeles, Calif. 90024
CLARK, Peter D., 1527 Pierre Ave., Windsor, Ont., Canada
CLARK, Ralph N., 32 Longview Dr., Longmeadow, Mass. 01460
CLARK, Robert L., 3809 47th St., N.W., Washington, D.C. 20016
CLARK, Ronald J., Univ. of Wisconsin, Dept. of Agric. Econ., 310 King Hall, Madison, Wis. 53706
CLARK, Samuel L., P.O. Box 1049, Berkeley 1, Calif.
CLARK, Thomas D., 12302 Shadow Lane, Bowie, Md. 20715
CLARK, Wayne W., Dept. of Econ., Brigham Young Univ., Provo, Utah 84601
CLARK, William A., 31298 Oakhill Way, Hayward, Calif. 94544
CLARKE, Edwin C., 3221 6th Ave., Beaver Falls, Pa. 15010
CLARKE, Richard L., III, 128 W. 10th, Claremont, Calif. 91711
CLARKE, Stephen V. O., Fed. Res. Bank of N.Y., For. Res. Div., Fed. Res. P.O. Sta., New York 45, N.Y.
CLARKSON, Geoffrey P., School of Ind. Mgmt., Massachusetts Inst. of Tech., Cambridge, Mass. 01922
CLARKSON, Kenneth W., Univ. of California, Inst. of Gov. and Public Aff., Los Angeles, Calif.
CLAROS, Jose C., Tariff Bd., Ottawa, Ont., Canada
CLAUNCH, (Mrs.) Patty G., Sch. of Bus. Admin., Univ. of Massachusetts, Amherst, Mass.
CLAUNCH, Sidney J., Jr., Sch. of Bus. Adm., Univ. of Massachusetts, Amherst, Mass.
CLAWSON, C. Joseph, 1349 Verdugo Blvd., La Canada, Calif. 91011
CLAY, Arthur T., Jr., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
CLAYCAMP, Henry J., Jr., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif.
CLAYTON, Elizabeth M., 5521 25th, N.E., Seattle, Wash. 98105
CLAYTON, Frank A., 110 Woodridge Crescent, Apt. 10, Ottawa 14, Ont., Canada
CLAYTON, John E., 6909 Arbor Lane, McLean, Va. 22101
CLEARY, Donald P., 2047 Jackson Ave., Apt. 305, Ann Arbor, Mich.
CLEAVER, George H., 21 Maxwell Ct., Huntington, L.I., N.Y.
CLEAVER, Joe M., 5914 89th Ave., Carrollton, Hyattsville, Md. 20784
CLELAND, Sherrill, Dept. of Econ., Kalamazoo Col., Kalamazoo, Mich.
CLEM, Charles S., Jr., 412 W. King St., Strasburg, Va.
CLEMEN, Rudolf A., 26 Lilac Lane, Princeton, N.J. 08540
CLEMENCE, Richard V., 61 Beverly Rd., Wellesley, Mass. 02181
CLEMENS, Eli W., Col. of Bus. Admin., Univ. of Maryland, College Park, Md.
CLEMENS, Keith M., 1929 1/2 Rose St., Berkeley, Calif. 94709
CLEMENT, Charles D., 150 Pineview Dr., Athens, Ga.
CLEMENT, Meredith O., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
CLEMENTE, Leopoldo M., 21 W. Goethe St., Apt. 7-J, Chicago, Ill. 60610
CLEMENTE, (Mrs.) Lilia C., 21 W. Goethe St., Apt. 7-J, Chicago, Ill. 60610
CLENDENIN, John C., P.O. Box 96, Sparta, Ill. 62286
CLESSENS, Paul, Brechtsebaan 87, Schoten (Antwerp), Belgium
CLEVELAND, Alfred S., Spring Hill Manor, Spring Hill Lane, Chevy Chase, Md. 21620
CLEVELAND, Frederick W., Stevens Inst. of Tech., Hoboken, N.J. 07030
CLEVELAND, James G., 812 S. Arlington Hill Dr., Arlington 4, Va.
CLEVELAND, William C., Indiana Univ., Dept. of Econ., Bloomington, Ind. 47405
CLEVENGER, Horace M., 21 Almeda Pl., Ferguson, Mo. 63135
CLEWETT, Richard M., Sch. of Bus., Northwestern Univ., Evanston, Ill.
CLEWETT, Robert L., Pennsylvania State Univ., Boucke Bldg., Rm. 230, University Park, Pa. 16802
CLICKNER, Edwin K., 7513 Wellesley Dr., College Park, Md. 20740
CLIFFORD, A. Jerome, John Carroll Univ., Cleveland, Ohio 44118
CLIFTON, Charles W., Univ. of Michigan, Rm. 3084, Inst. for Soc. Res., Ann Arbor, Mich.
CLINE, Denzel C., 134 Kensington Rd., East Lansing, Mich.
CLODIUS, Robert L., 3435 Edgehill Pkwy., Madison 5, Wis.
CLONES, Angelos T., 1931 N. Cleveland St., Apt. 601, Arlington 1, Va.
CLONES, (Mrs.) Julia, 1931 N. Cleveland St. Apt. 601, Arlington 1, Va.
CLOOS, George W., 2690 Forest Ct., Deerfield, Ill. 60015
CLOSE, Francis A., 356 Pennell Circle, Apt. 5, Alumni Village, Tallahassee, Fla.
CLOVER, Vernon T., 6114 Kenosha Drive, Lubbock, Tex. 79413
CLOWER, Robert W., Dept. of Econ., Northwestern Univ., Evanston, Ill.
CLOYD, Helen M., 1410 Maddox Dr., Muncie, Ind. 36150
CLUTE, Lauren V., St. Lawrence Univ., Dept. of Econ., Canton, N.Y. 13617

CLYMAN, Bernard, 18 Oaks Hunt Rd., Lake Success, Great Neck, N.Y.
COALE, Ansley J., Off. of Population Res., Princeton Univ., Princeton, N.J. 08540
COALE, Charles W., Jr., Pennsylvania State Univ., Dept. of Agric. Econ., 110 Weaver Bldg., University Park, Pa. 16801
COASE, Ronald H., Univ. of Chicago Law Sch., Chicago, Ill. 60637
COATS, A. W., Univ. of Nottingham, Dept. of Econ. History, University Park, Nottingham, England
COATS, Warren L., Jr., 6752 S. Oglesby, #3, Chicago, Ill.
COBB, Arthur L., 27-C Claymont Apts., Tuscaloosa, Ala.
COBB, T. C., 706 Snyder Rd., East Lansing, Mich.
COBERN, Morris, 1311 Delaware Ave., S.W., Apt. 736, Washington 24, D.C.
COBLE, J. A., Socony Oil Co., Inc., Mobile Oil Div., Econ. Dept., 150 E. 42nd St., New York 17, N.Y.
COBRIN, Harry A., c/o Clothing Mfrs. Asso. of U.S.A., 135 W. 50th St., Suite 1935, New York, N.Y. 10020
COCHRAN, John A., Arizona State Univ., Dept. of Econ., Tempe, Ariz. 85281
COCHRAN, Kendall P., North Texas State Col., P.O. Box 5172, Denton, Tex.
COCHRANE, Donald, Monash Univ., Dept. of Econ., Clayton, Vic., Australia
COCHRANE, Willard W., 2225 Folwell St., St. Paul, Minn. 55108
COCKS, Douglas L., 3515 Douglas Ave., Des Moines, Iowa 50310
CODDING, Robert E., 325 Ottawa St., Washington, D.C. 20021
CODY, Peter M., U.S. AID, c/o Amer. Embassy, APO San Francisco 96352
COE, Robert K., Virginia Poly. Inst., Col. of Bus., Dept. of Bus. Admin., Blacksburg, Va. 24061
COELHO, Philip R. P., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
COEN, Edward, Univ. of Minnesota, Dept. of Econ., Minneapolis 14, Minn.
COEN, (Mrs.) Margery C., Stanford Univ., Dept. of Econ., Stanford, Calif. 94305
COEN, Robert M., Stanford Univ., Dept. of Econ., Stanford, Calif. 94305
COFFEY, (Mrs.) Ann H., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore, Md. 21218
COFFEY, Joseph D., Univ. of California, Berkeley, Dept. of Agric. Econ., Berkeley, Calif.
COFFMAN, Maurice F., 20800 Clare Ave., Maple Heights, Ohio 44255
COGAN, L. Peter, 750 Park Ave., New York, N.Y.
COGEN, Charles, 535 N. Michigan Ave., Apt. 3112, Chicago, Ill. 60611
COHAN, Avery B., Sch. of Bus. Admin., Univ. of North Carolina, Chapel Hill, N.C.
COHAN, Hugo E., Santa Fe 1455, Rosario, Argentina
COHANY, Harry P. 3706 Astoria Rd., Kensington, Md. 20795
COHEN, Alvin, Lehigh Univ., Dept. of Econ., Bethlehem, Pa. 18015
COHEN, Arnold A., Int. Bus. Machines Corp., Old Orchard Rd., Armonk, N.Y. 10504
COHEN, Benjamin J., Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
COHEN, Benjamin I., 327 I St., S.W., Washington, D.C. 20024
COHEN, Bert R., 1436 Butler, Apt. 9, Los Angeles, Calif. 90025
COHEN, Daniel, 12916 Matey Rd., Silver Spring, Md.
COHEN, Harold A., Univ. of Georgia, Col. of Bus. Admin., Athens, Ga.
COHEN, Irving, 3505 Decatur Ave., Bronx, N.Y. 10467
COHEN, Jacob, Univ. of Pittsburgh, Dept. of Econ., Pittsburgh, Pa. 15213
COHEN, Jerome B., 135 E. 74th St., Apt. 10A, New York 21, N.Y.
COHEN, Jon S., 2419 Ashby Ave., Berkeley, Calif. 94705
COHEN, Kalman J., Grad. Sch. of Ind. Admin., Carnegie Inst. of Tech., Pittsburgh 13, Pa.
COHEN, Lawrence B., Columbia Univ., 302 S. W. Mudd, New York 27, N.Y.
COHEN, Leo, Southern Illinois Univ., Edwardsville, Ill. 62025
COHEN, Martin A., 55 Valley View Rd., Great Neck, N.Y.
COHEN, Morris, 411 Churchill Rd., West Englewood, N.J. 07666
COHEN, Neil G., 110 W. Rillito, Tucson, Ariz.
COHEN, Paul C., 639 Lincoln Ave., Winnetka, Ill.
COHEN, Sanford, Univ. of New Mexico, Dept. of Econ., Albuquerque, N.M.
COHEN, Sol D., 6540 170th St., Flushing 65, N.Y.
COHEN, Wilbur J., 9819 Capitol View Ave., Silver Spring, Md.
COHN, Elchanan, 131 N. Hyland Ave., Ames, Iowa 50012
COHN, Samuel, Bur. of Budget, Off. of Budget Rev., Washington 25, D.C.
COHN, Stanley H., 4201 Mass. Ave. N.W., Apt. 4028 C., Washington 16, D.C.
COHN, Walter I., 824 New Hampshire Ave., N.W., Apt. 612, Washington, D.C. 20037
COIT, Eleanor G., 69 Fifth Ave., Apt. 10J, New York 3, N.Y.
COKER, Robert C., Dept. of Econ., Indiana Central Col., Indianapolis 27, Ind.
COLACO, Francis X., 2510 Bancroft Way, Apt. #211, Berkeley, Calif. 94704
COLANTUONI, Ido E., 1616 18th St. N.W., Apt. 508, Washington 9, D.C.
COLBERG, Marshall R., Dept. of Econ., Florida State Univ., Tallahassee, Fla.
COLBURN, Lawrence D., 36 Via San Dimas, Fremont, Calif. 94538
COLBY, William G., Jr., 444 E. 66th St., New York, N.Y. 10021
COLDWELL, Philip, Fed. Res. Bk. of Dallas, Res. Dept., Dallas, Tex.
COLE, Arthur H., 15 Hemlock Rd., Cambridge, Mass. 01922
COLE, (Mrs.) Belle L., 62 Richardson Rd., Kensington, Calif. 94707
COLE, Charles L., 324 Park Ave., Long Beach 14, Calif.
COLE, David C., USOM APO 301, San Francisco 96301
COLE, David M., 300 Fern Oak Ct., McLean, Va. 22101
COLE, Donald P., Ohio State Univ., 306 Hagerty Hall, Columbus, Ohio
COLE, George S., 107 Smith Lane, Apt. 1-A, Syracuse, N.Y. 13210
COLE, Harrison W., Johns Manville Co., 22 E. 40th St., New York 16, N.Y.
COLE, Lawrence P., 216 Russell St., West Lafayette, Ind. 47178
COLE, Robert E., Washington State Univ., Dept. of Econ., Pullman, Wash.
COLE, Rosanne E., Nat. Bur. of Econ. Res., 261 Madison Avenue, New York 16, N.Y.
COLE, William E., Univ. of Tennessee, Dept. of Econ., Knoxville, Tenn.
COLEBERD, Robert E., Jr., Western Maryland Col., Westminster, Md. 21157
COLELLA, Francis J., 33-40 58th St., Woodside, N.Y. 11377
COLEMAN, Bruce P., Michigan State Univ., Dept. of Mgmt., Sch of Bus., 230 Eppley Center, East Lansing, Mich. 48823
COLEMAN, George W., Amer. Banking Asso., 90 Park Ave., New York, N.Y. 10016
COLEMAN, John R., Ford Found., Prog. in Econ. Dev. and Admin., 477 Madison Ave., New York, N.Y. 10022
COLEMAN, Raymond W., Univ. of Illinois, Col. of Bus. Admin., Chicago, Ill. 60611
COLGAN, Richard G., 126 Grand Ave., Staten Island, N.Y. 10301
COLIN, David H., R. D. 1, Centre Rd., Staatsburg, N.Y. 12580
COLLACOTT, R. H., Joseph Dyson and Sons, Inc., 33300 Lakeland Blvd., Eastlake, Ohio 44095
COLLADO, Emilio G., 435 E. 52nd St., Apt. 4-B, New York, N.Y. 10022
COLLAZZO, Charles, 2360 Massachusetts Ave., Lexington, Mass. 02173

COLLERY, Arnold, 17 Walnut St., Amherst, Mass. 01002
COLLIER, Boyd D., 1819 Mitchell, Waco, Tex. 76708
COLLIER, David S., 154 E. Superior St., Chicago, Ill. 60611
COLLIER, Robert P., 926 N. 15th E., Logan, Utah 84321
COLLINS, (Mrs.) Claire R., 2 Legend Lane, Westbury, N.Y.
COLLINS, James C., Jr., Southern Methodist Univ., Box 317, Dallas, Tex. 75222
COLLINS, James E., 1155 Santa Cruz Ave., Menlo Park, Calif. 94025
COLLINS, Lora S., 19 Pierrepont St., Brooklyn 1, N.Y.
COLLINS, Robert T., Univ. of South Carolina, Dept. of Econ., Columbia, S.C. 29208
COLLINS, Victoria J., 719 Neale Ave., S.W., Massillon, Ohio 44646
COLLINS, William H., Jr., c/o Mrs. Throgmorton, 203 S. Dixon St., Carbondale, Ill. 62903
COLLLINS, William J., Kahl Home, 1101 W. 9th St., Davenport, Iowa 52802
COLLINSON, William J., 88 Adams St., Garden City, N.Y.
COLM, Gerhard, Nat. Planning Asso., 1606 New Hampshire Ave., N.W., Washington 9, D.C.
COLWELL, Billy J., Univ. of Texas, Dept. of Econ., BEOB 408, Austin 12, Tex.
COLWELL, Robert C., 422 Woodland Dr., Forest Heights, Md. 20021
COMANOR, William S., U. S. Dept. of Justice, Antitrust Div., Washington, D.C.
COMBS, Ernest F., 5322 N. 40th, Tacoma, Wash. 98407
COMEAU, Robert L., Dalhousie Univ., Dept. of Econ., Halifax, N.S., Canada
COMIEZ, Maynard S., 224 N. George Mason Dr., Arlington 3, Va.
COMINS, Harold N., 6 Park Ave., Wakefield, Mass. 01880
COMISH, Newel H., 226 W. 1st South, Logan, Utah 84321
COMITINI, Salvatore, Dept. of Bus. and Econ., Arlington State Col., Arlington, Tex.
COMPARINI, Sergio, Huerfanos 1117, 9° Piso, Santiago, Chile
COMPTON, Wilson M., 734 College Ave., Wooster, Ohio
COMSTOCK, Edward G., 55 Crows Nest Rd., Bronxville 8, N.Y.
CONANT, Eaton H., 8340 S. Kimbark Ave., Chicago, Ill. 60619
CONANT, Luthur, 46 Shepard St., Apt. 22, Cambridge, Mass. 01922
CONANT, Michael, Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
CONARD, John H., 543 W. Mifflin St., Madison, Wis. 53703
CONDLIFFE, John B., 257 Stanford Ave., Berkeley 8, Calif.
CONDOIDE, Mikhail V., 155 W. Beechwold Blvd., Columbus, Ohio 43214
CONDON, (Mrs.) Carol M., 390 Morris Ave., Apt. 9, Summit, N.J. 07901
CONDOS, Apostolos, Iowa State Univ., Dept. of Econ., Ames, Iowa
CONEFREY, Joseph B., Jr., Rua Padre Francisco, 20-6-E., Lisbon, Portugal
CONGRAM, Howard S., Route 5, Monticello, Ind. 47960
CONKLIN, Charles F., 209 S. Johnson, Ada, Ohio 45810
CONKLIN, George T., Jr., 201 Park Ave. S., New York, N.Y.
CONKLIN, Maxwell R., U. S. Dept. of Com., Bur. of the Census-Ind. Div., Washington 25, D.C.
CONKLIN, Vivian G., 4501 Arlington Blvd., Apt. 712, Arlington, Va. 22203
CONKLING, Roger L., Conkling Inc., 917 Executive Bldg., 811 S.W. 6th Ave., Portland, Ore.
CONLAN, Don R., Chemical Bk. N. Y. Trust Co., 20 Pine St., New York, N.Y. 10015
CONLEY, Bryan C., 796 Embarcadero Del Norte, #203, Goleta, Calif. 93017
CONLEY, Garland B., 3208 Porter St., Washington 8, D.C.
CONLEY, Inez M., 2704 30th S.E., Washington 20, D.C.
CONLEY, Ronald W., 2702 Plyers Mill Rd., Silver Spring, Md. 20902
CONLISK, John, Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison, Wis. 53707
CONN, Harlan D., St. Andrews Presbyterian Col., Laurinburg, N.C. 28352
CONNELL, Donald R., Omaha Univ., Dept. of Econ., Omaha, Neb. 68132
CONNOLLY, (Mrs.) Barbara Z., 1081 North Ave., New Rochelle, N.Y.
CONNOLLY, Thomas P., 2217 Amherst Rd., Lewisdale, Hyattsville, Md.
CONNOR, Colin H., Emmanuel Col., 400 The Fenway, Boston, Massachusetts 02115
CONNOR, Francis J., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
CONNOR, John, 6363 St. Charles, New Orleans 18, La.
CONNOR, Michael A., 69 10 Yellowstone Blvd., Forest Hills, N.Y. 12068
CONNORS, Robert G., 1114 Berkshire Rd., Grosse Pointe Park, Mich. 48138
CONOVER, Arthur G., 4129 Leland St., Chevy Chase, Md. 21620
CONOVER, Harry, 51 Logan Rd., New Canaan, Conn.
CONRAD, Alfred H., Harvard Univ., Grad. Sch. of Bus. Admin., Boston, Mass. 02163
CONRAD, William E. F., 4710 Ravensworth Rd., Annandale, Va. 22003
CONRADUS, Guenter M., Occidental Col., Dept. of Econ., Los Angeles, Calif. 90041
CONRATH, David W., 4736 Osage Ave., Philadelphia, Pa. 19143
CONROY, Robert S., 643 Rompart Ave., Webster Groves, Mo. 63119
CONSTANTACOPOULOS, George C., 130 Wadsworth Ave., New York 33, N.Y.
CONSTANTIN, James A., Univ. of Oklahoma, Dept. of Mktg., Norman, Okla. 70369
CONSTANTINE, Glezakos, Univ. of Southern California, Dept. of Econ., Los Angeles, Calif. 90007
CONSTAS, Kimon, 1912 E. Beverly Rd., Milwaukee, Wis. 53211
CONTOPOULOS, Nicholas C., Marshall Univ., Dept. of Econ., Huntington, W. Va.
CONVERSE, (Mrs.) Muriel J., 2515 Prairie St., Ann Arbor, Mich.
CONVERSE, Paul D., 17 Montclair Rd., Urbana, Ill.
CONVEY, Edwin H., Loyola Col., Baltimore 10, Md.
CONWAY, Lawrence V., 2019 Colfax St., Evanston, Ill.
CONWAY, Paul F., University of Notre Dame, Col. of Bus. Admin., Notre Dame, Ind. 46556
CONWELL, Ralph, Dept. of Econ. and Soc., Univ. of Wyoming, Laramie, Wyo.
CONYERS, Alex D., Morehead State Col., Box 705, Morehead, Ky. 40351
COOGAN, James F., 12 Whiting Rd., Wellesley Hills, Mass. 02182
COOK, Alvin A., Jr.,/2703 Boylston, E., Apt. 102, Seattle, Wash. 98102
COOK, Donald E., Ventura Col., 4667 Telegraph Rd., Ventura, Calif.
COOK, Edward J., 136 Arthur St., Garden City, L.I., N.Y. 11535
COOK, Franklin H., 325 W. Park Ave., State College, Pa. 16801
COOK, Gail C. A., 201 Maple Lane, Rockcliffe Park, Ottawa 2, Ont., Canada
COOK, Harry L., Whittier Col., Dept. of Econ., Whittier, Calif. 90608
COOK, James D., P.O. Box 1481 Main Off., Los Angeles, Calif. 90053
COOK, Jay D., Jr., Washington and Lee Univ., Lexington, Va.
COOK, Paul W., Jr., Harvard Bus. Sch., Soldiers Field, Boston 63, Mass.
COOK, Robert F., 517 Main St., Hellertown, Pa. 18055
COOK, Robert G., 1803 Hatton Dr., Columbia, Mo. 65201
COOK, Robert I., Tri-State Col., Angola, Ind 46703
COOK, Robert J., 6201 Vanguard, Garden Grove, Calif.
COOK, Robert W., Univ. of Delaware, Div. of Urban Affairs, Newark, Del. 19711

COOK, Roy C., 3370 Highwood Dr. S.E., Washington 20, D.C.
COOK, Theodore, 308 K Eagle Hts., Madison 5, Wis.
COOK, William D., 6032 S. Kimbark Ave., Chicago, Ill. 60637
COOK, William G., Marshall Univ., Dept. of Econ., Huntington, W.Va. 25701
COOKE, Blaine M., United Airlines, Mktg. Serv., P.O. Box 8800, Chicago, Ill. 60666
COOKE, Charles K., 5534 Haverhill Rd., Birmingham, Mich.
COOKE, Gilbert W., Bowling Green State Univ., Bowling Green, Ohio 43402
COOKE, Helen J., 20 Overlook Rd., Mountain Lakes, N.J. 07046
COOKE, John P., 1105 S. Elm St., Denver, Colo. 80222
COOKE, Laurence S., Michigan State Univ., Dept. of Soc. Sci., Bessey Hall, East Lansing, Mich.
COOKE, Ronald C., 207 Bayview Hgts. Dr., Toronto 17, Ont., Canada
COOKENBOO, Leslie, 725 Richfield Bldg., 555 S. Flower St., Los Angeles 17, Calif.
COOKSON, Forrest E., Jr., c/o Nat. Planning Asso., 1525 18th St., N.W., Washington, D.C. 20036
COOLEY, Oscar W., 219 West Highland Ave., Ada, Ohio 45810
COOLSEN, Frank G., 5905 Greenlawn Dr., Bethesda, Md. 21811
COONS, Arthur G., 1852 Campus Rd., Los Angeles, Calif.
COOPER, Charles A., RAND Corp., 2700 Main St., Santa Monica, Calif.
COOPER, David E., 308 W. Pearl, Mount Pleasant, Iowa 52641
COOPER, Edward N., 6207 Bannockburn Dr., Bethesda, Md. 20034
COOPER, George W., R.D.1., South Van Leaven Dr., Rensselaer, N.Y. 13680
COOPER, John D., Red Oaks Mill Rd., Poughkeepsie, N.Y. 12603
COOPER, Richard N., Box 1987, Yale Sta., New Haven, Conn.
COOPER, Ronald L., 286 Wilson St., Albany 10, Calif.
COOPER, William H., Bucknell Univ., Econ. Dept., Lewisburg, Pa. 17837
COOTNER, Paul H., Massachusetts Inst. of Tech., Sch. of Ind. Mgt., Cambridge, Mass. 01922
COPELAND, Arthur, 8311 Forrest Ave., Philadelphia, Pa. 19150
COPELAND, John, 2708 Cathedral Ave., N.W., Washington 8, D.C.
COPELAND, Marjorie P., Drake Univ., Col. of Liberal Arts, Des Moines, Iowa 50311
COPELAND, Michael D. G., 142 West End Ave., Apt. 30-L, New York, N.Y. 10023
COPELAND, Morris A., Univ. of Missouri, Sch. of Bus. and Pub. Admin., Dept. of Econ., Columbia, Mo.
COPELAND, P. J., 8511 Cherry Valley Lane, Alexandria, Va. 22309
COPELAND, William G., Mead Corp., 118 W. First St., Dayton, Ohio 45402
COPELAND, William L., 5618 Heming Ave., Springfield, Va. 22151
COPELLO, Antonio F., Asociacion Nacional de Industriales, Apartado Aereo No. 4430, Bogota, D.E., Colombia, S.A.
COPER, Rudolf, 106 McClung St., Pass Christian Isles, Miss. 39571
COPES, Parzival, Simon Fraser Univ., Dept. of Econ. and Com., Burnaby 2, B.C., Canada
COPITHORNE, L. W., 9660 82nd Ave., Edmonton, Alta., Canada
COPPEDGE, Robert O., 524 Sweet Ave., Las Cruces, N. M.
COPPOCK, Dennis John, 23 Davenport Park Rd., Stockprt, Cheshire, England
COPPOCK, Joseph D., Pennsylvania State Univ., Dept. of Econ., University Park, Pa. 16802
COPPS, John A., Dept. of Econ., Western Michigan Univ., Kalamazoo, Mich.
CORAGGIO, Jose L., 5014 Spruce St., Apt. C-6, Philadelphia, Pa. 19139
CORBACI, Leo M., 925 White Oak Dr., South Bend 17, Ind.
CORBEILLE, Jerome P., 4 Patrick Pl., Niantic, Conn. 06357
CORBETT, James F., 76-79th St., Brooklyn, N.Y.
CORBIN, Donald A., Dept. of Econ. and Bus., Univ. of Hawaii, Honolulu 14, Hawaii
CORBO, Mario, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
CORCORAN, Bobby N., Middle Tennessee State Univ., Dept. of Econ., Murfreesboro, Tenn.
CORDELL, Arthur J., 201 Eye St., S.W., #202, Washington, D.C. 20001
CORDEN, W. M., Australian Nat. Univ., Dept. of Econ., Sch. of Pacific Studies, Canberra, A.C.T., Australia
CORDERO, Rafael de J., RFD 2, Box 39, Beverly Hills, Rio Piedras, P.R. 00745
COREA, Gamini, Horton Lodge, Horton Pl., Colombo 7, Ceylon
COREY, C. Stanley, 573 Vine St., Kent, Ohio 44240
COREY, E. Raymond, Harvard Bus. Sch., Soldiers Field, Boston 63, Mass.
CORKHILL, Robert J., 115 E. Market St., Bethlehem, Pa.
CORKUM, Keith H., Jr., Worcester Junior Col., 766 Main St., Worcester, Mass. 01608
CORMIER, Gerard H., 25 E. Wayne Ave., Silver Spring, Md. 20901
CORNEHLS, James V., Teachers Col.-Columbia Univ., US/AID, Amer. Embassy, Lima, Peru
CORNELISSEN, Frederick C., Chicery-la-Ville, Yonne, France
CORNELL, (Mrs.) Nina W., 201 N. Busey, Urbana, Ill. 61801
CORNELL, Paul A., 9885 Charleville Blvd., Beverly Hills, Calif. 90210
CORNELL, Peter M., Econ. Council of Canada, P.O. Box 527, Ottawa, Ont., Canada
CORNELL, Robert A., 431 Riverside Dr., Apt. 6-C, New York, N.Y. 10025
CORNER, C. Richard, 35-C S. Fairway, Pullman, Wash.
CORNWALL, John, Econ. Dept., Tufts Univ., Medford, Mass. 02052
CORR, Arthur V., 39 Bradford St., New Providence, N.J. 07974
CORREA, Hector, Wayne State Univ., Dept. of Econ., Detroit, Mich.
CORRIE, George B., 501 Ridgewood Rd., Huntington, W.Va.
CORRIGAN, (Mrs.) Josephine M., 435 Beach 135th St., Belle Harbor, L.I., N.Y. 11694
CORRY, Bernard, Univ. of California, Berkeley, Dept. of Econ., Berkeley, Calif. 94720
CORSON, Walter H., II, 28 Huron Ave., Cambridge, Mass. 02138
CORTELYOU, Rushton G., 5109 Underwood Ave., Omaha 32, Neb.
CORTI, Leonard J., 20100 Wells Dr., Woodland Hills, Calif
CORTNEY, Philip, 12 Chateau Banquet, Geneva, Switzerland
COSSABOOM, Roger A., 349 Fiske St., Hollis-ton, Mass 01746
COSTA, Rubens, 8702 Bradmoor Drive, Bethesda, Md. 20034
COSTAFREDA-AMOROS, Marcelino, Calle Tuset, 30 ático 2a, Barcelona 6, Spain
COSTANZO, Samuel A., Amer. Embassy, APO New York 09675
COSTELLO, Martin J., 2300 N. Stafford St., Arlington 7, Va.
COSTELLO, Peter M., Smith Col., Dept. of Econ., Northampton, Mass.
COSTELLO, William E., 8432 Thames St., Springfield, Va. 22151
COSTIS, Harry G., St. Mary's Univ., San Antonio, Tex. 78228
COTTA, Edmund A., Long Beach State Col., 6101 E. 7th St., Long Beach 4, Calif.
COTTER, James E., 6532 Tucker Ave., McLean, Va. 22101
COTTER, J. Emmons, Norton Co., 1 New Bond St., Worcester, Mass. 01606
COTTER, Jerry J., 1418 Gordon St., #8, Redwood City, Calif. 94061

COTTER, Richard V., Univ. of Nevada, Col. of Bus. Admin., Reno, Nevada 89507
COTTERMAN, William W., Georgia State Col., Computer Center, 33 Gilmer St., Atlanta, Ga. 30303
COTTINGHAM, Phoebe, 1321 Spruce St., Berkeley, Calif. 94709
COUDSY, Albert, Centre d'Etudes Econ., 59 Rue Rowa, Damascus, Syria
COUFOS, Peter, 1361 86th St., Brooklyn, N.Y.
COUGHLAN, John A., Villanova Univ., Col. of Com. and Fin., Villanova, Pa. 19085
COULSON, John S., Leo Burnett Co., Prudential Plaza, Chicago, Ill. 60601
COULTER, (Mrs.) Elizabeth J., 1825 N. Lake Shore Dr., Chapel Hill, N.C.
COULTER, Kirkley S., 608 S. George Mason Dr., Arlington, Va.
COUPE, John D., Univ. of Maine, Dept. of Econ., Orono, Me. 04473
COOPER, Walter J., 192 Broadway, Apt. 4K, Dobbs Ferry, N.Y. 10522
COURCHENE, Tom J., Univ. of Western Ontario, Dept. of Econ., London, Ont., Canada
COURCHESNE, Morris J., 26 Russell St., Bellows Falls, Vt.
COURNOYER, Norman G., 60 Dana St., Amherst, Mass.
COURT, Andrew T., 1517 Iroquois, Detroit, Mich.
COURT, John C., E-31 Gallatin, Harvard Bus. Sch., Boston, Mass. 02163
COURTNEY, Sister Dorothea, 3825 W. Pine Blvd., St. Louis, Mo. 63108
COURTNEY, Fred I., 2103 Mimosa Dr., Greensboro, N.C.
COUTSOUMARIS, George P., Center of Econ. Res., 6 Amerikis St., Athens, Greece
COUTU, A. J., North Carolina State Univ., Dept. of Econ., 201 Patterson Hall, Raleigh, N.C.
COUZENS, Michael J., 23 Fremont House, Stern Hall, Stanford, Calif.
COVER, Virgil D., Syracuse Univ., 116 College Pl., Syracuse, N.Y. 13210
COVIN, Charles F., Florida State Univ., P.O. Box U-762, Tallahassee, Fla.
COWAN, Donald R. G., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
COWAN, Gary R., 1254 151st Ave., Bellevue, Wash. 98004
COWAN, Hardin W., 1206 W. 51st St., Kansas City, Mo. 64112
COWART, Joseph R., Virginia Military Inst., Dept. of Econ., Lexington, Va. 24451
COWDEN, Dudley J., 304 Country Club Rd., Chapel Hill, N.C.
COWIE, Peggy L., 3853 Holly Hills Blvd., St. Louis 16, Mo.
COWING, Thomas G., 2315 Parker, Apt. 3, Berkeley, Calif. 94704
COWLES, Alfred, Suite 2215, Tribune Tower, 435 N. Michigan Ave., Chicago, Ill. 60611
COWLES, Richard V., 229 Willowmere Dr., Des Moines, Iowa 50621
COWLING, Keith, Univ. of Manchester, Dept. of Econ., Manchester 13, England
COWNIE, John B., 725 Marguerita Ave., Santa Monica, Calif. 90402
COX, Albert H., Jr., 10416 Royal Club Lane, Dallas 29, Tex.
COX, Edwin B., Boston Univ., Col. of Bus. Admin., 685 Commonwealth Ave., Boston, Mass. 02215
COX, Garfield V., 415 W. 11th St., Claremont, Calif. 91711
COX, John R., Univ. of New Hampshire, Whittmore Sch. of Bus. and Econ., Durham, N.H. 03824
COX, (Mrs.) Judith B., c/o B. W. Bauernfeind, 6612 Cornelia Dr., Minneapolis 24, Minn.
COX, Oliver C., Lincoln Univ., Jefferson City, Mo.
COX, Paul J., 906 Forest Dr., Albemarle, N.C. 28001
COX, Reavis, 503 Walnut Lane, Swarthmore, Pa. 198081
COX, William A., Inst. for Defense Anal., 400 Army-Navy Dr., Arlington, Va.
COX, William E., Jr., Sch. of Bus., Western Reserve Univ., Cleveland 6, Ohio
COYLE, Eugene P., 7 Carol Ave., Apt. 10, Brookline, Mass. 01506
COYNE, Thomas J., 672 S. Hawkins Ave., Akron 20, Ohio
COZ, Richard T., Univ. of Santa Clara, Santa Clara, Calif. 95053
COZZENS, Albert B., 1200 Hanna Bldg., P.O. Box 6508, Cleveland, Ohio 44101
CRADDOCK, William J., Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa 50010
CRAFT, Rolf V., 1417 Hawthorn Apts., Ames, Iowa 50010
CRAFTON, (Mrs.) Helen P., Univ. of Michigan, Dearborn Center, 4901 Evergreen Rd., Dearborn, Mich.
CRAFTON, L. Joseph, Univ. of Michigan, Dearborn Center, 4901 Evergreen Rd., Dearborn, Mich.
CRAGIN, Charles C., P.O. Box 1011, El Paso, Tex.
CRAHAN, Marcus E., 453 S. Spring St., Suite 1241, Los Angeles, Calif. 90013
CRAIG, Alton W. J., 2651 Elmhurst St., Ottawa 14, Ont., Canada
CRAIG, (Mrs.) Eleanor D., 22 Vining Lane, Westhaven, Wilmington, Del. 19807
CRAIG, George D., Louisiana State Univ., Dept. of Bus. Fin. and Statis., Baton Rouge, La. 70803
CRAIG, John T., AID-U.S. Embassy, Mogadiscio, c/o Dept. of State, Washington, D.C. 20523
CRAIG, (Mrs.) Mary E., 4607 King William Rd., Richmond 25, Va.
CRAIG, Paul G., Ohio State Univ., Dept. of Econ., Columbus 10, Ohio
CRAIG, Victor I., Jr., 1 Abington Ave., Ardsley, N.Y. 10502
CRAINE, Roger, 10-B Laurel Hill Rd., Greenbelt, Md. 20770
CRAMER, Curtis A., 5105 Yorktown Blvd., Arlington 7, Va.
CRAMER, Dale L., Univ. of Alabama, Box 1973, University, Ala. 35486
CRAMER, Edison H., Route 1, Box 120, Davidsonville, Md. 21035
CRAMP, A. B., The University, Dept of Econ., St. Leonard's Gate, Lancaster, England
CRAMPTON, Henry E., Amer. Tel. and Tel. Co., 195 Broadway, New York 7, N.Y.
CRANCH, Raymond G., Bryn Athyn, Montgomery County, Pa. 19009
CRANDALL, Burton B., State Univ. Agric. and Tech. Col., Box 806, Alfred, N.Y. 14802
CRANDALL, Robert W., Winthrop House, Apt. 701, 1727 Massachusetts Ave., N.W., Washington, D.C.
CRANDALL, William W., 203 Seville Way, San Mateo, Calif.
CRANE, Dwight B., 6637 Northumberland, Pittsburgh, Pa. 15217
CRANE, Frederick G., Drake Univ., Col. of Bus. Admin., Des Moines 11, Iowa
CRANE, Garry M., 916 Robin Rd., Silver Spring, Md. 20901
CRANE, (Mrs.) Jane-Ring F., 7103 Oakridge Ave., Chevy Chase, Md. 21620
CRANE, John B., 101 W. Broad St., Mount Pleasant, Iowa
CRANK, T. Harvey, Univ. of Virginia, Dunnington House, Sta. 2, Charlottesville, Va. 23903
CRANMER, H. Jerome, Col. of Liberal Arts, Drew Univ., Madison, N.J. 07940
CRAVEN, J. Howard, 3839 Dixon Pl., Palo Alto, Calif.
CRAVEN, John V., Box 555, Middlebury, Vt. 05753
CRAWFORD, Flora A., Park Col., Dept. of Econ., Parkville, Mo. 64152
CRAWFORD, J. G., Australian Nat. Univ., Res. Sch. of Pacific Studies, Canberra, A.C.T., Australia
CRAWFORD, James F., Georgia State Col. of Bus. Admin., 33 Gilmer St., S.E., Atlanta, Ga 30303
CRAWFORD, Jerry T., 257 Wilson St., Albany, Calif.
CRAWFORD, Malcolm D., 3631 East 7th Ave., Denver 6, Colo.
CRAWFORD, Mary M., Indiana Univ., Bloomington, Ind.

CRAWFORD, Morris H., U.S. Embassy, APO San Francisco 96346
CRAWFORD, Robert B., Concord Col., Athens, W.Va. 24712
CRAWFORD, Robert P., P.O. Drawer 1231, Lincoln, Neb.
CRAWFORD, Robert W., 1906 Oak Lane Rd., Wilmington, Del. 19803
CRAWFORD, William B., 206 Cedarwood, Flushing, Mich. 48433
CRAYCRAFT, Joseph L., 7319 Hamilton Ave., Cincinnati 31, Ohio
CREAMER, Daniel, Nat. Ind. Conf. Bd., 845 3rd Ave., New York 22, N.Y.
CREAMER, James T., Boston Col., Chestnut Hill, Mass. 01012
CREAN, John F., London House, Mecklenburgh Sq., London, W.C. 1, England
CREASON, Woodrow W., 202 S. 23rd, Terre Haute, Ind.
CREMER, Herbert S., Assabet Hts., Maynard (Stow), Mass. 01754
CREQUE, Darwin, 21 Main St., Charlotte Amalie, V.I. 00801
CREWS, Joseph M., Univ. of North Carolina, Sch. of Bus. Admin., Chapel Hill, N.C. 27515
CREWSON, Harry B., Jr., Dept. of Econ., Ohio Univ., Athens, Ohio 45701
CRICK, Nelson B., American Univ., Box 120, 113 Sharia Kasr el Aini, Cairo, U.A.R.
CRIM, Ed F., Jr., Univ. of Oklahoma, Dept. of Econ., Norman, Okla.
CRINER, Douglas E., 4415 S. 36th St., Apt. A-1, Arlington, Va. 22206
CRISAFULLI, Virgil C., 72 Meadow St., Clinton, N.Y. 13323
CRISPO, John H. G., Univ. of Toronto, Sch. of Bus., Toronto 5, Ont., Canada
CRIST, William D., Metropolitan Junior Col., 3845 McGee St., Kansas City, Mo. 64111
CRISTOFAR, Stephen, 620 W. 149th St., New York, N.Y. 10031
CROCKER, Thomas D., Univ. of Wisconsin-Milwaukee, 3203 Downer Ave., Milwaukee 11, Wis.
CROLL, Philip D., Sun Oil Co., 1608 Walnut St., Philadelphia, Pa.
CROLL, Robert F., 1411 Normandy Pl., Apt. 216, Manhattan, Kans. 66502
CROMWELL, Colin A., 9450 Savery, Detroit, Mich. 48206
CRONIN, John F., 1312 Massachusetts Ave. N.W., Washington 5, D.C.
CRONOPULOS, John, 83-46 225th St., Floral Park, N.Y.
CROOK, Stanley K., 112 Century Blvd., Apt. 4, Playa Del Rey, Calif. 90291
CROOKS, Eugene F., Hdqtrs. Operations Div., APO 403 New York
CROSBY, (Mrs.) Eleanor R., Box 656, Route 5, Wayzata, Minn. 55391
CROSBY, Lewis G., 139 Broadway, Rockville Centre, L.I., N.Y.
CROSBY, Martin J., 531 N. Quincy St., Abington, Mass. 02351
CROSS, (Mrs.) Cynthia S., 1021 Barton Dr., Apt. 209, Ann Arbor, Mich. 48105
CROSS, Ira B., 1454 LeRoy Ave., Berkeley, Calif. 94708
CROSS, James S., Sun Oil Co., 1608 Walnut St., Philadelphia 3, Pa.
CROSS, John G., 1021 Barton Dr., Apt. 209, Ann Arbor, Mich. 48105
CROSSER, Paul, Adelphi Col., Garden City, N.Y.
CROSSMON, Bradford D., 161 Pelham Rd., R.D. #2, Amherst, Mass. 01002
CROTEAU, John T., Univ. of Notre Dame, Dept. of Econ., Notre Dame, Ind. 46556
CROTON, Donald S., 1501 Morton, Ann Arbor, Mich.
CROUTER, Homer L., 1420 W. Sunset Ave., Decatur, Ill.
CROW, Robert T., 1921 Kalorama Rd., Apt. 103, Washington, D.C. 20009
CROWDER, Edward T., Exec. Office of the Pres., Office of Statis. Stand., Bur. of the Budget, Washington 25, D.C.
CROWDER, Walter F., Ind. Dist., 330 W. 42nd St., New York 36, N.Y.
CROWE, Marshall A., Canadian Imperial Bank of Com., 25 King St. W., Toronto 1, Ont., Canada
CROWE, Paul A., 246 Daniel Ave., S.E., Atlanta, Ga. 30317
CROWLEY, Edwin D., 6636 32nd St., N.W., Washington, D.C.
CROWLEY, Michael F., 4135 Southern Ave., S.E., Apt. 102, Washington, D.C. 20020
CROWLEY, Ronald W., Duke Univ., Dept. of Econ., Durham, N.C. 27706
CROZIER, Robert B., Econ. Coun. of Canada, P.O. Box 527, Ottawa, Ont., Canada
CRUM, Lawrence L., Univ. of Texas, Dept. of Fin., Austin, Texas 78712
CRUM, Robert E., 3303 W. Rogers Ave., Baltimore, Md. 21215
CRUTCHER, Joseph A., 848 Pine Ridge Knoll, Fullerton, Calif. 92632
CRUZ, Kenneth D., 87-70 115th St., Richmond Hill, N.Y. 11418
CRUZ, Roman A., Jr., 84 Maria Clara St., Quezon City, Philippines
CUBBAGE, Kenneth W., Univ. of Colorado, Sch. of Bus., Boulder, Colo. 80304
CULBERTSON, John, V, 11-T Ridge Rd., Greenbelt, Md.
CULBERTSON, John M., Univ. of Wisconsin, Dept. of Econ., Madison 6, Wis.
CULBERTSON, William R., 221-D E. Hanna Ave., Indianapolis, Ind. 46227
CULLATHER, James, P.O. Box 48, Notre Dame, Ind. 46556
CULLEN, Donald E., 134 Pine Tree Rd., Ithaca, N.Y.
CULLEN, James A., Southgate Alger Ct., Bronxville, N.Y.
CULLEY, J. F., USAID/Liberia, Dept. of State Mail Room, Washington, D.C. 20251
CULLEY, Wilmon E., Box 87 R.D. 1, Weirton, W.Va.
CULLITON, James W., U.S. Tariff Comm., Washington 25, D.C.
CULLITY, John P., 209 Belvidere Ave., Jersey City 6, N.J.
CULLUM, James B., Jr., Eastman Dillon, Union Securities and Co., 1 Chase Manhattan Plaza, New York 5, N.Y.
CULVER, Albert B., 2456 University Ave., Eugene, Ore.
CULVER, David W., Univ. of Georgia, Dept. of Agric. Econ., Athens, Ga.
CULYER, Anthony J., Univ. of Exeter, Dept. of Econ., Queen's Bldg., Exeter, England
CUMBERLAND, John H., Bur. of Bus. and Econ., Univ. of Maryland, College Park, Md.
CUMMINGS, Donald G., State Col. of Iowa, Dept. of Soc. Sci., Cedar Falls, Iowa 50613
CUMMINGS, Gerald H., 7050 River Rd., Flushing, Mich. 48433
CUMMINGS, Herbert J., 1435 4th St., S.W., Washington, D.C. 20024
CUMMINGS, Leland, Wyncote, Pa. 19095
CUMMINGS, Peter M., OF104403, A Btry. 2d BN 3rd FA, 3rd Armored Div., APO New York 09039
CUMMINGS, Ralph W., Jr., Univ. of Illinois, Dept. of Econ., Urbana. Ill. 61803
CUMMINGS, Ronald G., 7016 Highway No. 9 N.W., Parkville, Mo. 64152
CUMMINS, Gaylord, 636 Stratford Dr., New Orleans 23, La.
CUNANAN, Joaquin, Joaquin Cunanan and Co., P.O. Box 2288, Manila, Philippines
CUNNINGHAM, Benjamin H., 645 Taper Dr., Seal Beach, Calif. 90740
CUNNINGHAM, Brian, 71-24 165th St., Flushing, N.Y. 11365
CUNNINGHAM, Firman L., 715 Shawnee Dr., Murfreesboro, Tenn.
CUNNINGHAM, Roland L., Texas Tech. Col., 247 Men's #9, Lubbock, Tex. 79706
CUNNYNGHAM, Jon, 423 W. 120th St., Apt. 93, New York, N.Y. 10027
CUONG, Nguyen Hoang, Deputy Dir. Gen. of Trade Serv., Ministry of Nat. Econ., 59, Gia Long, Saigon, Vietnam
CUONG, Nguyen Nhu, Nat. Inst. of Admin., 10 Tran Quoc Toan, Saigon, Vietnam
CUPPER, Robert D., 4531 Forbes Ave., Pittsburgh, Pa. 15213

CURCIO, Joao, Jr., Rua dos Ingleses 92, Sao Paulo-SP, Brazil
CURNUTT, Jerry H., Kodak Office, Statist. Dept., 343 State St., Rochester, N.Y. 14650
CURRAN, Donald J., Canisius Col., Buffalo 8, N.Y.
CURRAN, Kenneth J., 916 Cheyenne Rd., Colorado Springs, Colo.
CURRAN, Ward S., Trinity Col., Box 1354, Hartford 6, Conn.
CURRIE, Blair C., 40 Jefferson Ave., Geneva, N.Y. 14456
CURRIE, George F., Jackson Col., P.O. 7066, Jackson, Miss.
CURRIER, Frederick P., 1482 Bowers, Birmingham, Mich.
CURRY, Ben F., Dept. of Econ., Georgia State Col., Atlanta, Ga.
CURRY, (Mrs.) Eleanor N., 180 Main St., Orono, Me. 04473
CURRY, Pembroke R., Jr., 109 Gatehouse Dr., Gatehouse Apts., Apt. E, Metairie, La. 70001
CURTIS, Clifford A., Dept. of Polit. and Econ. Sci., Queens Univ., Kingston, Ont., Canada
CURTIS, Frank J., Jr., Ferris State Col., Dept. of Soc. Sci., Big Rapids, Mich. 49307
CURTIS, John M., 1808 Allison Rd., Vancouver 8, B.C., Canada
CURTIS, L. Simington, Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
CURTIS, William R., 321 Cross Woods Dr., Falls Church, Va.
CURTIS, William W., Univ. of Illinois, 408 David Kinley Hall, Urbana, Ill. 61803
CURTISS, John S., Univ. of Cincinnati, Dept. of Econ., McMicken Hall, Cincinnati 21, Ohio
CURTISS, William M., Found. for Econ. Educ., Irvington on Hudson, N.Y. 10533
CURZON, Gerard, Grad. Inst. of Int. Studies, 132 Rue De Lausanne, Geneva, Switzerland
CUSACK, Sister Mary T., Rosary Col., River Forest, Ill. 62369
CUSHMAN, Edward L., 14250 Plymouth Rd., Detroit 32, Mich.
CUSICK, James, 7 Weatherby Rd., Hanover, N.H.
CUTLER, Addison T., Fed. Res. Bank of Cleveland, Cleveland 1, Ohio
CUTLER, Herschel, 1220 Blair Mill Rd., Silver Spring, Md.
CUTLER, Howard A., P.O. Box 210, College, Alaska 99735
CUTLER, Suzanne, 53 Washington Sq., South, #703, New York, N.Y. 10012
CUTT, James, York Univ., Econ. Dept., 2275 Bayview Ave., Toronto 12, Ont., Canada
CUTTS, Jesse M., 3212 Oberlin St., Orlando, Fla. 32804
CYERT, Richard M., Carnegie Inst. of Tech., Pittsburgh 13, Pa.
CYPERT, Clifford L., 2325 Forest Ave., Lansing, Mich. 48910
CYPIN, Jack G., 15 Hill Lane, Levittown, N.Y. 11756
CZAKO, Joseph, 2400 41st St. N.W., Washington, D.C. 20007
CZAMANSKI, Stanislaw, Cornell Univ., Dept. of City and Reg. Plan., Ithaca, N.Y. 14850
CZARNECKI, Gerald M., 2607 Senate Drive, Apt. A-6, Lansing, Mich. 48912
CZIRJAK, Laszlo, 130 Sun Dance Rd., Stamford, Conn. 06903

DAANE, James D., Bd. of Gov. of Fed. Res. System, Washington, D.C.
DAANE, Kenneth E., Arizona State Univ., Sch. of Bus. Admin., Bus. Dept., Tempe, Ariz.
DABNEY, (Mrs.) Henrietta L., 49 W. 72nd St., New York 23, N.Y.
da COSTA, Jose M. M., Universidade do Para, Faculdade de Ciencias Econ., Avenida Nazare, 121, Belem, Para, Brazil
DACY, Douglas C., 400 Army-Navy Dr., Arlington, Va.
DADONE, Aldo, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago. Ill. 60637
DAENECKE, Eric, APO 96528 (AID), San Francisco
DAFT, Lynn M., Nat. Comm. on Food Mktg., GSA-ROB, 7th and D Sts, S.W., Washington, D.C. 20407
DAFTARY, Farhad, 2723 Ashby Pl., Apt. D, Berkeley, Calif. 94705
DAGNINO-PASTORE, Jose M., Esmeralda 320, 4º Piso, Buenos Aires, Argentina
DAHL, Albin J., Univ. of Nevada, Dept. of Econ., Reno, Nev. 89507
DAHL, Frederick R., Bd. of Gov. of the Fed. Res. System, 20th and Constitution Ave., N.W., Washington, D.C. 20551
DAHL, Randle E., Western Res. Univ., N.D. Baker Memorial Bldg., Room 336, Cleveland 6, Ohio
DAHL, Thomas E., 7321 Takoma Ave., Takoma Park, Md. 20012
DAHLBERG, Arthur C., U.S. Econ. Corp., 420 Lexington Ave., New York 17, N.Y.
DAHLE, Robert D., Xerox Corp., 750 Sibley Tower Bldg., Rochester, N.Y.
DAICOFF, Darwin W., Dept. of Econ., Univ. of Kansas, Lawrence, Kans.
DAILEY, Don M., 5726 Stony Island Ave., Chicago, Ill. 60637
DAIUTE, Robert J., Rider Col., Trenton 9, N.J.
DAJANI, Wafai, P.O. Box 251, Amman, Jordan
DAKE, Jerry L., U.S. Naval Postgrad. Sch., Monterey, Calif. 93940
DAKE, Leland E., 206 Sansome St., San Francisco 4, Calif.
DAKIN, Bruce H., 1552 Woodlawn S.E., Grand Rapids, Mich. 49507
DALATI, Abed E., 200 S. 43rd St., Philadelphia, Pa. 19140
DALE, Ernest, 15 E. 55th St., New York, N.Y. 10022
DALE, Leon A., Col. of Bus. Admin., Univ. of Bridgeport, Dept. of Ind. Rela., Bridgeport 4, Conn.
DALIN, Irving, 315 Riverside Dr., New York 25, N.Y.
DALISAY, Amando M., 149 Minnesota Ave., Quezon City, Philippines
DALLAS, Sherman, Sch. of Indust. Mgt., Georgia Inst. of Tech., Atlanta, Ga.
DALRYMPLE, Douglas J., 6007 Babbitt Ave., Encino, Calif. 91316
DALTON, George, Dept. of Econ., Northwestern Univ., Evanston, Ill.
DALTON, James A., 65 Walnut St., Belmont, Mass. 02178
DALY, Conor J., 26 Nepean St., Ottawa 4, Ont., Canada
DALY, Dennis C., St. Mary's Col., Dept. of Bus. Admin., Winona, Minn.
DALY, Donald J., 440 Roger Rd., Ottawa 8, Ont., Canada
DALY, Rex F., Outlook and Situation Bd., U.S.D.A., Washington 25, D.C.
DAMASKENIDES, Anthony N., 14, Aristotelous St., Thessaloniki, Greece
D'AMBROSTO, Charles A., Col. of Bus. Admin., Univ. of Washington, Seattle 5, Wash.
DAMERON, Wilbur R., 285 Water St., Perth Amboy, N.J.
D'AMORE, James, Jr., 30 Allen Pl., Hartford, Conn.
DAMRAU, William F., 185 Park Row, Apt. 16-A, New York, N.Y. 10038
DANA, James D., Lawrence Col., Dept. of Econ., Appleton, Wis.
DANA, Joseph L., 10751 Rose Ave., #333, Los Angeles, Calif. 90034
DANBURG, Saul, 159 Oliver Rd., Waban, Mass. 02568
DANCE, Maurice E., San Fernando Valley State Col., Northridge, Calif.
DANE, Andrew J., 2127-B Duke, Lubbock, Tex.
DANEHY, Lester J., 117 Didama St., Syracuse, N.Y. 13224
DANELLIS, Constantine, Sacramento State Col., 6000 J St., Sacramento, Calif. 95819
DANFIELD, Richard L., 514 Meadow Lane, Sheboygan Falls, Wis. 53085
DANHOF, Clarence H., Brookings Inst., 1775 Massachusetts Ave., Washington 6, D.C.
DANIEL, Coldwell, III, Univ. of Houston, Dept. of Econ. and Fin., Houston, Tex. 77004
DANIEL, (Mrs.) Eleanor S., Lawrence Brook Manor, 34 North Dr., East Brunswick, N.J. 08816

DANIEL, John C., Lawrence Brook Manor, 34 North Dr., East Brunswick, N.J. 08816
DANIELIAN, N. R., 1625 Eye St. N.W., Washington 6, D.C.
DANIELS, Clinton C., Greensboro Col., Dept. of Econ., Greensboro, N.C. 27402
DANIELS, M. Gordon, Dept. of State—Santiago, Washington, D.C. 20521
DANIELS, Robert W., 13 Riccarton Crescent, Currie, Midlothian, Scotland
DANIELSEN, Albert L., 210 Clarke Dr., Athens, Ga. 30601
DANIELSON, Arnold G., 6004 Springhill Dr., Greenbelt, Md. 20770
DANKERT, Clyde E., Dept. of Econ., Dartmouth Col., Hanover, N.H. 03755
DANNEMANN, Geraldo, Rue Marechal Floriano 3, Canela, Salvador, Bahia, Brazil
DANTON, Lawrence A., Univ. of Omaha, Dept. of Econ., Omaha, Neb. 68101
DANYLYK, John T., 154 Birch St., Apt. C-4, Falls Church, Va. 22046
DAOUST, Joseph P., 18th and Thompson Sts., Philadelphia, Pa. 19121
DARBY, Orville L., 601 Applewood, Lee's Summit, Mo. 64063
DARBYSHIRE, Leslie, 111 Pasatiempo Dr., Santa Cruz, Calif.
DARCY, Robert L., Dept. of Econ., Ohio Univ., Athens, Ohio 45701
DARDIS, Rachel, 1001 E. State St., Ithaca, N.Y. 14850
DARLING, H. Daniel, 106 W. Monroe Ave., Alexandria, Va.
DARLING, John R., Jr., 2017 Southwood Dr., Champaign, Ill.
DARLING, Joseph W., 423 New Jersey Ave., S.E., Washington 3, D.C.
DARLING, Paul G., Bowdoin Col., Econ. Dept., Brunswick, Me. 04011
DARMSTADTER, Joel, Nat. Plan. Assoc., 1606 New Hampshire Ave., N.W., Washington 9, D.C.
DARNELL, Jerome C., Univ. of Colorado, Colo. Springs Center, Cragmor Rd., Colorado Springs, Colo. 80909
DARNTON, Donald C., Virginia Polytechnic Inst., Dept. of Econ., Blacksburg, Va. 24061
DA ROSA, J. Eliseo, 2020 Oleander Dr., Lexington, Ky. 40504
DARR, Richard K., Box 156, Rt. 1, River Falls, Wis. 54022
DARROCH, John D., U.S. Steel Corp., 525 William Penn Pl., Pittsburgh 30, Pa.
DARS, Lewis, City Col. of New York, Baruch Sch. of Bus. Admin., 17 Lexington Ave., New York 10, N.Y.
DAS GUPTA, Dave B., Atlantic Prov. Econ. Coun., 181 Westmorland St., Fredericton, N.B., Canada
DA SILVA LEME, Ruy A., c/o Escola Politecnica, Sao Paulo, Brazil
DASSEL, Virgil H., 681 Sheridan Ave., Columbus, Ohio 43209
DATAS-PANERO, Jose A., 2939 Van Ness St., N.W., Apt. 520, Washington, D.C. 20008
DAUDE, Bernard, 128 Avenue de la Republique, Paris Ile, France
DAUER, Ernest A., Household Fin. Corp., 3200 Prudential Plaza, Chicago, Ill. 60601
DAUGHERTY, Carroll R., Sch. of Bus., Northwestern Univ., Evanston, Ill. 60201
DAUM, Harry W., 9074 Doercrest Dr., Crestwood, Mo. 63126
DAUTEN, Carl A., 720 Cranbrook Dr., Kirkwood, Mo. 63501
DAUTEN, Paul M., 917 Lincolnshire Dr., Champaign, Ill.
DAVENPORT, Donald H., 434 State St., Albany, N.Y.
DAVENPORT, Michael, Sir George Williams Univ., Drummond St., Montreal 25, Que., Canada
DAVENPORT, Robert, 614 3rd St., S.W., Washington, D.C. 20024
DAVEY, Harold W., Dept. of Econ. and Soc., Iowa State Univ., Room 111 East Hall, Ames, Iowa
DAVID, A. S., Ford Found., P.O. Box 81, Katmandu, Nepal
DAVID, (Mrs.) Elizabeth L., 207 DuRose Ter., Madison, Wis. 53705
DAVID, Martin H., 207 DuRose Ter., Madison, Wis. 53705
DAVID, Paul A., Dept. of Econ., Stanford Univ., Stanford, Calif. 96128
DAVID, Paul T., Univ. of Virginia, Dept. of Govt. and For. Affairs, Charlottesville, Va.
DAVIDOFF, Lawrence M., 2113 18th St., N., Apt. 1061, Arlington, Va. 22201
DAVIDS, Lewis E., Sch. of Bus. and Pub. Admin., University of Missouri, Columbia, Mo.
DAVIDS, Thomas J., 1825 Fremont Dr., Alameda, Calif. 94501
DAVIDSON, Gestur B., 3325 15th Ave., S., Minneapolis, Minn. 55407
DAVIDSON, John H., 1987 S.W. 16th Ave., Portland 1, Ore.
DAVIDSON, Julius, Apt. 208, 4201 Cathedral Ave. N.W., Washington 16, D.C.
DAVIDSON, M. M., Fenn Col., Public Rela. Dept. and Dev. Office, Cleveland, Ohio
DAVIDSON, Mack L., Johnson C. Smith Univ., Charlotte 8, N.C.
DAVIDSON, Martin J., Univ. of Houston, Col. of Bus. Admin., Houston 4, Tex.
DAVIDSON, Paul, 1532 Brookhaven Rd., Wynnewood, Pa. 19096
DAVIDSON, Ralph K., 2 Washington Sq. Village, Apt. 8-0, New York 12, N.Y.
DAVIDSON, Roy M., 846 Justice Bldg., Ottawa, Ont., Canada
DAVIDSON, Sidney, Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
DAVIDSON, Thomas L., 5 Charcoal Rd., South Norwalk, Conn.
DAVIE, Bruce F., Georgetown Univ., Dept. of Econ., Washington 7, D.C.
DAVIES, David G., Econ. Dept., Duke Univ., Durham, N.C.
DAVIES, George R., 130 Grove St., Iowa City, Iowa
DAVIES, J. Ken, 877 N. 7th W., Provo, Utah
DAVIES, (Mrs.) Margaret G., 395 S. Bonnie Ave., Pasadena, Calif. 91106
DAVIES, Margaret M., Seattle Univ., Seattle 22, Wash.
DAVILA, Oswaldo, Santo Domingo 187, Quito, Ecuador
DAVIS, Benjamin, 20 Yawl St., Marina Del Rey, Venice, Calif.
DAVIS, Bernard, Berea Col., Bus. & Econ. Dept., Berea, Ky.
DAVIS, Charles H., University Gardens Apts., Apt. 2225, Dallas, Tex. 75205
DAVIS, Donald A., 12001 Pine St., Los Alamitos, Calif. 90720
DAVIS, Edward H., 61 Oakland Ave., Waterbury, Conn. 06710
DAVIS, Frank G., Fairleigh Dickinson Univ., Teaneck, N.J. 07666
DAVIS, Frank S., 111 Santa Paula Ave., San Francisco, Calif.
DAVIS, Gilbert C., Jr., 6609 Overhill Rd., Shawnee Mission, Kans.
DAVIS, Harold L., Bur. of Reclamation, U.S Dept. of Int., Bldg. 46, Denver Fed. Center, Denver, Colo.
DAVIS, Hiram S., Western Mgt Consultants, 100 W. Clarendon, Phoenix, Ariz. 85013
DAVIS, Horace B., Shawm Hill, Sandwich, Mass. 02563
DAVIS, Howard W., Univ. of California, Dept. of Econ., Riverside, Calif.
DAVIS, J. Anderson, Dept. of Econ., Mississippi State Univ., P.O. Box 294, State College, Miss. 39762
DAVIS, James N., Jr., 207 College Ave., Winfield, Kans. 67156
DAVIS, James V., Cornell Univ., 57-F Hasbrouck Apts., Ithaca, N.Y. 14850
DAVIS, Jim E., Amer. Embassy, Box 85, FPO New York 09544
DAVIS, John C., Morgan's Mill Farm, Bluemont, Va. 22012
DAVIS, John D., Bethany Col., Dept. of Econ., Bethany, W. Va. 26032
DAVIS, John E., Barron and Davis, 601-604 Nelson Bldg., Charleston, W.Va.
DAVIS, Joseph S., 691 Mirada Ave., Stanford, Calif. 96128
DAVIS, (Mrs.) Karen, Rice Univ., Dept. of Econ., Houston, Tex. 77001

DAVIS, Kinchloe C., Oklahoma State Univ., Dept. of Agric. Econ., Stillwater, Okla. 74075
DAVIS, L. Harlan, 13909 Parkland Dr., Rockville, Md.
DAVIS, Lance E., Dept. of Econ., Purdue Univ., Lafayette, Ind.
DAVIS, Otto A., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh, Pa. 15213
DAVIS, Phillip E., Wisconsin Tel. Co., 722 N. Broadway, Room 1501-J, Milwaukee, Wis. 53202
DAVIS, Richard G., 92 Atlantic Ave., Brooklyn, N.Y. 11201
DAVIS, Richard M., Dept. of Econ., Univ. of Oregon, Eugene, Ore.
DAVIS, Robert K., Resources for the Future, 1755 Massachusetts Ave., N.W., Washington, D.C. 20036
DAVIS, Thomas E., 9217 Wedd Dr., Overland Park, Kans.
DAVIS, Tom E., Cornell Univ., Dept. of Econ., Ithaca, N.Y. 14850
DAVIS, Thomas G., 1126 Barnwell St., Columbia, S.C.
DAVIS, Thomas J., 2401 Bayshore Blvd., Apt. 1212, Tampa, Fla. 33609
DAVIS, Warren B., P.O. Drawer 1166, Pittsburgh 22, Pa.
DAVIS, William C., Univ. of Arizona, Col. of Bus., Tucson, Ariz.
DAVIS, William S., III, 127 Dulcos Lane, Highland Park, N.J. 08904
DAVIS, William Z., 911 Robbins Ave., Niles, Ohio 44446
DAVISSON, Charles N., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
DAWKINS, William C., Jr., Marietta Col., Dept. of Econ., Marietta, Ohio 45750
DAWSON, C. C., Route #3, Cadiz, Ky. 42211
DAWSON, George G., 90-20 102nd St., Richmond Hill, N.Y. 13144
DAWSON, John A., 1593 Dorion Ave., Ottawa 8, Ont., Canada
DAWSON, John C., Econ. Dept., Grinnell Col., Grinnell, Iowa 50112
DAWSON, Ray E., Univ. of Cincinnati, Sch. of Bus., Cincinnati, Ohio 45221
DAY, Ernest H., Univ. of Maryland, Dept. of Econ., B.P.A., College Park, Md. 20742
DAY, Jerome J., Jr., 409 S. 40th St., Philadelphia, Pa. 19104
DAY, John S., Purdue Univ., Herman C. Krannert Grad. Sch. of Ind. Admin., Lafayette, Ind. 47907
DAY, Richard H., Dept. of Econ., Univ. of Wisconsin, Madison 6, Wis.
DAZA-ROA, Alvaro, Carrera 1a. #77-54, Bogota, Colombia
D'CRUZ, Raphael C., St. Xavier Col., 103rd and Central Park, Chicago, Ill. 60655
de ABREU, Luiz Simoes, Av. Antonio Enes, 66, Cx. Postal 1554, Lourenco Marques, Mozambique
DEACON, Ruth E., Ohio State Univ., 1787 Neil Ave., Columbus 10, Ohio
DE ALESSI, Louis, Dept. of Econ., Duke Univ., Durham, N.C.
DEAN, Arthur H., 48 Wall St., New York 5, N.Y.
DEAN, Charles R., Dean Econ., 156 E. 52nd St., New York, N.Y. 10022
DEAN, Edwin R., Univ. of Ibadan, N.I.S.E.R., Ibadan, Nigeria
DEAN, Gerald W., 638 Elmwood Dr., Davis, Calif. 95616
DEAN, Howard, Bankers Life Co., 711 High St., Des Moines 7, Iowa
DEAN, James C., Univ. of Kentucky, Dept. of Agric. Econ., Lexington, Ky. 40506
DEAN, James W., Harvard Univ., Dept. of Econ., Cambridge 38, Mass.
DEAN, Joel, Joel Dean Asso., Inc., 30 Rockefeller Plaza W., New York, N.Y. 10020
DEAN, Leslie A., 26923 Wright Dr., Selfridge Air Force Base, Mich. 48045
DEAN, Prentice N., Buena Vista Col., Storm Lake, Iowa 50588
de ANDRADE, Iguatemy J., Rua Morgado de Mateus 139, Sao Paulo, Brazil
DE ANDREA, Vincent, Univ. of Massachusetts, Econ. Dept., Amherst, Mass. 01003
DEANE, Richard G., 5088 Cedardale Lane, Flushing, Mich. 48433
DE ANGELIS, Arthur E., Jr., 10943 Margatehall Dr., Bridgeton, Mo. 63044
DEANS, Robert H., Dept. of Econ., Duquesne Univ., Pittsburgh, Pa.
DEASTLOV, Francis J., Int. Monetary Fund, 19th and H St., N.W., Washington 25, D.C.
DEAVER, John V., Chase Manhattan Bank, 9th Fl., 1 Chase Manhattan Plaza, New York, N.Y. 10015
DEAVERS, K. L., 3706 12th St. S., Arlington, Va. 22204
DE BARD, August A., Jr., 15 Alexander Dr., Red Bank, N.J.
de BEERS, John S., 5135 Newport Ave., Washington, D.C. 20016
DeBENEDICTIS, Michele, Inst. di Econ. e Polit. Agr., Facolta di Agr., Portici, Naples, Italy
de BOER, Eleanor, 1716 Valencia, N.E., Albuquerque, N.M.
DE BOER, Lloyd M., Univ. of Illinois, 419 Commerce-W., Dept. of Marketing, Urbana, Ill.
de BONE, Georges, 1672 W. Longview Ave., Stockton, Calif.
DEBREU, Gerard, Dept. of Econ., Univ. of California, Berkeley 4, Calif.
de CAPRILES, Miguel A., New York Univ., Washington Sq. E., New York 3, N.Y.
DECARLI, Edgardo, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
DECARLO, Guy R., 2000 S. Eads St., Apt. 1220, Arlington, Va.
de CARMELLE, Sister M., Alvernia High School, 3900 N. Lawndale Ave., Chicago, Ill. 60618
de CHAZEAU, Melvin G., Cornell Univ., Sch. of Bus. and Pub. Admin., Ithaca, N.Y.
DE CICCO, Ernest M., 4 Cider Mill Rd., Lynnfield, Mass.
DECKER, Robert L., Econ. Dept., Union Oil Co., Box 7600, Los Angeles 17, Calif.
DE COU, Donald F., Dept. of Econ. and Soc., Kansas State Univ., Manhattan, Kans.
DEDERICK, Robert G., The Northern Trust Co., 50 S. LaSalle St., Chicago, Ill. 60603
DE FALCO, Ciro, 786 Garden St., New York 60, N.Y.
DeFONTENAY, Patrick, Int. Bank for Recon. and Dev., Econ. Dept. Room 1021, 1818 H St., N.W., Washington, D.C. 20433
de FORD, Herman J., 1304 Dewey St., Hollywood, Fla.
DeFOREST, John D., 600 W. Carpenter, Fairfield, Iowa 52556
DeFRAIN, Patricia L., Eastern Washington State Col., Div. of Hist. and Soc. Sci., Cheney, Wash. 99004
De FULVIO, Eugene J., 7 E. Broad St., Bethlehem, Pa.
DEGANI, Amina, Univ. of Singapore, Econ Dept., Bukit Timah Rd., Singapore 10, Malaya
de GARA, John P., 165 W. Patterson Ave., Columbus 2, Ohio
DEGEN, Robert A., Dept. of Econ., Univ. of the South, Sewanee, Tenn. 37375
DeGREGORI, Thomas R., Case Inst. of Tech., Dept. of Humanities, Cleveland, Ohio 44106
de GROOT, Louis, 9 Lorraine Ct., New City, N.Y. 10956
DEHEM, Roger, Faculty of Soc. Sci., Laval Univ., Quebec, P.Q., Canada
DEIF, Nazih A., Ministry of Treasury, Cairo, Egypt, U.A.R.
DEIN, Raymond C., Univ. of Nebraska, Col. of Bus. Admin., Lincoln, Neb.
DEINZER, Harvey T., Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla. 32601
Del ROSSI, James A., 12204 Braxfield Ct., Apt. 5, Rockville, Md. 20850
DEITER, John C., 2803 Kimberly Lane, Tampa, Fla. 33618
de JANOSI, Peter E., 525 E. 86th St., New York, N.Y.
de JOUVENEL, Bertrand, Par Borne, Chateau D'Anserville, Oise, France
DEKOCK, Jules, Naciones Unidas CEPAL, 871, Providencia, Santiago, Chile
de KOK, Yvonne, 5100 Connecticut Ave., N.W., Apt. 407, Washington, D.C. 20008

De KORNE, Baldwin L., 1408 Bates, S.E., Grand Rapids, Mich. 49506
DE LA CRUZ, Santiago F., P.O. Box 1245, Univ. of the East, Manila, Philippines
DELAERE, Anne-Marie, 88 Avenue des Allies, Louvain, Belgium
de LAKY, Barnabas R., Dept. of Nat. Health and Welfare, Res. and Statis. Div., Brooke-Clayton Bldg., Room 374, Ottawa 3, Ont., Canada
DELANO, Myles S., Michigan State Univ., 342 Eppley Center, East Lansing, Mich. 48823
DELAPLAINE, John W., Office of Pro. Adv., Inter-Amer. Dev. Bank, 801 17th St., Washington, D.C. 20577
DELAPLANE, Walter H., Univ. of Arizona, Dept. of Academic Affairs, Tucson, Ariz.
DELARCO, Jose F. N., c/o Betty Dick, 101-D Patterson Hall, North Carolina State Col., Raleigh, N.C. 27607
DELAVAN, William H., Jr., 108 E. Genesee St., Skaneateles, N.Y. 13152
De LEEUW, Frank, 3175 Porter St., N.W., Washington, D.C. 20008
De LEEUW, John S., Old Dominion Col., Norfolk, Va. 23508
DELEHANTY, George E., Northwestern Univ., Dept. of Econ., Evanston, Ill.
DELEHANTY, John A., U.S. Dept. of Labor-OMAT, 1730 M St., Room 510, Washington, D.C.
DeLEO, Alfred V., Duquesne Univ., Sch. of Bus. Admin., 600 Forbes Ave., Pittsburgh 19, Pa.
de LIMA-VITULE, Affonso A., Av. Oswaldo Cruz 70 ap. 302, Rio de Janeiro, Gb, Brazil
DELLA VALLE, Philip A., 455 Huntington Ave., Buffalo, N.Y. 14216
DELLENBARGER, Lynn E., Jr., West Virginia Univ., Col. of Com., Morgantown, W.Va. 26506
DELLIN, Lubomir A. D., 37 Brewer Pkwy., So. Burlington, Vt. 05670
DELLIQUADRI, Carmen L., Woodland Rd., Houghton, Mich. 49931
DELLON, Howard N., 1633 Jefferson St., Rockville, Md.
de LOGERES, Marc deF., 360 E. 72nd St., Apt. 2401, New York, N.Y. 10021
DELPHENDAHL, Johannes, Univ. of Maine, Dept. of Agric. Econ., Arono, Me. 04473
del REY, Eusebio, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
DeLUCIA, Lawrence A., 3426 N. Fox Ave., Tucson, Ariz. 85716
DELVALLE, Kenneth T., Campbell Soup Co., 375 Memorial Ave., Camden 1, N.J.
Del VECCHIO, G., Via Acherusio, 36, Rome, Italy
DeMAAGD, Gerald R., 343 Elizabeth Ave., Rockford, Mich. 49341
de MAEYER, Jaak F. R., Willem Geetsstraat, 12, Mechelen, Belgium
De MAGALHAES, Pedro J. G., 431 Riverside Dr., Apt. F-C, Woodbridge Hall, New York, N.Y.
DeMAIRO, Constantine A., 41-27 76th St., Queens, N.Y. 11373
DEMAKOPOULOS, Steve A., 600 W. 178th St., New York 33, N.Y.
DeMARIS, Edwin J., 108 S. Mattis Ave., Champaign, Ill.
DeMARTINI, Howard C., 2365 Newton St., Akron, Ohio 44305
de MATTIA, Edmund H., 4 Willowbrook Rd., Storrs, Conn. 06268
de MAYNADIER, Alain, Central Nat. Bank of Cleveland, P.O. Box 6179, Cleveland, Ohio 44101
DEMBITZ, Lewis N., 3414 Garfield St. N.W., Washington 7, D.C.
De MELTO, Dennis P., 1648 S. Taylor Rd., Cleveland 18, Ohio
De MELTO, (Mrs.) Jane O., 1648 S. Taylor Rd., Cleveland 18, Ohio
de MENIL, George, Massachusetts Inst. of Tech., Dept. of Econ. and Soc. Sci., Cambridge 39, Mass.
DEMENTIS, Gilbert, 527 E. Wisconsin Ave., Neenah, Wis.
DEMENY, Paul G., Office of Pop. Res., Princeton Univ., 5 Ivy Lane, Princeton, N.J. 08540
DEMING, F. L., 2601 Woodley Pl., Washington, D.C. 20008
DEMMER, Ronald A., Amer. Motors Corp., 14250 Plymouth Rd., Detroit 32, Mich.
DEMOND, Helen F., 1238 29th St. N.W., Washington 7, D.C.
de MORAES, Waldemar, 3200 S. 28th St., Apt. 403, Alexandria, Va. 22302
DEMOS, Emanuel G., 15 Tremont St., New Britain, Conn.
DeMOUY, Louis D., 5520 Karen-Elaine Dr., Lanham, Md. 20801
DEMPSEY, J. R., Univ. of Detroit, Lansing-Reilly Hall, McNichols Rd. at Livernois, Detroit 21, Mich.
DEMSETZ, Harold, Grad. Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
DENGATE, Richard M., 28915 Hales Ave., Madison Heights, Mich. 48071
DENGES, Jerry A., 632 Athens Ave., Cincinnati, Ohio 45226
DENISON, Edward F., 7108 Exeter Rd., Bethesda, Md. 21811
DENISON, (Mrs.) Elsie L., 7108 Exeter Rd., Bethesda, Md. 21811
DENNIS, Henry C., Box 211 ASR, Alameda, N.M. 87114
DENNLER, Carl, Jr., Univ. of Massachusetts, Sch. of Bus. Admin., Amherst, Mass.
DENSMORE, Max L., 5132 Tilbury Rd., Dayton, Ohio 45424
DENT, James K., 6500 Westland Rd., Bethesda, Md. 20034
DENTE, Leonard A., Danbury State Col., 181 White St., Danbury, Conn. 06814
DENTON, Frank T., 8 Higwood Dr., Ottawa 5, Ont., Canada
DENTON, (Mrs.) Joan M., 2828 Connecticut Ave., Washington 8, D.C.
DENTON, John H., Univ. of California Extension, 2223 Fulton St., Berkeley 4, Calif.
DENZER, Thomas F., Rockhurst Col., 5225 Troost Ave., Kansas City 10, Mo.
de OYAGUE, Alfredo Picasso, 102 rue du Ranelagh, Paris 16eme, France
De PASS, Calvin L., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
DePASS, Rudolph E., Univ. of Maryland, Dept. of Agric. Econ., College Park, Md.
DEPEW, Donald R., 139 Sycamore Dr., Pittsburgh, Pa. 15237
DEPEW, (Mrs.) Nancy M., 139 Sycamore Dr., Pittsburgh, Pa. 15237
De PODWIN, Horace J., 1 Tunstall Rd., Scarsdale, N.Y.
DEPPERSCHMIDT, Thomas O., 1800 8th Ave., Canyon, Tex.
DePRANO, Michael E., Univ. of Southern California, University Park, Dept. of Econ., Los Angeles 7, Calif.
DePRINCE, Albert E., Jr., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
de RAET, Pierre, c/o Mr. Alfred Bossu, 282 rue du Noyer, Brussels 4, Belgium
DERECHO, Augusto T., Shell Co., P.O. Box 441, Manila, Philippines
DERIAN, Gordon V., 2 Francis Ct., Glen Cove, N.Y. 11542
DERNBERGER, Robert F., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
DeROECK, Richard J., 966 Riley Dr., Albany, Calif. 94710
De ROOY, Jacob, 181 Conover Ave., Nutley, N.J. 07110
DERRICKSON, Gardner F., 2836 Cherry St., Falls Church, Va. 22042
DERTOUZOS, Demetrios N., Rider Col., Trenton, N.J. 08602
DERUGIN, G., 128 S. Commonwealth Ave., Los Angeles 4, Calif.
DERUS, Sister Margit, Rosary Col., River Forest, Ill. 60305
DERY, George C., Lowell Tech. Inst., Dept. of Econ. and Mgt., Commonwealth of Massachusetts, Lowell, Mass.
de RYCKE, Laurence, Occidental Col., 1600 Campus Rd., Los Angeles, Calif. 90041
DESAI, Meghnad J., Univ. of California, 309 Giannini Hall, Berkeley, Calif.
De SALVIA, Donald N., 900 Ackerman Ave., Syracuse 10, N.Y.

De SALVO, Joseph S., 8201 N. Keating Ave., Skokie, Ill.
De SANTES, Ricardo C., 52 Summer St., Malden, Mass.
DESCARTES, S. L., 16 Almendro St., Santurce, P. R. 00913
de SCHWEINITZ, Karl, Jr., Economics Dept., Northwestern University, Kresge Cent. Hall, Evanston, Ill.
DESMOND, Thomas C., 94 Broadway, Newburgh, N.Y.
DESNOYERS, Thomas H., Milwaukee Rd., Room 796, Union Sta., Chicago, Ill. 60606
De SPAIN, Edward, 14 W. Chestnut St., Chicago, Ill. 60610
DESPRES, Emile, 5 Grove Ct., Portola Valley, Calif. 96122
DesROCHERS, Gilles, 710, McEachran, Outremont, P.Q., Canada
DESSAL, Murray D., 6937 Winterberry Lane, Bethesda, Md. 20034
DESTA, Bekele, 2520 Durant Ave., Berkeley, Calif. 94704
de TARNOWSKY, Ivan, 1080 Eddy, San Francisco 9, Calif.
DETLEFSEN, Guy-Robert, The Pillsbury Co., 620 Pillsbury Bldg., Minneapolis 2, Minn.
DETOMASI, Don D., 3017 Blakiston Dr., N.W., Calgary, Alta., Canada
DEUTCH, Jacob, 721 Kahn Dr., Baltimore 8, Md.
DEUTERMANN, (Mrs.) Elizabeth P., 6907 Heyward St., Philadelphia, Pa. 19119
DEUTSCH, Antal, Queen's Univ., Dept. of Econ., Kingston, Ont., Canada
DEUTSCH, Gertrude, 305 E. 11th St., New York 3, N.Y.
DEUTSCH, John J., Econ. Coun. of Canada, P.O. Box 527, Ottawa, Ont., Canada
de VARAGES, Raoul B., 1416 Queen's Rd., Los Angeles, Calif. 90046
DeVEAU, Burton W., 5 Earich Rd., Athens, Ohio 45701
DEVINE, Eugene J., 724 Sunset Ave., Venice, Calif.
DEVINO, Gary T., 9 Sterling Ct., East Brunswick, N.J. 08816
DEVINO, William S., Univ. of Maine, Sch. of Bus. Admin., Orono, Me. 04473
De VISSER, Burton A., 8154 Laurelmont Dr., Los Angeles 46, Calif.
DEVLIN, David T., Fed. Res. Bank of New York, 33 Liberty St., New York, N.Y. 10012
de VOGHEL, Luc, 264, av. Van Horenbeeck, Brussels 16, Belgium
De VRIES, Barend A., 10018 Woodhill Rd., Bethesda, Md. 21811
de VRIES, Jan, 3439 Yale Sta., New Haven, Conn.
de VRIES, Margaret G., 10018 Woodhill Rd., Bethesda, Md.
DeVRIES, Marvin G., Grand Valley State Col., College Landing, Allendale, Mich. 49401
De VYVER, Frank, 8 Sylvan Rd., Durham, N.C.
DEWALD, William G., Ohio State Univ., Dept. of Econ., 1775 S. College Rd., Columbus, Ohio 43210
DEWEY, Donald, Econ. Dept., Columbia Univ., New York 27, N.Y.
DEWEY, Fredrick A., 60 E. 42nd St., New York 17, N.Y.
de WILDE, John C., Int. Bank for Recon. and Dev., 1818 H St. N.W., Washington 25, D.C.
De WIT, Ynto B., c/o Ismog, Linnaeusstraat 2a, Amsterdam, The Netherlands
DeWITT, Nicholas, 617 E. Eighth St., Bloomington, Ind.
De YOUNG, Robert H., Wilkes Col., Wilkes-Barre, Pa.
DEYRUP, Felicia J., 200 N. Broadway, Nyack, N.Y. 10960
DHAR, Samvit P., c/110 South Moti Bagh, New Delhi, India
DHILLON, Pritam S., Pennsylvania State Univ., Dept. of Agric. Econ., University Park, Pa. 17774
DHRYME, Phoebus J., Univ. of Pennsylvania, Wharton Sch. of Fin. and Com., Philadelphia, Pa. 19104
DIAB, Muhammad A., Dept. of Econ., Amer. Univ. of Beirut, Beirut, Lebanon
DIAKOPOULOS, C., 2, Dohiariou St., Athens (812), Greece
DIAMOND, Arnold H., 1105 Caddington Ave., Silver Spring, Md.
DIAMOND, Daniel E., New York Univ., Sch. of Com., Dept. of Econ., New York, N.Y. 10003
DIAMOND, Douglas B., 7206 Loch Lomond Dr., Bethesda, Md. 21811
DIAMOND, Harold S., 427 85th St., Brooklyn, N.Y.
DIAMOND, Jack J., 58-18 256th St., Little Neck, N.Y. 14754
DIAMOND, James J., De Paul Univ., 25 E. Jackson, Chicago, Ill. 60604
DIAMOND, (Mrs.) Patricia McC., 427 85th St., Brooklyn, N.Y.
DIAMOND, Peter A., Univ. of California, Dept. of Econ., Berkeley 4, Calif.
DIAS-CARNEIRO, O. A., Rua Das Laranjeiras 76, Apt. 702, Rio de Janeiro, Brazil
DIAZ-ALEJANDRO, Carlos F., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
DIAZ-LOPEZ, Felix, Econ. Dev. Admin., Lee Towers Bldg., 5455 Wilshire Blvd., Suite 1814, Los Angeles 36, Calif.
DIBEE, Khalil, Seattle Univ., Box 185 C and F, Seattle, Wash.
DICE, Charles A., 3720 N. High St., Columbus 14, Ohio
DICK, G. William, Jr., 1506 Morrison Dr., Lynchburg, Va. 24503
DICK, Trevor J. O., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
DICKENS, Albert E., 1627 K St., Room 208, Washington, D.C. 20006
DICKENS, Paul D., Univ. of Oklahoma, Dept. of Econ., Norman, Okla.
DICKERSON, Robert E., Dept. of Econ., Humboldt State Col., Arcata, Calif. 95521
DICKEY, G. Edward, 541 7th St., S.E., Washington, D.C. 20003
DICKEY, Robert I., Univ. of Illinois, 250 Commerce West, Urbana, Ill.
DICKEY, Ronald W., R.R. 1, Pawnee, Ill. 62558
DICKINSON, Frank G., 103 Wendell Pl., De Kalb, Ill. 60115
DICKINSON, Raymond L., Bell Tel. Lab., Inc., Everett-Crawford's Corner Rd., 2E-214, Holmdel, N.J. 07733
DICKISON, William E., 279 Blair Dr., Fairborn, Ohio 45324
DICKNEIDER, William C., Jr., 1108 La Cadena Ave., Arcadia, Calif.
DICKSON, Thomas I., Jr., Dept. of State, Cochabamba, Washington, D.C. 20521
DIEBOLD, William, Jr., 204 N. Broadway, Upper Nyack, N.Y.
DIEFFENDERFER, John C., I.E.S.C., 720 Fifth Ave., New York, N.Y. 10019
DIEMER, Brother Eugene B., Cardinal Stritch Hall, Christian Brothers House of Studies, 2455 Avery, Memphis, Tenn. 38112
DIER, John R., 763 Ocean Pkwy., Brooklyn 30, N.Y.
DIERSON, Frank T., 205 E. 42nd St., New York, N.Y. 10017
DIETZ, Arthur T., Sch. of Bus. Admin., Emory Univ., Atlanta 22, Ga.
DIETZ, Peter O., Sch. of Bus., Northwestern Univ., Evanston, Ill.
DIKE, George K., Agric. Econ. Dept., Michigan State Univ., East Lansing, Mich.
DI LEO, Francis X., 12 Sharon Ct., Milford, Conn. 06463
DILL, Arnold A., Fed. Res. Bank of St. Louis, Res. Dept., P.O. Box 442, St. Louis, Mo. 63166
DILL, Bernard C., Richmond Prof. Inst., 901 W. Franklin, Richmond 20, Va.
DILLABER, Philip A., 116 Northampton Dr., Hampton, Va.
DILLARD, Dudley, University Park, 7007 Forest Hill Dr., Hyattsville, Md.
DILLARD, (Mrs.) Louisa G., University Park, 7007 Forest Hill Dr., Hyattsville, Md.
DILLEY, Charles A., 1161 N.E. 86th St., Miami 38, Fla.
DILLINGHAM, W. P., Florida State Univ., Econ. Dept., Tallahassee, Fla.
DILLMAN, Buddy L., 1306 Heritage Pl., Morgantown, W. Va.

DILLON, John L., Univ. of New England, Faculty of Agric. Econ., Armidale 5N, New South Wales, Australia
DILLON, Robert E., Col. of Bus. Admin., Univ. of Cincinnati, Cincinnati 21, Ohio
DILLOWAY, Phylipp, 139 Linden Tree Rd., Wilton, Conn. 06897
DILWORTH, Barbara, Apt. 103 Detrick Pl., Frederick, Md.
DIMITRIJEVIC, Dimitrije, Vojvode Stepe 13, Belgrade, Yugoslavia
DIMLING, John A., Jr., 2111 Lansill Rd., M-120, Lexington, Ky. 40504
DIMMLER, George F., 1516 Arch St., Berkeley 8, Calif.
DIMOCK, Frank C., 302 Bay St., 10th Fl., Toronto 1, Ont., Canada
DIMOCK, Marshall E., RD No. 1, Bethel, Vt. 05032
DIMOND, Charles L., 47 Fernwood Rd., Larchmont, N.Y. 10538
DINCE, Robert R., Jr., Univ. of Georgia, Col. of Bus. Admin., Dept. of Banking and Fin., Athens, Ga. 30601
DING, Edwin, Col. of the Pacific, Stockton, Calif.
DINGLE, Mona E., Univ. of Missouri, 222 B. and P.A., Columbia, Mo. 65202
DINIC, Carl J., 262 Ridge Rd., Douglas Manor, L.I., New York 11363
DINKELACKER, E. William, 11815 Indigo Rd., Silver Spring, Md. 20906
DiPAOLO, Gordon A., 299 Henry St., Brooklyn 1, N.Y.
DIRECTOR, Aaron, Univ. of Chicago, Law School, Chicago, Ill. 60637
DIRKS, Frederick C., 5608 Pioneer Lane, Washington, D.C. 20016
DIRKSEN, Charles J., Sch. of Bus., Univ. of Santa Clara, Santa Clara, Calif.
DIRKSEN, James L., 3944 Floral Ave., Norwood, Ohio 45212
DIRLAM, Joel B., Univ. of Rhode Island, Col. of Bus. Econ. Dept., Kingston, R.I. 02836
di STEFANO, S. John, 12009 Long Ridge Lane, Bowie, Md. 20715
DITCHETT, Harry S., Mail No. 384, c/o Martin Co., Orlando, Fla. 32805
DITMARS, August, 61-35 98th St., Rego Park, N.Y. 11374
DITMARS, Earl E., 46 Chatsworth Ave., Larchmont, N.Y. 10538
DITTMER, Richard W., 3217 Comanche Rd., Pittsburgh 34, Pa.
DITTRICH, Andrew C., 425 Riverside Dr., Apt. 6-G, New York, N.Y. 10025
DIVINE, Thomas F., Marquette Univ., Milwaukee 3, Wis.
DIXON, Donald F., 743 Clarendon Rd., Narberth, Pa. 19072
DIXON, Louis V., Univ. of Southern Mississippi, Dept. of Econ., Southern Sta., Box 202, Hattiesburg, Miss. 39401
DIXON, Roger C., Amer. Embassy, APO New York 09170
DIZ, Adolfo C., Casilla de Correo 48, Tucuman, Argentina
DIZMANG, Oscar K., N. 5411 Post St., Spokane, Wash. 99208
DJAHANBANI, Khosro, 621 Church, #9, Ann Arbor, Mich.
DJAHANBIN, H. A., 1068 Boisverdure, Stey-Foy, P.Q., Canada
DJIMOPOULOS, Evangelos S., 249 W. 99th St., New York, N.Y. 10025
DOAK, Ervin J., Univ. of Toronto, Dept. of Polit. Econ., Toronto 5, Ont., Canada
DOAN, Mason C., 9606 Merwood Lane, Silver Spring, Md.
DOBELL, A. R., Harvard Univ., Dept. of Econ., Cambridge, Mass. 02138
DOBIAS, Peter, Inst. für Weltwirtschaft an der Univ., Düsternbrooker Weg 120-122, 23 Kiel, Germany
DOBRIANSKY, Lev E., Georgetown Univ., Washington, D.C. 22312
DOBROVOLSKY, Sergei P., Dept. of Econ., Rensselaer Poly. Inst., Troy, N.Y.
DOBSON, Ledford W., 103 Virginia St., Campbellsville, Ky. 42718
DOCKERAY, James C., University Park, 4010 Van Buren St., Hyattsville, Md.
DOCKSON, Robert R., 2275 Adair, San Marino, Calif. 92069
DODD, James H., College Sta., Box 1205, Fredericksburg, Va. 22404
DODGE, David A., Princeton Univ., Grad. Col., Princeton, N.J. 08540
DODGE, Norton T., Dept. of Econ., Univ. of Maryland, College Park, Md.
DODGE, William H., Univ. of Wisconsin, 307 Commerce Bldg., Madison, Wis.
DODSON, Edward N., III, 580 Loma Verde Ave., Palo Alto, Calif.
DOENGES, Byron, Willamette Univ., Salem, Ore. 97301
DOENGES, Rudolph C., Univ. of Texas, B.E.O.B. 106, Austin, Tex. 78712
DOERFLER, Ernest, N.Y. State Labor Rela. Bd., 270 Broadway, New York 7, N.Y.
DOERFLINGER, William J., 30 Schofield St., Newark, N.J. 07106
DOERING, Werner W., 1620 Adams St., Madison, Wis. 53711
DOERINGER, Peter B., Harvard Univ. R.O.T.C. 130, 12 Divinity Pl., Cambridge, Mass. 01922
DOESCHER, Donnie L., 3785 F St., Lincoln, Neb. 68510
DOHAN, Michael R., Massachusetts Inst. of Tech., Grad Sch., Econ. Dept., Cambridge 39, Mass.
DOHERTY, A. Noel, Univ. of Oklahoma, Dept. of Economics, Norman, Okla.
DOHIA, Sayed, 1033 13th, Boulder, Colo.
DOLBEAR, F. Trenery, Jr., 1328 Wightman St., Pittsburgh, Pa. 15217
DOLBEARE, Harwood B., 2735 S.W. 5th Pl., Gainesville, Fla.
DOLINS, Stanley L., Quarters 4411J, USAF Academy, Colo. 80840
DOLLEY, James C., University Sta., Austin 12, Tex.
DOLLISON, Robert B., 3013 Ordway St., N.W., Washington, D.C. 20008
DOLMATCH, Theodore B., Pitman Publishing Corp., 20 E. 46th St., New York, N.Y. 10017
DOLNICK, David, 951 Vernon Ave., Glencoe, Ill. 60022
DOLPHIN, Robert, Florida State Univ., Sch. of Bus., Tallahassee, Fla. 32306
DOMAR, Evsey D., Massachusetts Inst. of Tech., Dept. of Econ. 52-364, Cambridge, Mass. 01922
DOMENCICH, Thomas A., 48 Pinckney St., Boston, Mass.
DOMIKE, Arthur L., Regional Office of FAO, Casilla 10095, Santiago, Chile
DOMIN, Joseph S., Radwyn Apts. E-54, 275 Bryn Mawr Ave., Bryn Mawr, Pa. 19010
DOMINGO, Francisco B., Dev. Bk. of Philippines, David St. and M. Del Banco, Manila, Philippines
DOMINGUES, Nilo F., Visconde de Piraja St. 121/602, Rio de Janeiro, Brazil
DOMINGUEZ, Loreto M., 4905 Klingle St. N.W., Washington 16, D.C.
DOMOLKY, George C., 12 E. 72nd St., New York, N.Y.
DOMONKOS, John A., 741-H Clearview Dr., Euclid, Ohio 43018
DONALDSON, David J., Univ. of British Columbia, Dept. of Econ., Vancouver, B.C., Canada
DONALDSON, Elvin F., Ohio State Univ., 1775 S. College Rd., Columbus 10, Ohio
DONALDSON, Loraine, Georgia State Col., Econ. Dept., 33 Gilmer St., S.E., Atlanta, Ga. 30303
DONDERO, Lawrence J., 9604 Hillridge Dr., Kensington, Md. 20795
DONHEISER, Alan D., 846 Massachusetts Ave., Apt. 5D, Arlington, Mass. 02174
DONNELL, Richard H., U.S.S. Pocono (AGC-16), c/o Fleet P.O., New York, N.Y.
DONNELLEY, Robert G., 1355 N. Sandburg Ter., Apt. 2708, Chicago, Ill. 60610
DONNELLY, Peter F., 171 Kent St., Brooklyn 22, N.Y.
DONNELLY, Robert H., c/o Boise-Cascade Corp., Boise, Idaho

DONNELLY, William A., 430 Ridge Rd. Apt. 9, Greenbelt, Md. 20770
DONNELLY, William F., Fordham Univ., Spellman Hall, Bronx, N.Y. 10458
DONNER, Arthur W., 4533 Locust St., Philadelphia, Pa. 19139
DONNER, Kenneth, 410 Brighton Pl., Elkins Park, Pa. 17023
DONNER, Otto, 10011 Connecticut Ave., Kensington, Md. 20795
DONOFRIO, Robert J., 560 E. 189th St., Bronx, N.Y.
DONOGHUE, Francis, 45 Cooper St., Boston 13, Mass.
DONOHOE, Joseph M., 1517 33rd St., N.W., Washington 7, D.C.
DONOVAN, Clement H., Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla. 32603
DONOVAN, John A., Becker Junior Col., 41 Sever St., Worcester 9, Mass.
DONOVAN, John V., Univ. of New Hampshire, Whittemore Sch. of Bus. and Econ., Dept. of Econ., Durham, N.H. 03824
DON VITO, Pasqual A., 3514 Quesada St., N.W., Washington, D.C.
DOODY, Francis S., High St., Pembroke, Mass. 02359
DOOLEY, Mary L., Route 1, Brodhead, Wis. 53520
DOOLEY, Peter C., Pennsylvania State Univ., Dept. of Econ., 120 Boucke Bldg., University Park, Pa. 17774
DORAI, Gopal C., Wayne State Univ., Dept. of Econ., Detroit 2, Mich.
DORFMAN, Ben D., 4719 30 St. N.W., Washington 8, D.C.
DORFMAN, Joseph, Columbia Univ., Dept. of Econ., Fayerweather Hall, New York 27, N.Y.
DORFMAN, Robert, Univ. of California, Dept. of Econ., Berkeley, Calif. 94720
DORMAN, (Mrs.) Elsie S., 10, 713 Woodsdale Dr., Silver Spring, Md.
DORMAN, Robert O., 10, 713 Woodsdale Dr., Silver Spring, Md.
DORNER, Peter P., Dept. of Agric. Econ., Univ. of Wisconsin, Madison 6, Wis.
DORSCH, Arthur R., 198 Falmouth Rd., West Springfield, Mass. 01089
DORSETT, Howard, 1106 S. 2nd, Fairfield, Iowa 52556
DORSEY, John W., Jr., 1306 Cresthaven Dr., Silver Spring, Md. 20903
DOSIK, (Mrs.) Diana G., USAID—Lima, Dept. of State, Washington, D.C. 20521
DOSIK, Richard S., USAID—Lima, Dept. of State, Washington, D.C. 20521
DOSS, Veda, 101 Smith Lane, Syracuse, N.Y. 13210
DOSSANI, Nazir G., Univ. of Pennsylvania, Regional Sci. Dept., Philadelphia, Pa.
DOSSER, Douglas, Univ. of York, Dept. of Econ., Heslington, York, England
DOST, Jeanne E., 21 Mountain View Dr., Corvallis, Ore.
DOSTER, James F., Univ. of Alabama, P.O. Box 1955, University, Ala. 35486
DOTY, Don D., Mississippi State Univ., Dept. of Econ., P.O. Box 282, State College, Miss. 39762
DOUGALL, Herbert E., Grad. Sch. of Bus., Stanford Univ., Stanford, Calif. 96128
DOUGLAS, Edna, Washington State Univ., White Hall, Pullman, Wash.
DOUGLAS, James H., Jr., 33 S. Clark St., Chicago, Ill.
DOUGLAS, Norman S., 32-39 92nd St., Jackson Hgts., N.Y. 11369
DOUGLAS, Paul H., 109 Senate Off. Bldg., Washington, D.C.
DOUGLAS, R. J., 1106 Pugh Dr., Galena Park, Tex. 77547
DOUGLASS, David F., 34 Glenside Rd., South Orange, N.J. 07079
DOUGLASS, Gordon K., Pomona Col., Claremont, Calif.
DOUKAS, John A., 6025 Joseph Dr., Oxford, Ohio 45056
DOUTY, Christopher, San Francisco State Col., Dept. of Econ., San Francisco, Calif.
DOUTY, Harry M., 4612 Butterworth Pl., N.W., Washington 16, D.C.
DOVERSBERGER, Richard A., Box 221, Warren, Ind. 46792
DOW, Louis A., Dept. of Econ., North Carolina State Col., Raleigh, N.C.
DOWD, Douglas F., Dept of Econ., Cornell Univ., Ithaca, N.Y.
DOWD, Laurence P., Sch. of World Bus., San Francisco State Col., 1600 Holloway Ave., San Francisco 27, Calif.
DOWD, Richard F., 127 Pacific St., Brooklyn, N.Y. 11201
DOWDLE, Barney, Univ. of Washington, Econ. Dept., Seattle, Wash. 98105
DOWNING, Glenn, 2811 Longview Ave., Roanoke, Va. 24014
DOWNING, Glenn D., 1000 S. Pickwick, Springfield, Mo. 65804
DOWNING, Robert E., Gwynedd, Pa. 14936
DOWNS, Anthony, 39 Brinker Rd., Barrington, Ill. 60010
DOWNS, Frederick T., Box 248, University Park, N.M. 88070
DOXEY, George V., York Univ., Dept. of Econ., 2275 Bayview Ave., Toronto 12, Ont., Canada
DOXEY, Willard B., Econ. Dept., Brigham Young Univ., Provo, Utah 84601
DOXTATOR, Daniel W., 325 Thompson Ave., Mountain View, Calif. 94041
DOYLE, Edward V., 14 Michael Dr., Blauvelt, N.Y.
DOYLE, George A., 35 Saxon Rd., Worcester, Mass.
DOYLE, James E., III, 11 Middlesex Ave., Apt. 17C, Metuchen, N.J.
DOYLE, (Mrs.) Marion W., 120 Ardendale Dr., Daly City, Calif.
DOYLE, Robert G., 57 Hampshire St., Methuen, Mass. 01860
DRABBLE, B. J., Bank of Canada, Ottawa 4, Ont., Canada
DRAGONETTE, Joseph E., 4103 Russell Ave., Apt. 2, Mount Rainier, Md. 20822
DRAKE, Charles G., Southern Illinois Univ., Edwardsville, Ill.
DRAKE, Louis S., Southern Illinois Univ., Bus Div., Alton, Ill.
DRANDAKIS, Emmanuel, Cowles Found., 2125 Yale Sta., New Haven, Conn.
DRAY, (Mrs.) Margaret C., 231 Wentworth Ave., Glencoe, Ill. 60022
DREBIN, Allan R., Cornell Univ., Grad. Sch. of Bus. and Pub. Admin., Ithaca, N.Y. 14850
DREES, Franz, Int. Monetary Fund, 19th and H Streets, Washington 25, D.C.
DREIBLATT, David, 321 West 103, New York, N.Y. 10025
DREIMAN, Lawrence S., 5525 Trent St., Chevy Chase, Md. 21620
DRELL, Bernard, 3000 Spout Run Pkwy., Apt. B-411, Arlington 1, Va.
DRENNAN, Mathew P., 90 Park Ter. East, Apt. 6-G, NewYork 34, N.Y.
DRESSLER, Boris G., 622 W. 141st St., New York, N.Y. 10031
DREW, James W., Jr., 163 W. Tompkins, Galesburg, Ill.
DREWRY, L. A., Jr., Col. of Bus. Admin., Univ of Georgia, Athens, Ga.
DREYER, Avon M., Central Michigan Univ., Dept. of Econ., Mount Pleasant, Mich. 48858
DRINKO, Leonard F., 214 Charles, East Lansing, Mich. 48823
DRISCOLL, Daniel M., Jr., Rutgers—The State Univ., Dept. of Econ., 60 College Ave., New Brunswick, N.J. 08903
DRISCOLL, George F., 37 Carmita Ave., Rutherford, N.J.
DRIVER, Beverly L., 429 3rd St., Ann Arbor, Mich. 48104
DROFFNER, Robert G., 724 Everett Ave., West Collingswood, N.Y. 08091
DROKE, Tanya, 4723 9th, N.E., Seattle, Wash. 98105
DRONBERGER, Warren, 56 Chester Pl., New York, N.Y. 10304
DRONSON, John M., 4038 S. Oarner Rd., Lafayette Hill, Pa. 19444
DROUT, William M., Jr., 11915 Taylorcrest Rd., Houston 24, Tex.
DRULEY, Homer L., 205 N. Ridge Ave., Arlington Heights, Ill. 60004

DRUMM, Roger L., Addison-Wesley Publishing Co., Reading, Mass.
DRUMMOND, Ian M., Dept. of Polit. Econ., Univ. of Toronto, Toronto 5, Ont., Canada
DRURY, Horace B., 5220 Spruce Tree Ave., Bethesda, Md. 21811
DRYDEN, Myles M., Univ. of Edinburgh, Dept. of Polit. Econ., Edinburgh, Scotland
DUBEN, Ernest C., 83-30 118th St., Kew Gardens, N.Y. 14478
DUBEY, Akhilesh, 338 Beverley Blvd., Upper Darby, Pa. 19082
DUBEY, Vinod, Econ. Dept., Rm. 1021, Int. Bank for Recon. and Dev., 1818 H St., Washington, D.C. 20433
DUBKE, Marie E., 105 E. Bellows, Mt. Pleasant, Mich. 48858
DuBOFF, Richard B., Bryn Mawr Col., Dept. of Econ., Bryn Mawr, Pa. 19010
DuBOIS, Rene G., 1 Emerson Pl., Boston, Mass. 02114
DUBOV, Irving, Dept. of Agri. Econ., Univ. of Tennessee, Knoxville, Tenn.
DUBRIN, (Mrs.) Chantel deM., Nat. Bur. of Econ. Res., 261 Madison Ave., New York, N.Y. 10016
Du BRUL, Stephen M., General Motors Bldg., Rm. 11-134, Detroit 2, Mich.
DUBY, Thomas E., 321 N. Main St., South Deerfield, Mass. 01373
DUCHARME, Robert G., 400 W. Madison St., Rm. 2500, Chicago, Ill. 60603
DUCHESNEAU, Thomas D., P.O. Box 143, Keene, N.H.
DUCHMAN, John H., Univ. of Chicago, 1369 E. Hyde Park Blvd. (403), Chicago, Ill. 60615
DUCK, Bernard F., Jr., Univ. of Texas, Dept. of Econ., Austin, Tex. 78712
DUCKSTAD, John H., 1801 15th St., South, Moorhead, Minn. 56560
DUDENHOEFFER, Albert E., 5225 Troost Ave., Kansas City 10, Mo.
DUDGEON, Esther J., 4105 Southern Ave., S.E., Apt. 4-C, Washington, D.C. 20020
DUDKO, Stanley J., Belmont Abbey Col., Belmont, N.C. 28012
DUDRA, Michael, St. Francis Col., Loretto, Pa. 15940
DUE, (Mrs.) Jean M., Univ. of Ill., Dept. of Econ., 330 Commerce W., Urbana, Ill.
DUE, John F., Univ. of Illinois, Dept. of Econ., 330 Commerce W., Urbana, Ill.
DUELL, Dennis C., 1105½ W. Oregon, Urbana, Ill. 61801
DUERR, William A., 121 Windsor Pl., Syracuse 10, N.Y.
DUESENBERRY, James S., 4521 Cumberland Ave., Chevy Chase, Md. 20015
DUFFETT, Walter E., Dominion Bur. of Statis., Ottawa, Ont., Canada
DUFFUS, William M., c/o Melvin, 1722 Madison Ave., Evansville, Ind. 47714
DUFFY, James L., Boston Col., Chestnut Hill, Mass. 01012
DUFFY, John A., Jr., 1503 E. Barringer St., Philadelphia, Pa. 19150
DUFFY, John P., Jr., 18 Mill River Rd., Chappaqua, N.Y. 10514
DUFFY, Joseph R., 108 Grand St., Jersey City 2, N.J.
DUFFY, William J., 434 Biddle Ave., Wilkinsburg, Pa. 15221
DUFOUR, F. Philip, 20 Oak St., Old Town, Me. 04468
DUGAN, Dennis J., Brown Univ., Dept. of Econ., Providence 12, R.I.
DUGAN, James E., Dept. of Econ., Univ. of Colorado, Boulder, Colo.
DUGGAN, William A., 7012 Sea Cliff Rd., McLean, Va. 22101
DUGGAR, Jan W., 171-6 Brittain Dr., Tallahassee, Fla.
DUKE, Curtis C., Virginia State Col., Petersburg, Va.
DUKE, Richard M., 3512 Turner Lane, Chevy Chase, Md. 20015
DUKES, James H., U.S. Naval Academy, E.H. and G. Dept., Annapolis, Md. 21402
DULAN, Harold A., Col. of Bus. Admin., Univ. of Arkansas, Fayetteville, Ark.
DULLES, Eleanor L., Spring Hill West, McLean, Va. 22101
DUMAN, Barry L., 9201 Lamont Ave., Elmhurst 73, N.Y.
DUNAYEVSKAYA, Raya, 4482 28th St., Detroit, Mich.
DUNBAR, Forrest S., Jr., 1930 Quintus Dr., Bellmore, L.I., N.Y. 11710
DUNCAN, Delbert J., Sch. of Bus. Admin., Univ. of California, Berkeley 4, Calif.
DUNCAN, Joseph W., 6660 Merwin Rd., Worthington, Ohio 43085
DUNCAN, Peter D., c/o Stanford Res. Inst., Menlo Park, Calif.
DUNCAN, (Mrs.) Priscilla W., Cabot Corp., 125 High St., Boston, Mass. 02100
DUNCAN, Robert C., 430 Rushmore Ave., Mamaroneck, N.Y. 10543
DUNCOMBE, Bruce F., Georgetown Univ., Dept. of Econ., Washington 7, D.C.
DUNCOMBE, Henry, Jr., 46 Valley Rd., Plandome, N.Y. 13137
DUNFORD, Edith, 15 West 11th St., New York, N.Y. 10011
DUNHAM, Scott H., 2155 Ward Way, Woodside, Calif. 95990
DUNKELBERG, William C., 3112 Inst. for Soc. Res., Box 1248, Ann Arbor, Mich.
DUNKMAN, William E., Univ. of Rochester, Morey Hall, Rochester 20, N.Y.
DUNLAP, James W., 2945 Bancroft Rd., Akron, Ohio 44313
DUNLAP, Ruane D., 4410 S. Hagadorn Rd., Okemos, Mich. 48864
DUNLEVY, James A., 806 Colfax St., Evanston, Ill. 60201
DUNLOP, John Thomas, Dept. of Econ., Harvard Univ., Cambridge, Mass. 01922
DUNN, Edgar S., Jr., 7615 Range Rd., Alexandria, Va. 22306
DUNN, LeRoy, Trinity Col., Dept. of Econ., Hartford, Conn.
DUNN, Robert, Jr., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
DUNPHY, Loretta M., 133 Parland Pl., San Antonio 9, Tex.
DUNSDON, Rollin O., Univ. of Illinois, 130 Com. Annex, Urbana, Ill.
DUNSING, Marilyn, Univ. of Illinois, 155 Bevier Hall, Urbana, Ill.
DUNSTAN, Mary F., 412 Highland St., Montevallo, Ala. 35115
DUPREY, James N., 600 N. Lilac Drive, Apt. 414, Minneapolis, Minn. 55422
DUPRIEZ, Gerard, B.P. 158, Univ. Lovanium, Leopoldville XI, Congo Republic
DURAND, David, 212 Follen Rd., Lexington, Mass. 02173
DURAND, Edith E., 212 Follen Rd., Lexington, Mass. 02173
DURBIN, Clarence, Notre Dame Univ., Notre Dame, Ind. 46556
DURBOROW, John R., 2 Horatio St., New York, N.Y. 10014
DURGAN, G. Sturtevant, 11 S. Highland St., West Hartford, Conn. 06090
DURHAM, Floyd, Texas Christian Univ., Dept. of Econ., Ft. Worth, Tex. 76129
DURHAM, Henry C., Jr., U.S. Naval Acad., Dept. of English, Hist. and Gov., Annapolis, Md.
DURIEZ, Philip, Texas Western Col., Dept. of Econ., El Paso, Tex.
DURO, (Mrs.) George, 2551 Eaton, University Hgts. 18, Ohio
DURR, Ernst, Gartenstrasse 2, 85 Nürnberg-Reichelsdorfer Keller, Germany
DUSEK, Jaroslav, 542 W. 112th St., Apt. 1-C, New York, N.Y. 10025
DUSKIN, Gerald L., 28 Gage Rd., East Brunswick, N.J. 06616
DUSTON, Thomas E., 1543 Union-Maine Hwy., Endicott, N.Y.
DUTKA, (Mrs.) Anna B., 39 Claremont Ave., New York 27, N.Y.
DUTTA, Manoranjan, Rutgers State Univ., Col. of Arts., Dept. of Econ., New Brunswick, N.J.
DUTTON, Dean S., 506 Leslie St., Lansing, Mich. 48912
DUVALL, Jerry B., 25511 Oak Dr., Damascus, Md. 20750

DUVALL, Richard M., BEOB 723, Univ. of Texas, Austin, Tex. 78712
DVORAK, Eldon J., Econ. Dept., Long Beach State Col., Long Beach 4, Calif.
DVORAK, Ing. Dusan, Stepanska 30, Prague 1, Czechoslovakia
DVORSKY, Robert W., 173 W. Hillcrest Ave., Havertown, Pa. 19041
DWORETSKY, Joseph H., 300 Wyckoff Ave., Brooklyn 27, N.Y.
DWORKIN, Michael G., 25210 Gardner Ave., Oak Park, Mich. 48649
DWYER, Virginia A., Western Electric Co., 222 Broadway, New York 38, N.Y.
DYCKMAN, Zachary, 1571 46th St., Brooklyn 19, N.Y.
DYDO, John S., 221 E. 84th St., New York 28, N.Y.
DYE, Earl V., 332 Arbor Way, State College, Pa. 16801
DYER, Carl L., North Carolina State Univ., Dept. of Econ., Raleigh, N.C.
DYGERT, Paul K., 36 Ivy Dr., Orinda, Calif. 94563
DYKEMA, Frank E., P.O. Box 1922, University, Ala. 35486
DYL, Edward A., 750 Coleman Ave., Apt. 11, Menlo Park, Calif. 94025
DYM, Charles H., IBM Corp., Advanced Systems Dev. Div., 2651 Strang Blvd., Yorktown Heights, N.Y. 10598
DYMSZA, William A., Sch. of Bus. Adm., Rutgers Univ., 18 Washington Pl., Newark 2, N.J.
DZIADEK, Fred, 71 S. Cassady Ave., Columbus, Ohio
DZIADOSZ, Thomas A., 5917 Loretto Ave., Philadelphia, Pa. 19149

EADS, George C., 560 Prospect St., Apt. 1, New Haven, Conn. 06511
EAGAN, William F., Box 86, Notre Dame, Ind. 46556
EAGLY, Robert V., Econ. Dept., Wayne State Univ., Detroit, Mich.
EAKENS, Robert H. S., 6305 Shoal Creek West Dr., Austin, Tex. 78731
EAKIN, Billy J., 751 41st Ave., San Francisco, Calif. 94121
EAMES, Herbert H., Jr., 40 Rockledge Rd., Hartsdale, N.Y.
EAPEN, A. Thomas, 94-30 60th Ave., Apt. 3-D, Rego Park, N.Y. 11373
EAPEN, (Mrs.) Ama N., 94-30 60th Ave., Apt. 3D, Rego Park, N.Y. 11373
EARL, John F., Dept. of Econ., Univ. of New Brunswick, Fredericton, N.B., Canada
EARL, Thomas E., 932 Canyon Rd., Apt. 2, Logan, Utah 84321
EARLEY, James S., Univ. of Wisconsin, Madison 6, Wis.
EARNEST, Robert C., Sch. of Bus., Florida State Univ., Tallahassee, Fla.
EASON, Warren W., Syracuse Univ., Dept. of Econ., Syracuse, N.Y. 13210
EASSON, (Mrs.) Lelia M., 1621 N. Wakefield St., Arlington 7, Va.
EAST, William F., 6802 Broyhill St., McLean, Va.
EASTBURN, David P., 75 Short Rd., R.D. 3, Doylestown, Pa. 18901
EASTERBROOK, William T., Univ. of Toronto, 273 Bloor St. W., Toronto, Ont., Canada
EASTERLIN, Richard A., Wharton Sch. of Fin., and Com., Univ. of Pennsylvania, Philadelphia 4, Pa.
EASTHAM, George M., 701 Bolton Walk, Apt. 103, Goleta, Calif. 93017
EASTMAN, H. C., Dept. of Polit. Econ., Univ. of Toronto, 100 St. George St., Toronto, Ont., Canada
EASTMAN, Sheila B., York Univ., 2275 Bayview Ave., Toronto 12, Ont., Canada
EASTON, Allan, 24 Middle Neck Rd., Roslyn, N.Y. 11576
EASTWOOD, Richard T., Texas Medical Center, 406 Jesse James Bldg., Houston 25, Tex.
EASTWOOD, Robert, Sch. of Bus., Columbia Univ., New York 27, N.Y.
EATHERLY, Billy J., Mississippi State Univ., P.O. Box 892, State College, Miss. 39762
EATON, Edgar I., 6481 Lyric Lane, Falls Church, Va. 22044
EATON, (Mrs.) Frances N., Rust Col., Holly Springs, Miss. 38635
EATON, John R., London Grad. Sch. of Bus. Studies, 28 Northumberland Ave., London, W.C. 2, England
EATON, Robert C., 221 Alexander Blvd., Elmhurst, Ill.
EBEL, Jorg, Universitat Sem. fur Theoret. VWL, 6 Saarbrucken, Germany
EBEL, Robert D., Purdue Univ., Dept. of Econ., Krannert Bldg., West Lafayette, Ind.
EBERDT, Sister Mary L., Marycrest Col., 1607 W. 12th St., Davenport, Iowa 52804
EBERHART, E. Kingman, Col. of Wooster, Dept. of Econ., Wooster, Ohio
EBERLEIN, Otto P., 3699 Stoneleigh Rd., Cleveland Heights, Ohio 44121
EBERLY, Charles H., Punahou Sch., Honolulu 14, Hawaii
EBERT, Robert R., 3748 W. 178th St., Cleveland, Ohio 44111
EBY, Barnett S., 703 W. Broadway, Maryville, Tenn. 37801
ECHEVARRIA-SALVAT, Oscar, P.O. Box 3559, Washington, D.C. 20007
ECKARD, Edwin W., 4830 Woodvale Dr. N.W., Atlanta 5, Ga.
ECKAUS, Richard S., Center of Int. Studies, Massachusetts Inst. of Tech., Cambridge, Mass. 01922
ECKER, Paul, Meadow Pl., Carmel Valley, Calif.
ECKERT, James B., 4584 Indian Rock Ter N.W., Washington 7, D.C.
ECKERT, Ross D., 1978 Linda Flora Dr., Los Angeles 24, Calif.
ECKLER, A. Ross, 3643 Brandywine St. N.W., Washington, D.C.
ECKLES, Elvis L., 11 Ferndale St., Albany 8, N.Y.
ECKLES, Robert W., Southern Illinois Univ., Edwardsville, Ill. 62025
ECKLEY, Robert S., 3911 N. Bigelow, Peoria, Ill.
ECKSTEIN, Alexander, Dept. of Econ., Univ of Michigan, Ann Arbor, Mich.
ECKSTEIN, Otto, Coun. of Econ. Advisers, Washington, D.C. 20506
ECKSTEIN, Peter C., 63-C Dana St., Cambridge, Mass. 02138
ECKSTEIN, Shlomo, c/o Israel Embassy, Rio Rhin 57, Mexico 5, D.F., Mex.
ECONOMOS, Gus L., Col. of Com., De Paul Univ., 25 E. Jackson Blvd., Chicago, Ill. 60604
EDDE, Richard, 311 E. 50th St., New York, N.Y. 10022
EDDIE, Scott M., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
EDDISON, John C., 202 Primrose St., Chevy Chase, Md. 20015
EDDY, Harry L., Assoc. of Amer. Railroad. Bur. of Railway Econ., Transportation Bldg., Washington 6, D.C.
EDDY, Richard E., 805 S. Lincoln Ave., Urbana, Ill. 61801
EDEL, Matthew D., 171-07 84th Rd., Jamaica 32, N.Y.
EDEL, William C., 325 Hillside Ave., Elizabeth town, Pa. 17022
EDELMAN, Louis A., 3900 16th St. N.W., Apt 639, Washington 11, D.C.
EDELMAN, Milton T., Southern Illinois Univ., Carbondale, Ill.
EDELSON, Noel M., 151 Dogburn Rd., West Haven, Conn. 06091
EDELSTEIN, Michael, 3422 Powelton Ave., Philadelphia, Pa. 19104
EDEN, Arthur, 9708 Dameron Dr., Silver Spring, Md.
EDEN, Philip, 1303 Walnut St., Berkeley, Calif. 94709
EDENS, David G., Univ. of Connecticut, Storrs, Conn. 06268
EDER, George J., Univ. of Mich., Grad. Sch. of Bus. Admin., Ann Arbor, Mich.
EDERER, Robert J., Rosary Hill Col., Buffalo 26, N.Y.
EDGE, Stephanie K., Univ. of Kent, Eliot Col., Canterbury, Kent, England
EDGEL, Ralph L., 601 N.E. Vassar Dr., Albuquerque, N.M. 87106
EDGMAND, Michael, Michigan State Univ.,

Dept. of Econ., Marshall Hall, East Lansing, Mich.
EDGREN, John A., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
EDMINSTER, (Mrs.) Diane K., Econ. Dept., Univ. of Utah, Salt Lake City, Utah
EDMINSTER, Robert, Econ. Dept., Univ. of Utah, Salt Lake City, Utah
EDMISTON, Henry H., P.O. Box 139, Kansas City, Mo.
EDMONDS, Robert H., 2369 Markham Ave., San Jose 25, Calif.
EDMUNDS, Ervin R., Columbia Basin Col., 2600 N. Chase, Pasco, Wash. 99301
EDMUNDS, Stahrl W., 1681 Stone Canyon Rd., Los Angeles 24, Calif.
EDOZIEN, Emmanuel C., 324 John St., Ann Arbor, Mich.
EDWARDS, Alfred L., Off. of the Secretary, U.S. Dept. of Agric., Washington, D.C. 20250
EDWARDS, Bruce E., 7812 Mary Knoll Ave., Bethesda, Md. 21811
EDWARDS, Charles E., Univ. of South Carolina, Sch. of Bus. Admin., Columbia, S.C. 29208
EDWARDS, Corwin D., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
EDWARDS, Daniel J., 4916 44th St., N.W., Washington, D.C. 20016
EDWARDS, Edgar O., Rice Univ., Dept. of Econ., Houston, Tex.
EDWARDS, Edward E., Town Hill Rd., Nashville, Ind. 47448
EDWARDS, Ford K., 5301 Dorset Ave., Chevy Chase, Md. 21620
EDWARDS, Franklin, Main Treasury, Rm. 4458, Washington, D.C. 20220
EDWARDS, (Mrs.) Gertrud G., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
EDWARDS, James D., Dept. of Acct. and Fin. Admin., Michigan State Univ., East Lansing, Mich.
EDWARDS, Richard P., 689 Winona Blvd., Rochester, N.Y. 14617
EDWARDS, Thomas L., 1864 Kirby Rd., McLean, Va. 22101
EDWARDS, Wallace I., 5431 Tallawanda Lane, Oxford, Ohio 45056
EFROYMSON, Clarence W., RR 2, Box 292, Carmel, Ind. 46032
EGAN, Douglas, State Univ. of New York at Buffalo, Sch. of Bus. Admin., Crosby Hall, Library Circle, Buffalo 14, N.Y.
EGAN, Francis J., 35-33 158th St., Flushing 58, N.Y.
EGAN, James P., Le Moyne Col., Le Moyne Hgts., Syracuse, N.Y.
EGAN, John W., 2336 E. 70th Pl., Chicago, Ill. 60649
EGAND, Leo M., 155 W. 68th St., New York City, N.Y. 10023
EGBUCHE, Bennett, Fed. Off. of Statis., Lagos, Nigeria
EGERER, Gerald V., Sonoma State Col., Div. of Soc. Sci., Rohnert Park, Calif.
EGGERS, E. T. Georgia State Col., Atlanta 3, Ga.
EGGERS, Melvin A., Econ. Dept., Syracuse Univ., Syracuse, N.Y.
EGLE, Walter P., Dept. of Econ., Univ. of Cincinnati, Cincinnati, Ohio
EHEMANN, G. Christian, 2733 Linden Rd., Iowa City, Iowa
EHRENBERG, Ronald G., 20 Stuyvesant Oval, New York, N.Y.
EHRENHALT, Samuel M., 2201 Ave., L, Brooklyn, N.Y. 11210
EHRENREICH, Joseph W., Univ. of Southern California, Grad. Sch. of Bus. Admin., University Park, Los Angeles, Calif. 90007
EHRLICH, Edna E., 410 Prospect St., East Orange, N.J.
EHRLICH, Morton, 1655 Flatbush Ave., Apt. C1702, Brooklyn, N.Y. 11210
EHRLICH, Otto H., 410 Prospect St., East Orange, N.J. 07017
EHRLING, Robert F., 88-35 183rd St., Jamaica, N.Y. 11432
EICHER, Carl K., Econ. Dev. Inst., Univ. of Nigeria, Enugu Campus, P.M.B. 1080, Enugu, Nigeria
EICHNER, Alfred S., 511 W. 112th St., Apt. 17, New York 25, N.Y.
EIDMAN, Vernon R., Oklahoma State Univ., Dept. of Agric. Econ., Stillwater, Okla. 74075
EILBIRT, Henry, 17 Lexington Ave., New York 10, N.Y.
EILBOTT, Peter, 79-B Parkway Dr., W., East Orange, N.J. 07017
EILBOTT, Richard, 1 Sickles St., New York 40, N.Y.
EILENBERG, Howard, 196-32 53rd Ave., Fresh Meadows 65, N.Y.
EILENSTINE, Donald L., Box 567, Baldwin City, Kan. 66006
EILERT, John W., Memphis State Univ., Bur. of Bus. Res., Memphis, Tenn. 38111
EINHORN, Henry A., 140 Hepburn Rd., Apt. 9-L, Clifton, N.J.
EINZIG, Robert S., 1937 St. Francis Way, San Carlos, Calif.
EIS, Carl, 1463 40th St., Brooklyn, N.Y. 11218
EISENBERG, Richard K., 6408 Tone Dr., Bethesda, Md. 20034
EISENBERG, Walter L., 550 Fifth Ave., New York 36, N.Y.
EISENMENGER, Robert W., Fed. Res. Bank of Boston, 30 Pearl St., Boston, Mass.
EISENPREIS, Alfred, Allied Stores Corp., 401 Fifth Ave., New York 16, N.Y.
EISENSTADT, Samuel, Value Line Investment Survey, 5 E. 44th St., New York 17, N.Y.
EISINGER, Don W., 1236 Fidelity Bldg., Indianapolis, Ind. 46204
EISNER, Robert, Dept. of Econ., Northwestern Univ., Evanston, Ill. 60201
EITEMAN, David K., Grad. Sch. of Bus., Univ. of California, Los Angeles 24, Calif.
EITEMAN, Wilford J., Univ. of Michigan, Sch. of Bus Admin., Ann Arbor, Mich.
EKEBALD, Frederick A., Northwestern Univ., 208 Bus. Bldg., Evanston, Ill. 60201
EKEN, Mark S., 601 19th St. N.W., Washington 6, D.C.
EKKER, Charles, Cx Postal 721, Vitoria ES, Brazil
EKLAND, Leonard M., 18260 Northlawn Ave., Detroit, 21, Mich.
ELAC, John C., 2814 Hillegass Ave., Berkeley, Calif. 94705
ELAM, Houston G., 230 Jay St., Apt. 15-G Brooklyn, N.Y. 11201
ELANDER, Eugene F., 224 Fernwood Ave., Dayton, Ohio 45405
ELBERFELD, Robert A., 1135-1/2 11th Ave., Huntington, W.Va.
ELBING, Alvar O., Jr., Amos Tuck Sch. of Bus. Admin., Dartmouth Col., Hanover, N.H. 03755
ELCHIBEGOFF, Ivan M., 4206 Kaywood Dr., Mt. Rainier, Md. 20822
EL DALY, El Sayed A. H., 555 Davidson Rd., University Hgts., New Brunswick, N.J.
ELDER, Robert F., RFD 5, Governors Island, Laconia, N.H. 03246
ELDON, Donald, 420 Cloverdale Rd., Ottawa, Ont., Canada
ELDRIDGE, Douglas H., NBER, 261 Madison Ave., New York, N.Y. 10016
ELEK, Peter S., 1 Rodney Rd., Rosemont, Pa.
ELEY, Joseph J., 350 Lexington Ave., Suite 702, New York 16, N.Y.
ELGASS, George A., Chrysler Corp., P.O. Box 1919, Detroit, Mich. 48231
ELGERE, Peter, 61-15 97th St., Rego Park, N.Y.
ELIADES, Evangelos A. E., c/o Bank of Greece, Athens, Greece
ELISON, Leo M., Colgate Univ., Hamilton, N.Y. 13346
EL-KAMMASH, Magdi M., North Carolina State Col., Sch. of Gen. Studies, Dept. of Econ., Raleigh, N.C.
ELKIN, David B., 310 West 106 St., New York, N.Y. 10025
ELKIN, Philip, Ferris State Col. (Com.), Big Rapids, Mich. 49307
ELLENBERG, David B., 5-2C Meadowbrook Village, Plainfield, N.J. 07062
ELLENBOGEN, (Mrs.) Gladys C., 217 Columbus Ave., Hasbrouck Heights, N.J.
ELLENBOGEN, Jack, 217 Columbus Ave., Hasbrouck Heights, N.J. 08040

ELLER, Cornelius A., Le Moyne Col., Le Moyne Hgts., Syracuse, N.Y. 13214
ELLESON, Ruth, 40-66 Ithaca St., Elmhurst 73, N.Y.
ELLICKSON, Donald L., Wisconsin State Univ., Dept. of Econ., Eau Claire, Wis.
ELLIOT, George F., Wilkes Col., Wilkes-Barre, Pa.
ELLIOTT, Daniel A. A., Dept. of Educ., Monrovia, Liberia
ELLIOTT, Edith A., 419 Jessamine Blvd., Daytona Beach, Fla. 32018
ELLIOTT, George A., 303 Clemow Ave., Ottawa, Ont., Canada
ELLIOTT, John E., Dept. of Econ., Univ. of Southern California, Los Angeles 7, Calif.
ELLIOTT, Lloyd J., Southern Regional Coun., 5 Forsyth St., N.W., Atlanta, Ga. 30303
ELLIOTT, Robert D., 2916 Argyle Dr., Alexandria, Va.
ELLIOTT-JONES, Michael F., 18 Prospect Ave., Greenwich, Conn.
ELLIS, George H., Fed. Res. Bank, 30 Pearl St., Boston 6, Mass.
ELLIS, Howard S., USAID Brazil, APO New York 09676
ELLIS, Ira T., Woodbrook, 116 Country Club Dr., Wilmington 3, Del.
ELLIS, Paul W., 310 N. Decatur, Olympia, Wash. 98501
ELLIS, Perry, Chapman Col., General Delivery, Beale Air Force Base, Calif. 95903
ELLIS, R. Wendell, Westminster Col., Fulton, Mo.
ELLIS, Richard K., Westminster Col., Fulton, Mo. 65251
ELLIS, Robert, Jr., 213 S. Dixon St., Carbondale, Ill.
ELLSBERG, Daniel, Amer. Embassy, APO San Francisco 96243
ELLSWORTH, Heber M., 1933 Bidwell Ave., Sacramento 19, Calif.
ELLSWORTH, Paul T., Univ. of Wisconsin, Dept. of Econ., 365 Soc. Sci. Bldg., Madison 6, Wis.
EL MALLAKH, Rogaie, Univ. of Colorado, Dept. of Econ., Boulder, Colo.
ELMENDORF, Milburn C., Evangel Col., Box 407, Springfield, Mo.
ELMOS, Mary B., 629 Noyes St., Evanston, Ill. 60201
EL-NAGGAR, Said, UNCTAD, Res. Div., European Off. of UN, Palais des Nations, Geneva, Switzerland
ELOISE, Sister Mary, Mundelein Col., 6363 N. Sheridan Rd., Chicago, Ill. 60626
EL-RAHAIE, Mahmoud, Ministry of Nat. Plan., Cairo, Egypt
ELSASSER, Robert W., 8004 Freret St., New Orleans, La. 70118
ELSBERND, Lawrence B., 4805 Bristow Dr., Anandale, Va. 22003
ELSE, Willis I., 602 E. Evergreen Ave., Mt. Prospect, Ill. 60057
EL-SHERBINI, Abdel A., UNTAB, P.O. Box 565, Amman, Jordan
ELSNER, Curt M., 21 Deborah Way, Fanwood, N.J. 07023
ELWERT, Bert E., 4454 N. Parkside Ave., Chicago, Ill. 60630
ELY, J. Edward, U.S. AID/Blantyre, c/o State Dept., Washington, D.C.
ELY, (Mrs.) Margaret H., 21 Cross Rd., Waterford, Conn. 06385
ELZINGA, Kenneth, 3301 Portage St., Kalamazoo, Mich. 49001
EM, Zyong Zin, 29 Peter Jordan Str., 1190 Vienna XIX, Austria
EMANUEL, Edward E., 1530 Chestnut St., Philadelphia, Pa. 19102
EMERLING, Edward G., St. Bonaventure Univ., P.O. Box 13, St. Bonaventure, N.Y. 14778
EMERSON, David L., 492 Sand Creek Rd., Albany, N.Y. 12205
EMERSON, Frank C., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn.
EMERSON, Hugh N., Albright Col., Bus. and Econ. Dept., Reading, Pa. 19604
EMERSON, M. Jarvin, Dept. of Econ. and Soc., Kansas State Univ., Manhattan, Kans.
EMERSON, Robert D., 5116 Woodlawn Ave., Chicago, Ill. 60615
EMERY, Robert F., 3421 Shepherd St., Chevy Chase, Md. 21620
EMI, Koichi, Hitotsubashi Univ., Inst. of Econ. Res., Kunitachi, Tokyo, Japan
EMMER, Robert E., US AID Amer. Embassy, Amman, Jordan
ENCARNACION, Jose, Jr., Univ. of the Philippines, Sch. of Econ., Quezon City, Philippines
ENCHES, Evelyn L., 940 E. Mendocino, Altadena, Calif.
ENDLER, Oscar L., 4404 46th St., N.W., Washington, D.C. 20016
ENG, Maximo V. F., 504 W. 112th St., New York 25, N.Y.
ENGBERG, Holger, 375 Riverside Dr., Apt 14-D, New York 25, N.Y.
ENGBERG, Vernon L., Univ. of Houston, Dept of Transp., Int. Bus. and Resources, Houston, Tex. 77004
ENGEL, James F., Ohio State Univ., Col. of Com. and Admin., 1775 South College Rd., Columbus, Ohio 43210
ENGEL, William J., Jr., 5106 Michigan, Kansas City 30, Mo.
ENGEN, Gunnar, c/o Don Cecilio Sagarna, Espalter 5, Madrid, Spain
ENGERMAN, Stanley L., Univ. of Rochester, Dept. of Econ., Rochester, N.Y. 14627
ENGLAND, Richard W., 53047 Venus Dr., Utica, Mich. 48087
ENGLISH, Donald, 220 Triphammer Rd., Ithaca, N.Y.
ENGLISH, Harry E., 757 Sun Life Bldg., Montreal, P.Q., Canada
ENGLISH, John C., Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
ENGLISH, Joseph B., 410 State Off. Bldg., 65 Court St., Buffalo, N.Y. 14202
ENKE, Ernest L., Univ. of Missouri, 331 Bus. and Pub. Admin., Columbia, Mo. 65202
ENKE, Stephen, 2767 N. Quincy St., Arlington, Va. 22207
ENNERFELT, Per G., Angsvägen 21, Saltsjö-Storängen, Nacka, Sweden
ENOS, Edward S., c/o RMK-BRJ, APO San Francisco 96243
ENOS, John L., Magdalen Col., Oxford, England
ENOS, Joseph J. Jr., 1305 S. Brawley, Fresno, Calif. 93706
ENRICO, Raul A., JuJuy 313, DTO. L, Buenos Aires, Argentina
ENSEKI, Frances, 1825 New Hampshire Ave., N.W., Washington, D.C.
ENSENAT, Alfonso, Paseo del Prado 24, Madrid 14, Spain
ENSIGN, Frank B., Jr., 1795 Windsor Rd., San Marino, Calif.
ENSLEY, Grover W., Nat. Ass. Mutual Savings Bank, 200 Park Ave., New York 17, N.Y.
ENTENBERG, Robert D., Univ. of Denver, Col. of Bus. Admin., Denver 2, Colo.
ENTHOVEN, Alain C., 3203 Alabama Ave., Alexandria, Va.
ENZER, Hermann, 7131 Kismet Pl., Dayton, Ohio 45424
EPP, Donald J., 1415 N. 41st St., Lincoln, Neb. 68503
EPPLEY, Martha A., 623 Lucknow Rd., Harrisburg, Pa. 17110
EPPS, Thomas W., Duke Univ., Dept. of Econ., Durham, N.C. 27706
EPSTEIN, Jose D., 6029 Onondaga Rd., Washington 16, D.C.
EPSTEIN, Joseph B., 1108 Schindler Dr., Silver Spring, Md.
EPSTEIN, Lenore A., 2206 N. Taft St., Arlington 1, Va.
EPSTEIN, Lionel, 10707 Huntley Ave., Silver Spring, Md. 20902
ERB, Emerson C., Jr., Abadan Inst. of Tech., Abadan, Iran
ERB, Norman H., 422 N. Indiana Ave., Apt. 2, Bloomington, Ind.
ERB, Richard D., 722 University Dr., Menlo Park, Calif.
ERBE, Rene, O.E.E.C., 2 Rue André Pascal, Paris 16, France
ERCEG, John J., 2476 Dysart Rd., University Hgts. 18, Ohio

ERDMAN, Henry E., Div. of Agric. Econ., Univ. of California, Berkeley 4, Calif.
ERICKSON, Edward W., North Carolina State Univ., 203 Patterson Hall, P.O. Box 5368, Raleigh, N.C. 27607
ERICKSON, Roland A., General Foods Corp., 250 North St., White Plains, N.Y. 10602
ERICKSON, Victor, 5512 Hoover St., Bethesda, Md. 21811
ERICKSON, W. Bruce, Bowling Green State Univ., Dept. of Econ., Bowling Green, Ohio 43402
ERICSON, Richard F., 12613 Bunting Lane, Bowie, Belair, Md. 21014
ERIKSEN, Conrad, 421 Wickham, Manhattan, Kans.
ERIKSSON, John R., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
ERION, Gene L., Univ. of Alaska, Col. of Bus. and Econ., College, Alaska 99735
ERLANDSON, Gordon, North Dakota State Univ., Dept. of Agric. Econ., Fargo, N.D. 58103
ERLICH, Alexander, Program on East Central Europe, 431 W. 117th St., New York 27, N.Y.
ERLON, Leo A., Rockland Community Col., Suffern, N.Y. 10901
ERNST, Arthur M., 197 Elmhurst Dr., Orchard Park, N.Y. 14127
ERNST, Maurice C., 4320 Starr Jordan Dr., Annandale, Va. 22003
ERNSTOFF, Lois E., 1545 18th St., N.W., Apt. 705, Washington, D.C. 20036
ERON, Joseph J., 100 Oakridge Dr., Solvay, N.Y. 13465
ER SELCUK, Muzaffer M., 2238 Union St., Lafayette, Ind.
ERTELL, Merton W., Sch. of Bus. Admin., Univ. of Buffalo, Buffalo, N.Y.
ERWIN, Carlos C., Univ. of Kentucky, Col. of Com., Lexington, Ky. 40502
ESBITT, Milton, Hamilton Col., Dept. of Econ., Clinton, N.Y. 13323
ESCHER, Albert J., North Park Col., Dept. of Econ., 5125 N. Spaulding Ave., Chicago, Ill.
ESCOBAR, Gustavo, Edificio-Polar-Piso 13, Plaza Venezuela, Caracas, Venezuela
ESCOUBE, William E., 301 G St. S.W., Apt. 416, Washington 24, D.C.
ESKEW, Henry L., Jr., 7928 Jansen Dr., Springfield, Va. 22150
ESKIN, Gerald J., 73 Arthur Ave., S.E., Minneapolis, Minn. 55414
ESLICK, Donald F., Marquette Univ., Dept. of Econ., Milwaukee, Wis. 53233
ESPADAS, Orlando T., Univ. of Puerto Rico, Dept. of Econ., Rio Piedras, P.R.
ESPOSITO, Louis A., 12 Parsons St., Brighton, Mass. 02324
ESREY, (Mrs.) Julie C., 10 Fox Chase Rd., Madison, N.J. 07940
ESSER, Hans A., Gottesweg 177, 5000 Cologne, West Germany
ESSIG, James L., 115 2nd St., Dumont, N.J. 07628
ESTANISLAO, Jesus, Inst. of Econ. Dev. and Res., Univ. of the Philippines, Diliman, Quezon City, Philippines
ESTES, James E., 1645 Joiner Rd., Columbia, S.C. 29209
ESTEVE-SERRANO, Tomas A., 79 Serrano St., Madrid 6, Spain
ESTEVES, Vernon R., Gov. Dev. Bank for P.R., Box 4591, San Juan, P.R.
ESTLE, Edwin F., 117 Butler Ave., Wakefield, Mass. 01880
ESWINE, Harold M., Kent State Univ., Kent, Ohio
ETTIN, Edwin C., Fed. Res. System, Bd. of Gov., Banking Sec., Div. of Res. Stat., Washington, D.C. 20551
ETZEL, Walter T., Jr., 100 N.E. 1st Ave., Miami 32, Fla.
EUCHARIA, Sister Maria, Brentwood Col., Brentwood, L.I., N.Y. 11717
EULER, Roger S., c/o Amer. Consulate Gen. Frankfurt, Amer. Embassy, Warsaw, APO 757, New York 09757
EVANS, Boyd A., Jr., Middle Tennessee State Univ., Box 21, Murfreesboro, Tenn. 37130
EVANS, Chester E., Wayne State Univ., Sch. of Bus. Admin., Detroit, Mich. 48202
EVANS, G. Stanley, Kaiser Steel Corp., P.O. Box 58, Oakland 4, Calif.
EVANS, George H., Jr., Johns Hopkins Univ., Baltimore, Md. 21218
EVANS, Gordon W., Dept. of State, USAID/ACCRA, Washington, D.C. 20521
EVANS, Homer C., Dept. of Agric. Econ., West Virginia University, Morgantown, W.Va.
EVANS, James G., Sr., 4641 N. 23rd Rd., Arlington, Va.
EVANS, John L., 8022 Ashworth, N., Seattle, Wash. 98103
EVANS, John S., State Univ. of South Dakota, Sch. of Bus., Vermillion, S. Dak. 57069
EVANS, Keith D., 8218 Encino Ave., Northridge, Calif.
EVANS, Michael K., Univ. of Pennsylvania, Econ. Dept., Wharton Sch., Philadelphia, Pa.
EVANS, Robert, Jr., Brandeis Univ., Dept. of Econ., Waltham, Mass. 02154
EVANS, Robert G., 69 Wendell St., Cambridge, Mass. 02138
EVANS, Robert L., 19471 Newhaven Dr., Huntington Beach, Calif.
EVANS, William S., 105 Hollow Tree Ridge Rd., Noroton Heights, Conn. 06058
EVANS, Wilmoth D., Cornell Univ., 364 Ives Hall, Ithaca, N.Y. 14850
EVENSON, Robert, 5110 Kenwood Ave., Chicago, Ill. 60615
EVERETT, Henry J., Abraham and Straus, Brooklyn 1, N.Y.
EVERETT, Richard W., Mktg. Services, Continental Can Co., Inc., 633 Third Ave., New York, N.Y. 10017
EVERETT, Winthrop B., 1046 Edgemoor Ct., Lancaster, Pa.
EVINS, William D., 136 S. Madison, La Grange, Ill. 60525
EWALD, Henry P., Jr., 4007 Connecticut Ave., N.W., Washington, D.C. 20008
EWERS, Joseph R., President's Off., Indiana Univ., Bloomington, Ind. 47401
EWERT, David C., 132 Thornbush Dr., West Lafayette, Ind. 47906
EWING, John B., Woodley Park Towers, 2737 Devonshire Pl., N.W., Washington 8, D.C.
EXTER, John Edward, 290 Boulevard, Mountain Lakes, N.J. 07046
EYERMAN, (Mrs.) Lisbeth J., 61-40 Saunders St., Rego Park, N.Y. 13679
EYSENBACH, Mary L., 2302 Harvard St., Palo Alto, Calif. 94306
EYSTER, Elvin S., Sch. of Bus., Indiana Univ., Bloomington, Ind.
EZEKIEL, Mordecai J. B., 5312 Allandale Rd., Washington, D.C. 20016
EZIGBO, Emmanuel O., Ministry of Econ. Plan., Enugu, Nigeria
EZZELL, Austin B., 2616 Westmont Blvd., Columbus 21, Ohio

FABELLA, Armand V., Jose Rizal Col., Mandaluyong, Rizal, Philippines
FABIAN, Robert G., Florida Atlantic Univ., Col. of Soc. Sci., Boca Raton, Fla. 33432
FABIAN, Tibor, 551 Riverside Dr., E., Princeton, N.J. 08540
FABRICANT, Ruth A., 2401 H St., N.W., Washington, D.C.
FABRICANT, Solomon, Nat. Bur. of Econ. Res., 261 Madison Ave., New York 16, N.Y.
FABRYCY, Mark Zdzislaw, 2 Washington Square Village, New York, N.Y. 10012
FACKLER, Walter D., Grad. Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
FADEN, Arnold M., Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa 50010
FAGAN, Elmer D., 180 Heather Lane, Palo Alto, Calif.
FAGERSTROM, Ray C., 1605 Milvia St., Apt. D., Berkeley, Calif. 94709
FAHEY, Joseph R., Weston Col., Weston, Mass. 01985
FAHL, Henry A., Eastern Central Motor Carriers, Asso., P.O. Box 3600, Akron, Ohio 44310
FAHLE, Vernon L., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh, Pa. 15213
FAHLSTROM, Lawrence H., Jr., Donnelly College, 1236 Sandusky, Kansas City, Kans.
FAIN, Douglas M., 702 Forest Glen Rd., Albany, Ga. 31705
FAIR, Marvin Luke, 3412 O St., N.W., Washington, D.C. 20007

FAIR, Ray C., 541 School St., Belmont, Mass. 02178
FAIRBANKS, Robert P., Northern Illinois Univ., Dept. of Econ., DeKalb, Ill. 60115
FAIRCHILD, Fred R., Leetes Island, Old Quarry Rd., Guilford, Conn. 06437
FALCK, Edward, 1625 Eye St., N.W., Washington, D.C.
FALCON, Walter P., Harvard Univ., Dept. of Econ., 1737 Cambridge St., Cambridge, Mass.
FALCONE, Nicholas S., 110 E. 42nd St., New York, N.Y. 10017
FALERO, Frank, Jr., 156-6 Herlong Dr., Tallahassee, Fla.
FALICK, Abraham, 11661 Montana, Los Angeles, Calif. 90049
FALK, I. S., Yale Sch. of Medicine, Dept. of Epid. and Public Health, 60 College St., New Haven 11, Conn.
FALK, Karl L., Fresno State Col., Fresno, Calif.
FALKOWITZ, David, 327 Westheimer, Apt. 3, Houston 6, Tex.
FALKOWSKI, Daniel C., 235 W. 4th St., New York 14, N.Y.
FALKSON, Louis M., Cornell Univ., Dept. of Econ., Ithaca, N.Y. 14850
FALLENBUCHL, Zbigniew M., Univ. of Guyana, Georgetown, British Guiana
FAMA, Eugene F., Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
FAMADAS, José, P.O. Box 752, Flushing, N.Y. 11352
FAN, (Mrs.) Jenny Neng Ying, 400 S. Union St., Burlington, Vt.
FAN, Kenneth Kuan-Ling, 400 S. Union St., Burlington, Vt.
FAN, Yuan-sheng, 400 S. Union St., Burlington, Vt.
FAND, David I., Dept. of Econ., Univ. of Buffalo, Buffalo 14, N.Y.
FARAH, Ghazi T., Wisconsin State Univ., Econ. Dept., Whitewater, Wis. 53190
FARB, Warren E., Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
FARBER, David J., 10611 Cavalier Dr., Silver Spring, Md. 20901
FARBER, Evelyn W., 10611 Cavalier Dr., Silver Spring, Md. 20901
FARBMAN, Michael E., New York State Sch. of Ind. and Labor Rela., Cornell Univ., P.O. Box 34, Ithaca, N.Y. 14850
FARBO, (Mrs.) Eleanor B., 575A Prospect St., New Haven, Conn.
FARHI, Victor R., 114-59 225th St., Cambria Heights, N.Y. 11411
FARIOLETTI, Marius, 4822 3rd St., N., Arlington, Va.
FARLEY, John T., Col. of Great Falls, Great Falls, Mont.
FARLEY, John U., 6412 Aylesboro Ave., Pittsburgh 17, Pa.
FARLEY, Noel J. J., Goucher Col., Dept. of Econ., Towson, Md. 20691
FARLEY, Rawle, United Nations, P.O. Box 358, Tripoli, Libya
FARMAN, Wilson L., 30 Payne St., Hamilton, N.Y. 13346
FARMER, Richard N., Indiana Univ., Grad. Sch. of Bus., Bloomington, Ind.
FARNESS, Donald H., Oregon State Univ., Dept. of Econ., Corvallis, Ore.
FARNHAM, Wells, Stein Roe and Farnham, 135 S. LaSalle St., Chicago, Ill.
FARNSWORTH, Clyde H., Jr., Univ. of Missouri, Dept. of Econ., Columbia, Mo.
FARNSWORTH, Helen C., Stanford Univ., Food Research Inst., Stanford, Calif. 94305
FARNSWORTH, Robert S., Matson Navigation Co., Research Dept., 215 Market St., San Francisco 5, Calif.
FARR, Grant N., Pennsylvania State Univ., Dept. of Econ., University Park, Pa. 16802
FARR, Llewellyn G., 755 Fenimore St., Brooklyn, N.Y. 11203
FARRAG, Nour El-Din, Min. of Fin. and Ind., P.O. Box 9, Kuwait City, Kuwait
FARRAR, Curtis, 3561 Raymoor Rd., Kensington, Md. 20795
FARRAR, Floyd A., 1721 Lucas Ferry St., Athens, Ala.
FARRAR, Richard, 122 W. 13th St., New York 11, N.Y.
FARRELL, John J., Augustinian Col., 3900 Harewood Rd., N.E., Washington, D.C. 20017
FARRELL, Joseph F., 50 Ideal Ct., East Greenwich, R.I. 02818
FARRELL, Kenneth R., Univ. of California, 319 Giannini Hall, Berkeley 4, Calif.
FARRELL, M. J., Gonville and Caius Col., Cambridge, England
FARRELL, Raymond A., 587 Oriole Pkwy., Toronto 12, Ont., Canada
FARRER, Dean G., 128 S. Edward Ave., Apt. B, Fullerton, Calif. 92633
FARRIS, Martin T., Arizona State Univ., Col. of Bus. Admin., Tempe, Ariz.
FARRIS, Paul Leonard, Sch. of Agric., Purdue Univ., Dept. of Agric. Econ., West Lafayette, Ind. 47178
FARSTAD, Edmund, 5303 Camberley Ave., Bethesda, Md. 21811
FARWELL, Loring C., 735 Happ Rd., Northfield, Ill.
FASICK, Clyde A., 517 Whitehall Rd., Athens, Ga. 30601
FASSBERG, Harold E., 7224 Millwood Ct., Bethesda, Md. 21811
FATEMI, Ali M.S., Univ. of Akron, Dept. of Econ., Akron, Ohio 44304
FATJO, General C., Pierce Manor Apts., 400 Irvington Ave., Apt. 203D, Elizabeth, N.J. 07208
FATTORE, James L., Jr., St. Joseph's Col., 810-44 Econ. Dept., Calumet Campus, 4721 Indianapolis Blvd., East Chicago, Ind.
FAUCETT, Jack G., 5104 River Hill Rd., Washington 16, D.C.
FAUCETT, Philip M. Jr., ROCAP, American Embassy, Guatemala City, Guatemala
FAULHABER, Bertold, 31 Larch Lane, Massapequa Park, L.I., N.Y. 11762
FAULHABER, R. W., 5653 S. Harper, Chicago, Ill. 60637
FAULKENDER, Robert E., Windham College. Econ. Dept., Putney, Vt. 05346
FAULKNER, (Mrs.) Connie P., Western Washington State Col., Dept. of Econ., Bellingham, Wash. 98225
FAULKNER, J. Kaye, Western Washington State Col., Dept. of Econ., Bellingham, Wash 98225
FAULKNER, Michael J., American Int. Underwriters Co., 2 Mainzerlandstrasse 6000, Frankfurt/Main, West Germany
FAUVER, Robert C., 105 Normandy Dr., Silver Spring, Md. 20901
FAVA, John L., 78-12 35th Ave., Jackson Heights 72, N.Y.
FAY, John S., First Boston Corp., 20 Exchange Pl., New York, N.Y. 10005
FAYERWEATHER, John, The International Exec., 64 Ferndale Dr., Hastings-on-Hudson, N.Y. 13076
FEARN, Robert M., 562 Wimbleton Dr., Raleigh, N.C. 27609
FEBRES, Abraham E., Universidad Agraria "La Molina," Lima, Peru
FECHTER, Alan E., 2506 John Marshall Dr., Arlington, Va. 22207
FEDELER, Jerry A., R.R. 1, Sumner, Iowa 50674
FEDER, Ernest, 1545 18th St., N.W., Apt. 516, Washington, D.C.
FEDKIW, John, U.S. Dept. of Agric., Forest Service, Div. of Forest Econ. Res., Washington 25, D.C.
FEFFERMAN, Arthur S., 9006 Linton St., Silver Spring, Md.
FEHD, Gerald A., 830 N. Chambliss St., Alexandria, Va. 22312
FEHMERS, J. M., c/o Messrs. Teixeira de Mattos, Box 392, Amsterdam C., Netherlands
FEI, Edward, USAID—Liberia, c/o American Embassy, Monrovia, Liberia
FEIGE, Edgar L., Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison, Wis. 53706
FEIGENBAUM, Armand V., 23 Edward Ave., Pittsfield, Mass. 01202
FEIN, Rashi, 3312 Rowland Pl. N.W., Washington 8, D.C.
FEINBERG, Daniel, 126 Hausch Blvd., Roosevelt, L.I., N.Y. 11575

FEINBERG, Mordecai, 24219 Ross Rd., Silver Spring, Md. 20910
FEINSTEIN, Lazar, Apartado 4102, Lima, Peru
FEIS, Clayton D., 1001 Carlos Dr., Lincoln, Neb. 68505
FEIT, John F., U.S.R.O. Pol/R, c/o American Embassy, APO New York, N.Y. 09777
FEIWEL, George R., Dept. of Econ., Univ. of Tennessee, Knoxville, Tenn. 37916
FEKETEKUTY, Geza, 436 Mount Lucas Rd., Princeton, N.J. 08541
FEKRAT, Mohammad A., 4600 Connecticut Ave., N.W., Apt. 403, Washington, D.C. 20008
FELDE, Leon S., 29 W. 64th St., New York, N.Y. 10023
FELDMAN, David Elijah, 88-25 153rd St., Jamaica 32, L.I., N.Y.
FELDMAN, Eugene P., Sunnyside, Spencerville, Md. 20868
FELDMAN, Lee D., 2505 Alder, Eugene, Ore. 97405
FELDMAN, Sidney, 11451 Lockwood Dr., Silver Spring, Md.
FELDMAN, Sidney Paul, School of Bus., Univ. of Kansas, Lawrence, Kans.
FELDSTEIN, Martin Stuart, Nuffield Col., Oxford, England
FELDSTEIN, Paul J., Univ. of Michigan, Sch. of Bus., Ann Arbor, Mich.
FELIX, David, Washington Univ., Dept. of Econ., St. Louis 30, Mo.
FELIZ, George C., 1555 Seneca Lane, San Mateo, Calif.
FELLER, David, Port of New York Authority, 111 8th Ave., Rm. 1557, New York, N.Y. 10011
FELLER, Irwin, 205 E. Waring, State College, Pa. 16801
FELLNER, William J., 131 Edgehill Rd., New Haven, Conn.
FELS, Eberhard M., 8032 Grafelfing, Akilindastrasse 12, Munich, Germany
FELS, Rendigs T., Vanderbilt Univ., Box 1664, Nashville, Tenn. 37203
FELSENTHAL, Leonard, United States Mission, 80 Rue de Lausanne, 1200 Geneva 21, Switzerland
FELSTEN, Benjamin, Food Fair Stores, Inc., 3175 John F. Kennedy Blvd., Philadelphia, Pa. 19104
FELTON, John R., 540 N. 73rd St., Lincoln, Neb. 68505
FENLON, Paul E., 5006 Weaver Ter. N.W., Washington, D.C. 20016
FENNELL, G. H., Canadian Industries Ltd., Box 10, Montreal, P.Q., Canada
FENNER, James, 675 West End Ave., New York 25, N.Y.
FENNINGER, Laurence, Jr., Bethlehem Steel Corp., Bethlehem, Pa. 18016
FENSOM, Cheryl A., 340 Gadsden Hall, 30 Plattsburg Ct., Washington, D.C. 20016
FENSTER, Walter, 1709 E. 4th St., Brooklyn, N.Y. 11223
FENSTERMAKER, Joseph Van, 1905 Carlton Dr., Kent, Ohio
FENSTERMAKER, Le Roy F., 4223 Hackett Ave., Lakewood, Calif. 90713
FENTON, Sister Mary G., Regis Col., Weston, Mass. 02193
FERBER, (Mrs.) Marianne A., 606 S. Western Ave., Champaign, Ill.
FERBER, Robert, 606 S. Western Ave., Champaign, Ill.
FEREBEE, Emory, 6900 Ridgewood Ave., Chevy Chase, Md. 21620
FERGENSON, M. C., 110-21 73rd Rd., Forest Hills, N.Y. 12068
FERGER, Wirth F., 4635 N. Rock Spring Rd., Arlington, Va. 22207
FERGUS, Joseph, 208 E. 105th St., New York 29, N.Y.
FERGUSON, Allen R., 5817 Marbury Rd., Bethesda, Md. 21811
FERGUSON, C. E., Dept. of Econ., Duke Univ., Durham, N.C.
FERGUSON, Clinton S., Richmond Professional Inst., Col. of William and Mary, 901 W. Franklin St., Richmond 20, Va.
FERGUSON, Frances D., 374 Chestnut Hill Ave., Brookline, Mass. 01506
FERGUSON, James M., Univ. of Rochester, Col. of Bus. Admin., Rochester, N.Y. 14627
FERGUSON, Robert H., 3 Congress St., Trumansburg, N.Y. 14886
FERNANDES, Santiago, Ronald Carvalho 21, Apt. 1201, Rio-Copacabana, Brazil
FERNANDEZ, Anibal, C. Londres, Edif. New York, #1, Urb. Las Mercedes, Caracas, Venezuela
FERNANDEZ, C. P., 205 Juan Luna, Manila, Philippines
FERNANDEZ, Daniel, 5535 Columbia Pike, Apt. 407, Arlington, Va.
FERNANDEZ, Lionel, 13503 Collingwood Ter., Silver Spring, Md. 20904
FERRALL, Victor E., Jr., 1322 Holly St., N.W., Washington, 12, D.C.
FERRAND-DAVIS, (Mrs.) Marguerite, 1500 S.W. 23rd St., Miami 45, Fla.
FERRARI, Robert Joseph, 114 Parkville Ave., Brooklyn 30, N.Y.
FERREIRA, Otavio S., Rua Dr., Marques Canario, 20 Apta. 604, Rio de Janeiro, Leblon, Brazil
FERRERO, Romulo A., A. Miro Quesada 376-403, Lima, Peru
FERRIN, J. Joseph, 5953 Crescent Ave., Buena Park, Calif.
FERTIG, Lawrence, 535 Fifth Ave., New York, N.Y. 10017
FESHBACH, Murray, 11403 Fairoak Dr., Silver Spring, Md. 20902
FETTER, Frank W., Northwestern Univ., Dept. of Econ., Evanston, Ill. 60201
FETTER, Robert B., Dept. of Ind. Admin., Yale Univ., New Haven, Conn.
FETTER, Theodore A., 2654 N. Robert Walker Pl, Arlington, Va. 22207
FETZER, Joseph B., San Jose State Col., Div. of Bus., San Jose 14, Calif.
FEUERLEIN, William J., Florida Atlantic Univ., Dept. of Econ., Boca Raton, Fla. 33432
FEULNER, Edwin J., Center for Strategic Studies, 810 18th St., N.W., Washington, D.C. 20006
FEY, Richard A., Brown Univ., Dept. of Econ., Providence, R.I.
FICEK, Karel F., 156 Brevator St., Albany, N.Y. 12206
FICHTNER, Charles C., 6785 S.W. 99th Ter., Miami 56, Fla.
FIDELINO DE FIGUEIREDO, Nuno, CEPAL, United Nations, Av. Providencia 871, Santiago, Chile
FIEDLER, Edgar R., 15 Rayson Lane, Dobbs Ferry, N.Y. 10522
FIEKOWSKY, (Mrs.) Hortense F., 5425 39th St., N.W., Chevy Chase 15, D.C.
FIEKOWSKY, Seymour, 5425 39th St., N.W., Chevy Chase 15, D.C.
FIELD, Alfred J., Jr., 2004 McCarthy, Ames, Iowa 50010
FIELD, Bertrand B., 85 Nercomer St., Richland, Wash. 99352
FIELD, Kenneth, 704 Valleyview Rd., Pittsburgh, Pa. 15216
FIELD, Maxwell, 210 Lincoln St., Boston 11, Mass.
FIELD, Robert M., III, 11924 Stoney Creek Rd., Potomac, Md.
FIELDS, Wesley R., 965 E. Alder, Walla Walla, Wash. 99362
FIENUP, Darrell F., Inst. of Agric., Univ. of Minnesota, St. Paul 1, Minn.
FIER, Louis, Brooklyn Col., Rm. 1215-B, Brooklyn, N.Y. 11210
FIESER, Max E., Sacramento State Col., Dept. of Econ., Sacramento, Calif.
FILGAS, James T., 2046 Sheridan Rd., Sch. of Bus., Evanston, Ill. 60201
FINCH, John L., 203-9 Stanford Village, Stanford, Calif. 94305
FINCH, Robert M., 1160 Fifth Ave., Apt. 311, New York, N.Y. 10029
FINDLAY, Alexander C., 3010 26th St., N.E., Washington 18, D.C.
FINDLAY, James L., Citizens and Southern Nat. Bank, P.O. Box 318, Macon, Ga.
FINE, Donald J., 102-30 67th Ave., Apt. 6-M, Forest Hills, N.Y.

FINE, Sherwood M., American Embassy, APO New York 09777
FINEGAN, Marcella E., 402 Grand Ave., Madison, Wis. 53705
FINEGAN, T. Aldrich, Vanderbilt Univ., Dept. of Econ. and Bus. Admin., Nashville 5, Tenn.
FINERTY, Joseph M., 7713 D St., Seat Pleasant, Md. 20687
FINGER, Joseph M., Duke Univ., Dept. of Econ., Durham, N.C. 27706
FINIGAN, William H., National Cash Register Co., Dayton 9, Ohio
FINK, Donald A., 6111 Madawaska Rd., Washington, D.C. 20016
FINK, Louis L., 402 78th St., Seat Pleasant, Md. 20027
FINK, William H., Univ. of Arizona, Dept. of Econ., Tucson, Ariz. 85721
FINKEL, E. Jay, U.S. Treasury Dept., Office of Int. Affairs, Washington, D.C. 20220
FINKELSTEIN, Stanley S., 4214 11th Ave., N.E., Apt. 111, Seattle, Wash. 98105
FINLEY, Bruce W., 1141 William St., State College, Pa. 16801
FINN, Gene L., 12008 Terra Lane, Bowie, Md. 20715
FINN, Sister Isabelle M., Marymount Col., Salina, Kans. 67401
FINN, Stephen J., 21-43 27th St., Astoria, N.Y. 11105
FINN, Thomas J., Wayne State Univ., Dept. of Econ., Detroit 2, Mich.
FINN, William R., 101-61 Lefferts Blvd., Richmond Hill, N.Y. 11419
FINNEGAN, Joseph H., 3350 Bronx Blvd., New York, N.Y. 10467
FINNEY, Katherine, Connecticut Col., Dept. of Econ., New London, Conn. 06320
FINSTER, Ronald D., 5723 E. 18th St., Tucson, Ariz.
FIRCH, Robert S., Dept. of Agric. Econ., Univ. of Arizona, Tucson, Ariz.
FIREOVED, Elgie, 525 Engler St., Magnolia, Ark. 71753
FIRESTONE, Frederic N., Lehigh Univ., Col. of Bus. Admin., Bethlehem, Pa.
FIRESTONE, John M., 5454 Sylvan Ave., Bronx 71, N.Y.
FIRESTONE, O. J., Rockcliffe Park, 375 Mento Pl., Ottawa 2, Ont., Canada
FIRFER, Alexander, American Embassy/USAID, APO New York, N.Y. 09478
FIRFER, (Mrs.) Amy W., American Embassy/USAID, APO New York, N.Y. 09478
FIROOZI, Free, 4330 Pine St., Philadelphia, Pa. 19104
FIRST, (Mrs.) Ramona K., 1235 Vista Grande, Millbrae, Calif.
FISCHBAUM, Marvin N., 100 Farrington St., Apt. 419, Terre Haute, Ind. 47807
FISCHER, Donald E., Arizona State Univ., Col. of Bus., Tempe, Ariz. 85281
FISCHER, E. Fred, Jr., P.O. Box 701, Austin, Tex. 76767
FISCHER, Gerald C., Sch. of Bus., Indiana Univ., Bloomington, Ind.
FISCHER, Joel D., 139 E. 33rd St., New York 16, N.Y.
FISCHER, Monroe C., P.O. Box 2672, Huntridge Sta., Las Vegas, Nev. 89104
FISCHER, Paul, 520 Crown St., Brooklyn 13, N.Y.
FISCHMAN, Leonard L., 1820 Jefferson Pl., N.W., Washington 6, D.C.
FISH, Hy, 1361 E. 52nd St., Chicago, Ill. 60615
FISH, Mary, 4714 Rusk St., Amarillo, Tex.
FISHBAUGH, Charles P., 1527 Tennyson Ave., Dayton, Ohio 45406
FISHER, Allan G. B., Kempsford House, Brize Norton, Oxon, England
FISHER, Allan J., 2309 Apache St., Adelphi, Md. 21710
FISHER, Clyde Olin, 329 Main St., Portland, Conn. 06480
FISHER, Dorothy A., 2000 N. Adams St., Apt. 509, Arlington, Va. 22201
FISHER, Douglas, State Univ. of New York, Grad. Sch. of Bus., Albany, N.Y. 12203
FISHER, Ernest M., 199 Hillcrest Ave., Leonia, N.J. 07737
FISHER, Franklin M., Dept. of Econ., M.I.T., Cambridge, Mass. 01922
FISHER, Gene H., RAND Corp., 1700 Main St., Santa Monica, Calif.
FISHER, Glenn W., 1201 W. Nevada, Urbana, Ill.
FISHER, H. H., 330 S. Spalding Dr., Beverly Hills, Calif. 90212
FISHER, Irving N., 1700 Briar Ridge, Glendale, Calif. 91208
FISHER, Janet A., 717 Knickerbocker St., Madison 11, Wis.
FISHER, Joseph L., 2608 24th St., N., Arlington, Va.
FISHER, Malcom R., Downing Col., Cambridge, England
FISHER, Paul, 7024 Bybrook Lane, Chevy Chase, Md. 21620
FISHER, Ralph H., 6693 MacArthur Blvd., Washington, D.C. 20016
FISHER, Robert D., 4525 Peacock Ave., Alexandria, Va.
FISHER, Walter D., Dept. of Econ. and Soc., Kansas State Univ., Waters Hall, Manhattan, Kans.
FISHER, William Halder, Battelle Memorial Inst., 1755 Massachusetts Ave., N.W., Washington, D.C. 20036
FISHLOW, Albert, Univ. of California, Dept. of Econ., Berkeley 4, Calif.
FISHMAN, Barry, 10471 Sandal Lane, Los Angeles, Calif. 90024
FISHMAN, (Mrs.) Betty G., Dept. of Econ., West Virginia Univ., Morgantown, W. Va.
FISHMAN, George S., RAND Corp, 1700 Main St., Santa Monica, Calif.
FISHMAN, Leo, West Virginia Univ., Morgantown, W. Va.
FISHMAN, Leslie, Univ. of Colorado, Dept. of Econ., Boulder, Colo.
FISK, Donald M., 4921 Auburn Ave., Bethesda, Md.
FITCH, David R., 2719 Crestwood Pl., Denton, Tex.
FITCH, Devin V., 121 Red Hill Rd., Princeton, N.J.
FITCH, Lyle C., 121 Red Hill Rd., Princeton, N.J. 08540
FITCHETT, Delbert A., Rockefeller Found., Casilla 2-P, Santiago 11, Chile
FITILIS, Argiris C., 2155 Linwood Ave., Fort Lee, N.J. 07024
FITKIN, Reed, 70 Pine St., Rm. 4910, New York, N.Y.
FITZGERALD, James, 3300 Bailey Ave., New York 63, N.Y.
FITZGERALD, John M., 153 E. 57th St., New York, N.Y. 10022
FITZGERALD, Lloyd E., Univ. of Detroit, Col. of Com., Detroit 21, Mich.
FITZGERALD, Mark J., Univ. of Notre Dame, Notre Dame, Ind. 46556
FITZGERALD, Thomas J., Jr., 17 Stonybrook Rd., West Caldwell, N.J. 07006
FITZPATRICK, Mary B., 322 Brookline St., Cambridge, Mass. 02139
FITZSIMMONS, Cleo, Purdue Univ., Home Econ. Bldg., West Lafayette, Ind. 47178
FITZWILLIAMS, Jeannette, 13 W. Maple St., Alexandria, Va.
FIUCZYNSKI, Hans W., 79 DeMott Lane, Somerset, N.J. 08873
FIXLER, L. Donald, Duquesne Univ., Dept. of Econ., Pittsburgh, Pa. 15219
FJELDSTED, Boyd L., Col. of Bus. Admin., Univ. of Nevada, Reno, Nev.
FLAGG, Denis A., 5449 Baja Dr., San Diego 15, Calif.
FLAGG, (Mrs.) Virginia G., 5449 Baja Dr., San Diego 15, Calif.
FLAMANT, Maurice, Serv. d'etude de l'Activite Econ., Fondation Nationale de Sci. Polit., 4 rue Michelet, Paris 6e, France
FLANAGAN, John B., 14 Beaumont Dr., Melville, N.Y. 11749
FLANAGAN, Robert J., 1737 Virginia St., Apt. 31, Berkeley, Calif.
FLANAGAN, Thomas M., 111 Kinross Lane, Pittsburgh 37, Pa.
FLANAGAN, William, Fordham Univ., New York 58, N.Y.
FLANDERS, Dwight P., Univ. of Illinois, Dept. of Econ., Urbana, Ill. 61801

FLANDERS, (Mrs.) June, Purdue Univ., Econ. Dept., Lafayette, Ind.
FLANIGAN, Raymond T., 29504 Westbrook Pkwy., Southfield, Mich. 48076
FLANNERY, James J., 4717 16th Rd., North Arlington 7, Va.
FLEISHER, Belton M., Ohio State Univ., Dept. of Econ., Columbus, Ohio 43210
FLEISCHMAN, William E., Univ. of Michigan, Dept. of Econ., 105 Econ., Ann Arbor, Mich.
FLEMING, (Mrs.) Gloria Hile, 3051 N St., N.W. Apt. 2, Washington 7, D.C.
FLEMING, Hugh J., 573 Marquette Dr., San Antonio 28, Tex.
FLEMING, John M., 3051 N St. N.W., Apt. 1, Washington 7, D.C.
FLEMING, Thomas F., Jr., 440 New Jersey Ave., S.E., Washington, D.C. 20003
FLETCHER, Daniel O., Ohio State Univ., Dept. of Econ., 1775 S. College Rd., Columbus, Ohio 43210
FLETCHER, Lehman B., Dept. of Econ. and Soc., Iowa State Univ., Ames, Iowa
FLETCHER, Leonard P., Univ. of Waterloo, Dept. of Econ., Waterloo, Ont., Canada
FLETCHER, Max E., Col. of Bus. Admin., Univ. of Idaho, Moscow, Idaho
FLEXNER, Kurt F., 31 Beechmont Ave., Bronxville, N.Y.
FLINK, Solomon J., Rutgers Univ., Sch. of Bus., 18 Washington Pl., Newark, N.J. 07102
FLOERSHEIM, Rachel, Fed. Res. Bank of New York, Foreign Res. Div., 33 Liberty St., New York, N.Y. 10045
FLORA, Donald F., 4507 University Way, Seattle, Wash. 98105
FLORES, Edmundo, Apartado Postal 5-305, Mexico, D.F., Mexico
FLORIAN, Dietmar A., 14 Clonmore Dr., Scarborough, Ont., Canada
FLORIANI, Juan B., 1638 W. Farwell Ave. 2-A, Chicago, Ill. 60626
FLORINSKY, Michael T., 14 Quai Perdonnet, Vevey, Switzerland
FLOYD, Charles F., Univ. of Georgia, Col. of Bus. Admin., Athens, Ga.
FLOYD, Joe S., Univ. of North Carolina, Carroll Hall, Chapel Hill, N.C.
FLOYD, Roy F., Box 122, Harrogate, Tenn.
FLUBACHER, Joseph F., 7634 Levis Rd., Cheltenham, Pa. 19012
FLUECKIGER, Gerald E., Purdue Univ., Dept. of Econ., Box 102, West Lafayette, Ind.
FLUKER, J. Robert, American Embassy, APO San Francisco, Calif. 96346
FLUMIANI, C. M., P.O. Box 821, Gloucester, Mass.
FLYNN, Edwin H., Univ. of Tennessee, Fin. Dept., Aconda Ct. 8A, Knoxville, Tenn. 37916
FLYNN, Oscar Rogers, Jr., 90 Morningside Dr., New York 27, N.Y.
FOA, Bruno, 1435 Lexington Ave., New York 28, N.Y.
FOERY, Raymond W., 6 E. Mercer Ave., Llanerch, Del. Co., Havertown P.O., Pa. 19083
FOGEL, Robert W., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
FOGEL, Walter, Grad. Sch. of Bus. Admin., Univ. of California, Los Angeles 24, Calif.
FOGLER, Elizabeth M., 11822 Kramper Lane, St. Louis, Mo. 63128
FOLDES, Lucien P., London Sch. of Econ., Houghton St., London, W.C.2, England
FOLEY, Francis L., Vienna—FSO, Dept. of State, Washington 25, D.C.
FOLEY, James W., 2308 S. Logan, Lansing, Mich.
FOLEY, Robert J., 84 Jane St., Stratford, Conn. 06497
FOLI, Reno C., 2907 S. 6th St., Terre Haute, Ind. 47802
FOLK, O. H., 3351 Stephenson Pl., N.W., Washington 15, D.C.
FOLSOM, James M., 815 Malcolm Dr., Silver Spring, Md.
FOLSOM, Roger N., U.S. Naval Postgrad. Sch., Dept. of Bus. Admin. and Econ., Monterey, Calif. 93940
FOLZ, William E., 510 E. C St., Moscow, Idaho
FONG, H. D., #3, 120th St., First Rd., Tien Mou, Taipei Hsien, Taiwan
FONG, (Mrs.) Pauline L., 1421 Arch St., Berkeley, Calif. 94708
FONOLLERA, Raymundo E., Central Mindaneo Univ., Agric. Econ. Dept., Musuan, Bukidnon, Philippines
FONSECA, Ernesto L., Super-Mercado La Criolla, S.A., Managua, Nicaragua
FOOKS, Marvin M., 800 4th St., S.W., Apt. S 107, Washington, D.C.
FOOTE, Merrill J., Illinois Inst. of Tech., Dept. of Bus. and Econ., Chicago, Ill. 60616
FORBES, Stuart C., 2627 Dartmouth Ave. N., St. Petersburg, Fla.
FORBES, William A., 830 Montgomery Ave., Bryn Mawr, Pa. 19010
FORBUSH, Dascomb R., 89 Market St., Potsdam, N.Y. 13676
FORBUSH, (Mrs.) Dorothy, 89 Market St., Potsdam, N.Y. 13676
FORD, Amos W., Louisiana Poly. Inst., Ruston, La.
FORD, Caroll W., Bondville, Vt. 05340
FORD, Douglas J., 140 Masters, Riverside, Calif. 92507
FORD, Edward J., Jr., 19 Morse St., Watertown, Mass. 02158
FORD, Joseph W., 330 1st Ave., Pelham, N.Y.
FORD, (Mrs.) Lucille G., 855 Eastern Ave., Ashland, Ohio 44805
FORD, Milton R., 203 Fairchild, San Antonio, Tex. 78236
FORD, Robert H., 5253-F Broadway, APO Seattle 98737
FORD, Robert S., Univ. of Michigan, 1012 Rackham Bldg., Ann Arbor, Mich.
FORDE, Arnold, 13451 Sunnyvale, Garden Grove, Calif.
FORGET, Claude E., 1624 Burnwood Rd., Baltimore, Md. 21212
FORKOSCH, Morris D., Brooklyn Law Sch., 375 Pearl St., Brooklyn, N.Y. 11201
FORMUZIS, Peter A., Michigan State Univ., East Lansing, Mich.
FORNER, Curtis F., 21 W. Roselle Ave., Roselle Park, N.J. 07068
FORREST, (Mrs.) Ona B., 531 E. 20th St., Apt. 12-E, New York 10, N.Y.
FORSTE, Robert H., Univ. of New Hampshire, Dept. of Econ., Morrill Hall, Durham, N.H. 03824
FORSTER, Arthur D., 118-60 Metropolitan Ave., 1-I, Kew Gardens, N.Y. 11415
FORSTER, Richard L., 120 Anderson Ave., Demarest, N.J. 07627
FORSTER, Walter E., 1317 Sunningdale Dr., Grosse Pointe Woods, Mich. 48236
FORSYTH, Jay D., Colorado Col., Colorado Springs, Colo.
FORSYTH, John D., Queen's Univ., Sch. of Bus., Kingston, Ont., Canada
FORSYTHE, Donald G., Corning Community Col., 5th and Chemung St., Corning, N.Y.
FORTES, America B., Rua Xavier de Castro 72, (Praia de Iracema), Fortaleza, Ceara, Brazil
FORTH, Milburn L., 200 S. York St., Mechanicsburg, Pa. 17055
FORTMAN, Bastiaan De Gaay, Joh. Verhulststraat 77, Amsterdam Z, Netherlands
FORTUNATI, Paolo, Instituto di Statistica, Piazza Scaravilli 2, Bologna, Italy
FOSS, Murray F., 6209 E. Halbert Rd., Bethesda, Md. 21811
FOSSETT, J. Stephen, P.O. Box 6507, Stanford, Calif.
FOST, Carolyn A., 1664½ Normal Dr., Bowling Green, Ky. 42101
FOSTER, Bennett B., Univ. of New Hampshire, Dept. of Forestry, Durham, N.H. 03824
FOSTER, Cyril E., Rutschbahn 17, 2 Hamburg 13, Germany
FOSTER, Edward M., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
FOSTER, Howard G., Cornell Univ., Box 32, N.Y.S.S.I.L.R. Ithaca, N.Y. 14850
FOSTER, J. Rhoads, 1101 17th St., N.W., Washington, D.C. 20036
FOSTER, James I., 789 Boyd Rd., Pleasant Hill, Calif. 94523
FOSTER, Margery S., Hollins College, Va. 24020
FOSTER, Robert D., Wichita State Univ., Col. of Bus. Admin. and Ind., Dept. of Econ., Wichita, Kans. 67208

FOSTER, (Mrs.) Ruth E., 308 G St., N.E., Washington, D.C. 20002
FOSTER, Susan B., Vassar Col., Ferry House, Poughkeepsie, N.Y.
FOTI, Alexander S., Canadian Nat. Railways, Fl. 4, P.O. Box 8100, Montreal, P.Q., Canada
FOTIS, Stephen W., 252 Senisa Dr., San Antonio 1, Tex.
FOTSCHKI, A. Ferd, Ronsdorfer Str. 83, Duesseldorf, Germany
FOUSEK, Peter, Fed. Res. Bank of New York, New York, N.Y. 10045
FOUST, James D., 38 Willow Terrace Apts., Chapel Hill, N.C.
FOX, Bertrand, 18 Edgewood Dr., Lexington, Mass. 01054
FOX, Donald F., Continental Coffee Co., 2550 N. Clybourn Ave., Chicago, Ill. 60614
FOX, Gerald L., 16 Bright St., Northampton, Mass.
FOX, Harland G., 135 E. Hartsdale Ave., Apt. 1A, Hartsdale, N.Y. 10530
FOX, Karl A., Dept. of Econ. and Soc., Iowa State Univ., Ames, Iowa
FOX, Richard, Adrian Col., Adrian, Mich.
FOX, Roger W., Univ. of Arizona, Dept. of Agric. Econ., Tucson, Ariz. 85721
FOX, Samuel, Univ. of Illinois, Navy Pier, Chicago, Ill. 60611
FOY, Robert J., 1006 Braman St., Lansing, Mich. 48910
FRAGA, Alfred, White St., Raynham Center, Mass. 02768
FRAINE, Harold G., 3420 Black Hawk Dr., Madison 5, Wis.
FRANCE, Robert R., Univ. of Rochester, 408 Morey Hall, River Campus Sta., Rochester 20, N.Y.
FRANCFORT, Alfred, Tower C, Rm. 305, 3955 Forbes Ave., Pittsburgh, Pa.
FRANCIS, Amadeo I. D., Box 8134, Fdz. Juncos Stn., Santurce, P.R. 00910
FRANCIS, Sister M. Catherine, Cardinal Cushing Col., 135 Fisher Ave., Brookline, Mass. 01506
FRANCIS, David R., Christian Science Monitor, New York News Bur., 588 Fifth Ave., New York, N.Y. 10036
FRANCIS, Gary E., Box 400, Greeley, Colo.
FRANCIS, Stanley F., 18159 Melrose Ave., Southfield, Mich.
FRANCISCO, Edgar W., III, Yale Univ., Dept. of Epid. and Public Health, 60 College St., New Haven, Conn. 06510
FRANCK, Peter G., Well House, R.F.D. 1, Laurel, Md. 20810
FRANCO-CAMACHO, Guillermo, 72A #17-15 Ap. 202, Bogota, Colombia
FRANCOIS, Bruce E., Kraft Pl., Cupshaw Lake, Ringwood, N.J.
FRANEK, Mikulas, St. Anselms Col., Manchester, N.H.
FRANK, Charles R., Jr., Yale Univ., Dept. of Econ., New Haven, Conn.
FRANK, Helmut J., Univ. of Arizona, B.P.A. Col., Dept. of Econ., Tucson, Ariz.
FRANK, Henry J., P.O. Box 104, Princeton, N.J. 08540
FRANK, Isaiah, 3102 Hawthorne St., N.W., Washington, D.C. 20008
FRANK, Walter S., 6 Chancy St., Cambridge, Mass. 02138
FRANKEL, Allen B., Duke Univ., Econ. Dept., Box 41, Durham, N.C. 27706
FRANKEL, Marvin, Univ. of Illinois, Bur. of Econ. and Bus. Res., 408 David Kinley Hall, Urbana, Ill.
FRANKEL, Pearl, 264 E. Broadway, New York 2, N.Y.
FRANKEL, Richard J., U. S. Army Environmental Hygiene Agency, Edgewood Arsenal, Md. 21010
FRANKEL, S. Herbert, Knoll House, Hinksey Hill, Oxford, England
FRANKENHOFF, Charles A., Faculty of Soc. Sci., Univ. of Puerto Rico, Rio Piedras, P.R. 00745
FRANKENTHALER, James R., 107-10 Shore Front Pkwy., Rockaway Park 94, N.Y.
FRANKLIN, Norton N., International Labour Office, Geneva, Switzerland
FRANKLIN, Philip E., 3734 Northampton St., N.W., Washington, D.C. 20015
FRANKLIN, Raymond S., City Univ. of New York, City Col., Dept. of Econ., New York 31, N.Y.
FRANKLIN, William B., 86-15 Broadway, Apt. 14E, Elmhurst 73, N.Y. 14059
FRANKMAN, Myron J., Ohio Univ., Dept. of Econ., Athens, Ohio 45701
FRANZ, Laurence, 112 Englewood Ave., Buffalo, N.Y. 14214
FRASER, Donald R., 1625 E. Prince Rd., Apt. 108, Tucson, Ariz.
FRASER, Herbert W., Fundacion Rockefeller, Apartado Aereo 6555, Cali, Colombia
FRASER, Robert D. Jr., 6022 9th Rd. N., Arlington, Va. 22205
FRASER, Roderick D., Queen's Univ., Dept. of Econ., Kingston, Ont., Canada
FRAZEE, Gilford, P.O. Box 61, New Martinsville, W.Va. 26155
FRAZER, Arthur W., 103 Martha's Rd., Alexandria, Va.
FRAZER, George E., Rm. 636, 120 S. LaSalle, Chicago, Ill. 60603
FRAZER, William J. Jr., Univ. of Florida, Col. of Bus. Admin., Dept. of Fin. and Ins., Gainesville, Fla. 32603
FRAZIER, Delmar R., Austin Peay State Col., Dept. of Econ., Clarksville, Tenn.
FRECHTLING, Douglas C., 5623 Newington Rd., Washington, D.C. 20016
FRECHTLING, John A., 23915 Lee Baker Dr., Southfield, Mich. 48075
FREDERICK, William C., Univ. of Pittsburgh, Sch. of Bus. Admin., 1925 Cathedral of Learning, Pittsburgh 13, Pa.
FREDERICKSEN, Dick H., 141 Montgomery St., Poughkeepsie, N.Y. 12601
FREDLAND, John, Dept. of Econ. Hist. and Gov., U. S. Naval Academy, Annapolis, Md.
FREDLAND, John E., 675 Ironwood Dr., Ann Arbor, Mich. 48103
FREDLUND, Melvin C., 6191 N.E. Radford Dr., Seattle, Wash. 98115
FREDRIKSON, E. Bruce, Univ. of Pennsylvania, Wharton School, Philadelphia 4, Pa.
FREEBAIRN, Donald K., Cornell Univ., Dept. of Agric. Econ., Ithaca, N.Y.
FREEDMAN, Arthur M., Wharton School, Fin. Dept., Univ. of Pennsylvania, Philadelphia 4, Pa.
FREEDMAN, Robert, Jr., 32 Payne St., Hamilton, N.Y. 13346
FREEMAN, A. Myrick, III, Bowdoin Col., Dept. of Econ., Brunswick, Me. 04011
FREEMAN, George E., Bank of Canada, Ottawa, Ont., Canada
FREEMAN, Jo F., American Embassy AID, APO New York, N.Y. 09674
FREEMAN, Joseph H., 11500 Lockhart Pl., Silver Spring, Md. 20902
FREEMAN, Ralph E., 1020 Boylston St., Chestnut Hill, Mass. 02167
FREEMAN, Richard B., 208 Laurel Dr., Oradell, N.J.
FREEMAN, Roger A., Hoover Inst., Stanford Univ., Stanford, Calif. 96128
FREI, Rudolf, Postfach 148, 8033 Zurich, Switzerland
FREITAS, Lewis P., Columbia Univ., 709 Bus. Bldg., New York 27, N.Y.
FRENCH, Benjamin C., Dept. of Agric. Econ., Univ. of California, Davis, Calif. 95616
FRENCH, Bruce H., 10 Cleveland Lane, Princeton, N.J. 08540
FRENCH, Norman D., Indiana State Univ., Div. of Soc. Sci., Terre Haute, Ind. 47809
FRENCH, Philip R., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
FRENCH, Richard E., 480½ Elm, Norman, Okla.
FRENZEL, Arnold, Carleton Univ., Dept. of Econ., Ottawa, Ont., Canada
FRERICHS, Bernard H., 1459-A S. Waterford Dr., Florissant, Mo. 63033
FRETTY, Ralph E., St. Olaf Col., Dept. of Polit. Sci., Northfield, Minn.
FREUDENBERGER, Herman, Tulane Univ., Dept. of Econ., New Orleans, La. 70118
FREUND, Peter F., Credit Res. Found., 44 E. 23rd St., New York, N.Y. 10010
FREUND, William C., 64 Circle Dr., Millington, N.J. 07946

FREY, Merle E., 172 Claremont Rd., Ridgewood, N.J. 07450
FREY, René L., Im Sesselacker 17, Basel, Switzerland
FREYMAN, Andrew J., 465 McLeod St., Apt. 4, Ottawa 4, Ont., Canada
FREYRE-SERRA, Jorge, Gov. Dev. Bank, Box 4591, San Juan, P.R.
FREYTAG, David C., 3040 Griest Ave., Cincinnati, Ohio 45208
FRICKE, Cedric V., 4901 Evergreen, Dearborn, Mich. 48124
FRIDAY, Charles B., Oregon State Univ., Dept. of Econ., Corvallis, Ore.
FRIED, Edward R., 9732 Bexhill Dr., Kensington, Md. 20795
FRIED, Joel S., 809 Simpson St., Evanston, Ill. 60201
FRIED, Milton, Amalgamated Clothing Workers of America, 15 Union Sq., New York 3, N.Y.
FRIEDBERG, Stephan J., 5715 S. Drexel Ave., Apt. 302, Chicago, Ill. 60637
FRIEDLAND, Claire, 5723 S. Dorchester, Chicago, Ill. 60637
FRIEDLAND, Seymour, 14 Ellison Ave., Bronxville, N.Y.
FRIEDLANDER, (Mrs.) Ann F., 1000 Memorial Dr., Cambridge, Mass. 02138
FRIEDLE, (Mrs.) Mabel M., 12 Estrella Rd., De Bary, Fla. 32713
FRIEDLY, John D., 2340 Winkleman, Pontiac, Mich.
FRIEDMAN, Barry L., North Carolina State Univ., Dept. of Econ., Raleigh, N.C. 27607
FRIEDMAN, Bernard H., 2847 Diamond St., Allentown, Pa.
FRIEDMAN, Clara H., 1722 Michael William Rd., Merrick, L.I., N.Y. 11566
FRIEDMAN, David H., 555 E. 21st St., Brooklyn, N.Y.
FRIEDMAN, Herman, 4 Zabriskie Ter., Monsey, N.Y. 10952
FRIEDMAN, James W., 227 Corbin Rd., Hamden, Conn.. 06439
FRIEDMAN, Jerome B., 8502 16th St., Apt. T-5, Silver Spring, Md. 20910
FRIEDMAN, Marshall, 203 Myers St., Apt. A, Lexington, Va. 24450
FRIEDMAN, Milton, Dept. of Economics, Univ. of Chicago, 1126 E. 59th St., Chicago, Ill. 60637
FRIEDMAN, Milton, 1722 Michael William Rd., Merrick, L.I., N.Y.
FRIEDMAN, Richard, San Fernando Valley State Col., Dept. of Econ. and Statis., Northridge, Calif.
FRIEDMAN, Roberto, 4531 Forbes Ave., Apt. 311, Pittsburgh, Pa.
FRIEDMANN, John R. P., c/o Ford Found., Santo Domingo 504, Of. 81, Santiago, Chile
FRIEDRICH, Walter K., Lieselottenstrasse 20, Homburg-Saar, Germany
FRIEND, Irwin, Wharton Sch., Univ. of Pennsylvania, Philadelphia 4, Pa.
FRIESEN, Ronald L., 431 Riverside Dr., Woodbridge Hall, Apt. 2-L, New York, N.Y. 10025
FRIEST, Philip L., Univ. of Minnesota, Dept. of Econ., Duluth, Minn. 55812
FRIKART, John M., 1919 E. Lee St., Tucson, Ariz.
FRISCH, Franz A. P., 11 Iva Lane, Wakefield Forest, Fairfax, Va. 22030
FRISCH, Ragnar, Univ. of Oslo, Inst. of Econ., Karl Johansgt 47, Oslo, Norway
FRISCH, Robert D., Elks Club, 509 S. 6th St., Springfield, Ill.
FRISCIA, August B., Rock Hill Rd., Bedford, N.Y. 10506
FRISINGER, Hubert H., 2428 Pemberton Dr., Toledo 6, Ohio
FRITSCH, Bruno, Langackerstr. 1222b, 8764 Herrliberg, Switzerland
FRITZ, David, 33-24 Junction Blvd., Apt. 2S, Jackson Heights 72, L.I., N.Y.
FRITZ, James S., 7438 Olive St., Kansas City, Mo. 64132
FRITZ, Wilbert G., 305 N. Kenmore St., Arlington, Va.
FRIZOL, Sylvester, Loyola Univ., 820 N. Michigan Ave., Chicago, Ill. 60611
FRNKA, Robert L., Wichita State Univ., Dept. of Econ., Wichita, Kans. 67208
FROEHLICH, Walter, Marquette Univ., Milwaukee, Wis.
FROHMAN, Charles E., 1313 Cedar Point Rd., Sandusky, Ohio
FROMAN, Lewis A., Russell Sage Col., Troy, N.Y.
FROMM, Gary, Brookings Institution, 1775 Massachusetts Ave., N.W., Washington 6, D.C.
FROOMKIN, Joseph, 114 E. 84th St., New York, N.Y. 10028
FROST, Alexander, III, Box 1532, Boulder, Colo. 80302
FRUCHT, Padraic P., 2119 Paul Spring Rd., Alexandria, Va.
FRUEH, Lloyd, II, Wright State Univ., Col. Glenn Highway, Dayton, Ohio 45431
FRUIN, Jerry E., General Mills, Inc., Operations Res. Dept., 9200 Wayzata Blvd., Minneapolis, Minn. 55426
FRUMKIN, Norman, 3737 Legation St., N.W., Washington, D.C. 20015
FRY, Edward R., 5129 Linette Lane, Annandale, Va. 22003
FRY, John N., 2643 Tantalus Dr., Honolulu, Hawaii 96813
FRY, Kenneth E., City of Milwaukee, Div. of Econ. Dev., Milwaukee, Wis.
FRYDE, (Mrs.) Sophie C., 498 West End Ave., Apt. 6-B, New York 24, N.Y.
FRYE, John G., 1155 15th St., N.W., Rm. 713, Washington, D.C. 20005
FRYER, John L., Canadian Labour Congress, Dept. of Res., 100 Argyle Ave., Ottawa 4, Ont., Canada
FUCHS, Frank A. Jr., 6124 Hillside, Apt. 9, Indianapolis, Ind. 46220
FUCHS, Ralph F., Sch. of Law, Indiana Univ., Bloomington, Ind.
FUCHS, Victor R., 9 Talley Rd., Roslyn, N.Y. 11576
FUERST, Alfred M., 68-23 Exeter St., Forest Hills, N.Y. 11375
FUJIOKA, Masao, 1-16, Honmachi, Shibuya-ku, Tokyo, Japan
FULKERSON, Frank B., P.O. Box 1481, Bartlesville, Okla.
FULLER, Denton A., Liberty Trust Co., Cumberland, Md.
FULLER, Earl W., 381 Rugby Ave., Rochester 19, N.Y.
FULLER, George A., Univ. of Utah, Dept. of Econ., Salt Lake City, Utah 84112
FULLER, George T., 108 Drake Rd., Somerset, N.J. 08873
FULLMER, Robert G., Tennessee Valley Authority, 213A New Sprankle Bldg., Knoxville, Tenn. 37902
FULMER, John L., Univ. of Kentucky, Bur. of Bus. Res., Col. of Com., Lexington, Ky.
FULMER, Vincent A., Massachusetts Inst. of Tech., Cambridge, Mass. 01922
FULTON, G. E., Sir George Williams Univ., Drummond St., Montreal 25, P.Q., Canada
FULTON, Joseph F., 6544 Placid St.. Falls Church, Va. 22043
FULTON, Roger H., 75 Apple Lane, Briarcliff Manor, N.Y. 10510
FUNCK, Rolf, Kaiserslauterner Strasse 11a, 75, Karlsruhe, West Germany
FUNDABURK, Emma L., Luverne, Ala. 36049
FUNK, David G., 29 Lawrence St., Cambridge, Mass. 01922
FUNK, Herbert J., St. Benedict's Col., Dept. of Econ., Atchison, Kans. 66002
FUREY, Joseph C., Univ. of Maryland, B.P.A. Col., Dept. of Econ., College Park, Md.
FURINO, Antonio, Univ. of Houston, Center of Res. in Bus. and Econ., Cullen Blvd., Houston 4, Tex.
FURNISS, Edgar S., Grad. Sch., Yale Sta., New Haven, Conn.
FURPHY, Thomas, 1840 Torresdale Ave., Philadelphia, Pa. 19124
FURST, Harold, 790 Keeler Ave., Berkeley 8, Calif.
FURTH, J. Herbert, 7520 Maple Ave., Takoma Park, Md. 21784
FURUBOTN, Erik G., State Univ. of New York, Dept. of Econ., Harpur Col., Binghamton, N.Y.

FURUHASHI, Yusaku, Univ. of Notre Dame, Dept. of Mktg., Notre Dame, Ind. 46556
FUSFELD, Daniel R., Dept. of Econ., Univ. of Michigan, Ann Arbor, Mich.
FUSS, M. A., International House, Univ. of California, Berkeley, Calif. 94720
FUSSA, Felix I., 540 Alden Dr., Rahway, N.J. 07065
FUTAKY, John, 44-10 Ketcham St., Elmhurst 73, L.I., N.Y. 14059
FUTHEY, George R., Susquehanna Univ., Selinsgrove, Pa. 17870
FUZ, George C., 1525 N. Sanborn Ave., Hollywood, Calif. 90027

GABLER, Louis R., 23 E. 40th St., Bayonne, N.J. 07002
GABRIEL, K. Georg, 900 Memorial Dr., 1903 E., Cambridge, Mass. 01922
GABRIEL, Peter P., 111 Church St., Weston, Mass. 02193
GADEHOLT, Bjorn, 17462 Blue Heron Rd., Lake Oswego, Ore.
GADSBY, Dwight M., 7925 Pennsylvania Ave., S.E., Washington, D.C. 20028
GADZIKOWSKI, Gilbert R., 17145 Fairfield, Detroit 21, Mich.
GAFFEY, John D., 1647 Perkins Dr., Arcadia, Calif.
GAFFNEY, M. Mason, Univ. of Wisconsin, Dept. of Econ., Milwaukee, Wis. 53211
GAGE, Ronald H., 2422 Saunders Station Rd., Pitcairn, Pa. 15140
GAGLIARDI, Carl N., 377 Ellsworth Ave., New Haven, Conn. 06511
GAINES, Tilford C., 38 S. Dearborn St., Chicago, Ill.
GAINSBRUGH, Martin R., Nat. Ind. Conf. Bd., 845 Third Ave., New York 22, N.Y.
GAJEWSKI, Peter, USOM c/o Postmaster APO, San Francisco, Calif. 96346
GAL, Nicholas P., 425 Riverside Dr., New York, N.Y. 10025
GALAMBOS, Louis, 3509 Alameda Circle, Baltimore, Md.
GALATIN, Malcolm, Univ. of Western Ontario, Univ. Col. of Arts and Sci., Room 252, London, Ont., Canada
GALBRAITH, John A., 600 Grosvenor Ave., Westmount, Montreal 6, P.Q., Canada
GALBRAITH, John K., Harvard Univ., Cambridge, Mass. 01922
GALBRAITH, Virginia L., Dept. of Econ. Soc., Mount Holyoke Col., South Hadley, Mass. 01075
GALBREATH, C. Edward, 8326 Still Spring Ct., Carderock Springs, Bethesda, Md. 20034
GALBREATH, George T., 418 St. Augustine Ave., Claremont, Calif.
GALCHUS, Kenneth, Washington Univ., Dept. of Econ., St. Louis 30, Mo.
GALE, Bradley T., 102 Montgomery St., Highland Park, N.J.
GALE, Hazen F., 2460 N. Stevens St., Alexander, Va. 22311
GALE, Hoyt R., 669 Sturtevant Dr., Sierra Madre, Calif. 91024
GALENSON, Walter, 1230 Contra Costa Dr., El Cerrito, Calif.
GALL, Galvin G., S. Eagleville Rd., Storrs, Conn. 06268
GALLAGHER, Edward P., 842 Garrett Lane, Springfield, Dela Co., Pa. 15144
GALLAGHER, James F., 313 E. 183rd St., Bronx, N.Y. 10450
GALLAGHER, John O., Upsala Col., Dept. of Economics, East Orange, N.J.
GALLARDO, Joselito S., 56 Sto. Tomas St., Quezon City, Philippines
GALLARDO, Lloyd L., 771 Solana Dr., Lafayette, Calif. 94549
GALLASTEGUI, Francisco, Poussin 76, Mexico 19, D.F., Mexico
GALLAWAY, Kenton L., 1626 Dwight Way, Apt. D, Berkeley, Calif.
GALLAWAY, Lowell E., 202 Cricket Lane, Wallingford, Pa.
GALLEHER, Richard M., 2921 Barrister Lane, Bowie, Md. 20715
GALLI, Timothy A., Box 214, Sta. A, Champaign, Ill. 61824
GALLIK, Dimitri M., 618 Eldrid Dr., Silver Spring, Md.
GALLIN, David R., 4231 Osage Ave., Philadelphia 4, Pa.
GALLMAN, Robert E., Dept. of Econ., Univ. of North Carolina, 210 Carroll Hall, Chapel Hill, N.C.
GALLOWAY, Leon J., 15 Twinlawns Ave., Hicksville, N.Y.
GALLUP, William A., Jr., 751 Boston Post Rd., Weston, Mass. 02193
GALPER, Harvey, Dartmouth Col., Econ. Dept., Hanover, N.H.
GAMBARDELLA, Joseph R., 1404 Fourth St., Jackson, Mich. 49203
GAMBINO, Amedeo, Via Emilio Cavalier 12, Rome, Italy
GAMBLE, Hays B., Box 21, Boalsburg, Pa. 16827
GAMBLE, Philip L., Univ. of Massachusetts, Dept. of Econ., Draper Hall, Amherst, Mass. 01003
GAMBLES, Glenn C., 9156 Springhill Ct., Greenbelt, Md. 20770
GAMBOA-F., Oswaldo, Oficina Cent. de Coord. y Planification, Miraflores, Caracas, Venezuela
GAMBS, John S., 13 Griffen Rd., Clinton, N.Y. 13323
GAMBY, Raymond R., Haile Sellassie I Univ., P.O. Box 1176, Addis Ababa, Ethiopia
GANDER, James P., Univ. of Utah, Econ. Dept., Salt Lake City, Utah
GANJEI, Nasser D., 20 Garlen Rd., Katonah, N.Y. 10536
GANNON, Colin A., P.O. Box 3583, Stanford, Calif. 94305
GANSLE, James J., 4123 Warner St., Kensington, Md. 20795
GANZ, Alexander, 16 Ash St., Cambridge, Mass. 02138
GANZ, Louis J., 2 Washington Square Village, New York, N.Y. 10012
GAPPERT, Gary, 119 College Pl., Syracuse, N.Y. 13210
GARB, Gerald, 864 Summerville Dr., Lexington, Ky. 40504
GARBARINO, Joseph W., Sch. of Bus. Admin., Univ. of Calif., Berkeley 7, Calif.
GARBUNY, Siegfried, 510 21st St., N.W., Washington 6, D.C.
GARCIA, Alexander, 62 Pierrepont St., Brooklyn, N.Y. 11201
GARCIA, Carlos E., Casilla 217, La Paz, Bolivia
GARCIA, F. L., 170 E. 94th St., New York, N.Y. 10028
GARCIA-ECHEVARRIA, Santiago, Espartero, 14, Bilbao, Spain
GARD, William L. S., 5805 Mission Dr., Mission Hills, Kans.
GARDINER, Emanuel O., Gov. of Liberia, Bur. of Econ. Res., Monrovia, Liberia
GARDNER, B. Delworth, Utah State Univ., Agric. Econ. Dept., Logan, Utah 84321
GARDNER, Bruce L., 5110 S. Kenwood Ave., Chicago, Ill.
GARDNER, Clinton L., 222 Broadway, Room 726, New York 38, N.Y.
GARDNER, Kelsey B., 7527 17th St., N.W., Washington 12, D.C.
GARDNER, Richard N., 3417 R St., N.W., Washington 7, D.C.
GARDNER, Stephen L., P.O. Box 500, Boulder, Colo.
GARDNER, Walter R., 4115 Leland St., Chevy Chase, Md. 20015
GARDNER, Wayland D., Western Michigan Univ., Econ. Dept., Kalamazoo, Mich.
GARDNER, William R., E 718 35th Ave., Spokane, Wash. 99203
GARDONE, Michael J., Jr., Northern Illinois Univ., Dept. of Econ., DeKalb, Ill. 60115
GARFF, Mark R., 3829½ Evanston Ave., N., Seattle 3, Wash.
GARFIELD, Frank R., 5810 Warwick Pl., Somerset, Chevy Chase, Md. 21620
GARFIELD, Paul J., Foster Assocs., 1523 L St., N.W., Washington, D.C.
GARFINKLE, Stuart, 10220 Parkwood Dr., Kensington, Md. 20795
GARIS, Roy L., 5464 Middlecrest Rd., Rolling Hills, Calif. 94572

GARLICK, Peter C., Howard Univ., Dept. of Econ., Washington, D.C. 20001
GARLIN, Victor, 2 Hill Point, San Francisco, Calif. 94122
GARLINGHOUSE, F. Mark, 25 Hemlock Rd., Short Hills, N.J. 07078
GARMA, Enrique B., Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
GARMAT, John, 9312 Piney Branch Rd., Silver Spring, Md.
GARNEL, Donald, 3430 Ensalmo Ave., San Jose, Calif. 95118
GARNSEY, Morris E., Dept. of Econ., Univ. of Colorado, Boulder, Colo.
GAROIAN, Leon, Oregon State Univ., Extension Hall 215, Corvallis, Ore.
GARRETSON, William W., 62 Louise St., Delmar, N.Y. 12054
GARRETT, Thomas M., Cambridge Center for Soc. Studies, 42 Kirkland St., Cambridge, Mass.
GARRIDO-TORRES, José, Rua Humberto de Campos, 1003, Leblon, Rio de Janeiro, Brazil
GARTNER, Franz G., 6118 Laurel Hill Blvd., Woodside, N.Y. 11377
GARTNER, Joseph, Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee 11, Wis.
GARVER, Earl S., Dept. of Econ., Manchester Col., North Manchester, Ind. 46962
GARVIN, Benjamin R. Jr., 139 Coventry Rd., Decatur, Ga. 30030
GARVIN, Wilfred J., 3409 Radnor Pl., Falls Church, Va.
GARVY, George, 33 Liberty St., New York, N.Y. 10045
GARZOUZI, Eva, 92 State St., Brockport, N.Y. 14420
GASPER, Louis C., Duke Univ., Dept. of Econ., Durham, N.C. 27706
GASTWIRT, Lawrence E., 803 S. Springfield Ave., Springfield, N.J. 07081
GATES, David F., Princeton Univ., Woodrow Wilson Sch., Princeton, N.J.
GATES, Jack K., Western Illinois Univ., Dept. of Soc. Sci., Macomb, Ill. 41455
GATES, James E., Col. of Bus. Admin., Univ. of Georgia, Athens, Ga.
GATES, Robert J., 11322 E. 19th, Independence, Mo. 64052
GATES, Theodore R., 5810 Surrey St., Chevy Chase, Md. 21620
GATES, William B., Jr., Sabin Dr., Williamstown, Mass. 01267
GATTI, James F., 3 Haviland Rd., Bloomfield, Conn.
GATY, Lewis R., II, Swarthmore Col., Swarthmore, Pa. 19081
GAUMNITZ, Richard K., 308 MacAlester St., St. Paul 5, Minn.
GAVELA, Manuel, 548 W. 113th St., New York, N.Y. 10025
GAVETT, Thomas W., Inst. of Ind. Rela., West Virginia Univ., Morgantown, W.Va.
GAVIN, Mortimer H., 761 Harrison Ave., Boston 18, Mass.
GAWTHROP, Paul R., Marietta Col., Dept. of Econ., Marietta, Ohio 45750
GAY, Merrill C., 3164 Upland Ter., N.W., Washington 15, D.C.
GAY, Robert M., Fort Hays Kansas State Col., Box 9, Hays, Kans.
GAYER, Paul D., 307-4 Stanford Village, Stanford, Calif. 94305
GAYOSO-QUINTANA, Antonio, 239-T Flavet 3, Gainesville, Fla. 32601
GBARDOE, Daniel Y., Bur. of Labor, Dept. of Com. and Ind., Monrovia, Liberia
GEBELEIN, Herbert, 11 Elm St., Northfield, Vt. 05663
GEBHART, George R., Ferris State Col., Econ. Dept., Big Rapids, Mich. 49307
GEBLER, Stanley I., 1652 E. 12th St., Brooklyn, N.Y. 11229
GEER, William D., Howard Col., Birmingham, Ala.
GEGAN, Vincent F., 4621 Windom Pl., N.W., Washington 16, D.C.
GEHRELS, Franz, Dept. of Econ., Indiana Univ., Bloomington, Ind. 47401
GEIER, Marsha L., 722 Clark St., Evanston, Ill. 60201
GEIGER, Erwin P., 16 Pennsylvania Ave., Montvale, N.J. 07645
GEIGER, Theodore, Nat. Planning Asso., 1606 New Hampshire, N.W., Washington 9, D.C.
GEIGER, (Mrs.) Theodore, Nat. Planning Asso., 1606 New Hampshire, N.W., Washington 9, D.C.
GEISERT, Wayne F., Bridgewater Col., Bridgewater, Va. 22812
GEISINGER, William R., 108½ S. Monroe St., Box 279, Troy, Ohio 45373
GEITHMAN, David T., Universidad del Valle, Facultad de Ciencias Econ., Apartado Aereo 2188, Cali, Colombia
GEKKER, Paul, 5847 Lowell Ave., Alexandria, Va.
GELB, Ira, 12 Dongan Pl., New York, N.Y. 10040
GELFAND, Jack E., State Univ. of New York, Grad. Sch. of Public Affairs, Albany, N.Y. 12203
GELFAND, Marvin, 868 N. Pleasant St., Amherst, Mass.
GELLER, Albert H., 430 Wildwood Dr., Park Forest, Ill. 60466
GELLMAN, Aaron J., 530 Woodlawn Ave., Glencoe, Ill. 60022
GELMAN, Herman A., c/o Herman A. Gelman Co., 206 Graham Ave., Brooklyn, N.Y. 11206
GELSON, Richard J., Col. of Notre Dame of Maryland, Dept. of Econ., Baltimore, Md. 21210
GEMMILL, Paul F., 406 Thayer Rd., Swarthmore, Pa. 19081
GEMMILL, Robert F., 3610 Albemarle St., N.W., Washington, D.C. 20008
GEMMILL, Robert M., 801 McAllister St., Hanover, Pa. 17331
GENOVESE, Francis C., Babson Inst. of Bus. Admin., Babson Park 57, Mass.
GENSEMER, Bruce, 1465 University Ter., Ann Arbor, Mich.
GEORGE, Clarke M., U. S. Embassy, Freetown, Sierra Leone, British West Africa
GEORGE, Edward Y., 25 N. Hancock St., Lexington, Mass. 02173
GEORGE, Henry C., Standard Oil Co., Mktg. Res. Dept., 18th Fl., Midland Bldg., Cleveland 15, Ohio
GEORGE, Peter J., McMaster Univ., Dept. of Econ., Hamilton, Ont., Canada
GEORGE, William C., USAID-PAD-Statistics, APO New York 09205
GEORGE, William H., Niagara Univ., Dept. of Econ., Niagara University, N.Y. 14109
GEORGE, William M., 10100 Gary Rd., Rockville, Md. 20854
GEORGES, Erika, 914½ Euclid, Santa Monica, Calif.
GEORGESCU-ROEGEN, Nicholas, Vanderbilt Univ., Box 1594, Nashville 5, Tenn.
GEORGIADIS, Hourmouzis G., Dept. of Econ., Princeton Univ., Princeton, N.J. 08540
GEORGIOU, Polyvio C., 34-43 60th St., Woodside, N.Y.
GERAGHTY, Michael F. X., 455 Senator St., Brooklyn, N.Y. 11220
GERAKIS, Andreas S., 925 25th St., N.W., Apt. 606, Washington, D.C. 20037
GERAN, Michael J., 43-70 Kissena Blvd., Apt. 22-F, Flushing, N.Y. 11355
GERARD, Irwin, Fed. Mediation Serv., 346 Broadway—1101, New York, N.Y. 10013
GERARD, Roy, 124 Victoria Pl., Syracuse 10, N.Y.
GERBER, Abraham, Amer. Electric Power Serv. Corp., 2 Broadway, New York 8, N.Y.
GERBER, Michael H., 102-45 62nd Rd., Apt. 3-C, Forest Hills, N.Y. 11375
GERDES, Mary S., 1337 Fargo Ave., Chicago, Ill. 60626
GERDES, Victor, 610 E. 5th St., Hallettsville, Tex. 77964
GERFIN, Harald, Dornliweg 19, 4125 Riehen, Switzerland
GERHARD, Hans W., Int. Monetary Fund, 19th and H St., Washington, D.C.
GERLANDO, Peter, Fairfax Village Apts., 2039 38th St., S.E., Apt. 201, Washington, D.C. 20020
GERMAN, Jeremiah J., 2422 Sylvale Rd., Baltimore, Md. 21209
GERMANE, Gayton E., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 94305

GERMANE, Janet R., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 94305
GERMANIS, John J., 5335 K St., Sacramento, Calif. 95819
GERNAND, Arthur C., 2500 Wisconsin Ave., Washington, D.C. 20007
GERONIMAKIS, Stylianos, Ministry of Co-ord., 3, Amerikis Str., Athens, Greece
GERRA, Martin J., Jr., IBM, Fed. Systems Div., 326 E. Montgomery Ave., Rockville, Md.
GERRISH, Catherine R., 207 Grove St., Cambridge, Mass. 01922
GERSCHENKRON, Alexander, Harvard Univ., Dept. of Econ., Cambridge, Mass. 01922
GERSHENBERG, Irving, Univ. of Connecticut, Dept. of Econ., Storrs, Conn. 06268
GERSON, Joseph E., 302 Beach Ave., Staten Island, N.Y. 10306
GERSTEL, Samuel, 2445 Ekers Ave., Montreal, P.Q., Canada
GERSTING, John M., Dept. of Econ., John Carroll Univ., Cleveland, Ohio
GERSTL, Carol L., 165 Midland Pl., Newark, N.J. 07106
GERY, Frank W., Old Dutch Rd., R. 1, Northfield, Minn.
GEWEHR, Hamilton D., 5021 Rodman Rd., Washington 16, D.C.
GEYER, Herbert G., City Univ. of New York, Dept. of Econ., Hunter Col., 695 Park Ave., New York, N.Y. 10021
GHALI, Jawad M., Ministry of Labor and Social Aff., Baghdad, Iraq
GHANDOUR, Marwan, Sherman Hall, 556, 909 S. 5th St., Champaign, Ill.
GHARRITY, Norman J., Ohio Wesleyan Univ., Dept. of Econ., Delaware, Ohio 43015
GHAZALAH, Ismail A., 1514 Oxford St., Berkeley Calif. 94709
GHEARING, Edward, 6409 Maplewood Dr., Falls Church, Va. 22041
GHERITY, James A., Jr., Northern Illinois Univ., Dept. of Econ., De Kalb, Ill. 60115
GHIARDI, John L., Office of Treas. Attache, American Embassy, APO New York 09794
GHOSH, Arabinda, Dept. of Agric. Econ., Univ. of Wisconsin, Madison, Wis. 53706
GHURANI, Izzat I., Arlington State Col., Dept. of Econ., Arlington, Tex.
GIACALONE, Joseph A., 82-50 135th St., Apt. 6G, Kew Gardens, N.Y. 14478
GIACCHI, Gaetano, 809 Classon Ave., Brooklyn 38, N.Y.
GIANARIS, Nicholas V., 23-45 36th St., Astoria, N.Y.
GIANG, Vu, Esso, Duong Thong Nhat, Saigon, Vietnam
GIANTURCO, Delio E., 1636 N. 21st St., Arlington 9, Va.
GIARDA, Piero D., 11 Sumner Rd., Cambridge, Mass. 02138
GIARDINA, I. Paul, Union Junior Col., Cranford, N.J. 07016
GIBB, Graham C., Canadian Int. Paper Co., Room 1440, Sun Life Bldg., Montreal, P.Q., Canada
GIBBENS, John M., Wisconsin State Univ., Whitewater, Wis. 53190
GIBBONS, James R., Univ. of Detroit, Col. of Com. and Fin., Detroit 21, Mich.
GIBBONS, William J., Fordham Univ., Bronx, N.Y. 10458
GIBBS, George, Claremont Men's Col., Pitzer Hall, Claremont, Calif.
GIBSON, Charles R., St. Joseph's Col., 59th and City Line, Philadelphia, Pa. 19131
GIBSON, Cyrus F., c/o Ford Found., Reforma 243, 7° Piso, Mexico 5, D.F., Mexico
GIBSON, Forrest W., 3365 Mentone Ave., Los Angeles 34, Calif.
GIBSON, James L., Centre Col., Danville, Ky. 40423
GIBSON, Roland, 106 Queen St., Chestertown, Md. 21620
GIBSON, Ted A., 617 Lemon Hill Ter., Fullerton, Calif. 92632
GIBSON, Weldon B., Standord Res. Inst., Menlo Park, Calif.
GIBSON, William E., 5550 S. Dorchester Ave., Chicago, Ill. 60637
GIDEONSE, Harry D., 2264 Brookside Ave., Yorktown Heights, N.Y. 10598
GIDEONSE, Martin C., 1216 Union St., Schenectady, N.Y.
GIDEONSE, Max, Dept. of Econ., Rutgers Univ., New Brunswick, N.J.
GIDEZ, Robert M., 6133 Leesburg Pike, Falls Church, Va. 22041
GIDWANI, Sushila J., 11 E. 100 Riverdale Ave., Yonkers, N.Y.
GIERSCH, Herbert, Universität des Saarlandes, 66 Saarbrücken 15, Germany
GIES, Thomas G., Sch. of Bus., Univ. of Michigan, Ann Arbor, Mich.
GIESBRECHT, Martin G., 20 Faculty Pl., Wilmington, Ohio 45177
GIESE, G. O., An Den Eichen 3, 2000 Glashuette /Holstein, Germany
GIFFEY, Richard C., 4900 Tennyson Dr., McLean, Va. 22101
GIFFIN, James F., Eastern Illinois Univ., Sch. of Bus., Charleston, Ill. 61920
GIFFORD, Adam, San Diego State Col., San Diego 15, Calif.
GIFFORD, Gilbert, Sch. of Bus. and Pub. Admin., Univ. of Arizona, Dept. of Econ., Tucson, Ariz.
GIFFORD, Guy C., Pfizer Export Corp., 1524/36 Union House, P.O. Box 323, G.P.O., Hong Kong
GIFT, Richard E., Davidson Col., Davidson, N.C. 28036
GILBERT, Gary, 3625 W. Marquette Rd., Chicago, Ill. 60629
GILBERT, Gerald Y., 918 Jenifer Rd., Horsham, Pa. 19044
GILBERT, Horace N., California Inst. of Tech., Pasadena, Calif. 91109
GILBERT, Jerome, Port of N.Y. Authority, 111 Eighth Ave. at 15th St., New York 11, N.Y.
GILBERT, Lawrence I., 2373 Jeanne St., Ann Arbor, Mich. 48104
GILBERT, Milton, Bank for Int. Settlements, Basle, Switzerland
GILBERT, Richard V., c/o Harvard Pakistan Advis. Grp., P.O. Box 7452, Strachen Rd., Karachi, Pakistan
GILBOY, (Mrs.) Elizabeth W., Harvard Univ., 1583 Massachusetts Ave., Cambridge, Mass. 01922
GILCHRIST, Forest E., Box 122, Shelton, Wash. 98584
GILES, Antonio, IICA-CIRA, Apartado Aereo 14592, Bogota, Colombia
GILES, James B., Rice Univ., Room 109, Lovett Hall, Houston 1, Tex.
GILL, Thomas F., Jr., 202 N. Main St., Wilkes-Barre, Pa.
GILLAM, Cornelius W., Univ. of Pennsylvania, Wharton Sch., Dept. of Bus. Law, Philadelphia, Pa. 19104
GILLESPIE, Robert W., 1014 W. William, Champaign, Ill.
GILLESPIE, W. Irwin, Carleton Univ., Dept. of Econ., Ottawa 1, Ont., Canada
GILLETTE, Charles C., 828 Seerley Blvd., Cedar Falls, Iowa 50613
GILLETTE, Richard C., 6531 Washburn Ct., McLean, Va. 22101
GILLIES, James M., Univ. of California, Sch. of Bus. Admin., Los Angeles, Calif. 90024
GILLILAND, Charles E., Jr., Temple Univ., Sch. of Bus. Admin., Philadelphia, Pa. 19122
GILLIM, Marion, Barnard Col., Dept. of Econ., Columbia Univ., New York 27, N.Y.
GILLIN, Edward F., Rural Bank of N.S.W., Box 41, G.P.O., Sydney, N.S.W., Australia
GILLINGHAM, Robert F., 2 Worthington Ave., Springlake, N.J.
GILLIS, Stephen M., 1626 Ridgeland Dr., Champaign, Ill.
GILLMAN, Joseph M., 16 Stevenson Ave., Hartsdale, N.Y. 10530
GILLMORE, Curry W., 688 Long Hill Rd., Briarcliff Manor, N.Y. 10510
GILLOGLY, David K., 13119 Beaver Ter., Rockville, Md. 20853
GILMAN, Harry J., 1201 S. Scott St., Arlington, Va.
GILMAN, Spencer S., 191 Verde Mesa, Danville, Calif. 94526
GILMARTIN, William M., 6613 Virginia View Ct., Washington, D.C. 20016

GILMORE, Eugene A., Baylor Univ., Hankamer Sch. of Bus., Waco, Tex.
GILMORE, John S., 2474 S. Jackson St., Denver, Colo. 80210
GILOANE, William, 3045 Legation St., N.W., Washington, D.C. 20015
GILPATRICK, Eleanor, 408 David Kinley Hall, Urbana, Ill.
GINESTAR, Angel, Julian Barraquero 70, Mendoza, Argentina
GINGERICH, Howard F., Lemon Admin. Comm., 117 W. 9th St., Room 905, Los Angeles, Calif. 90015
GINKEL, Alfred O., 43 Oak Manor Lane, Pittsford, N.Y. 14534
GINMAN, Peter J., 1302 A University Village, East Lansing, Mich.
GINSBERG, Sarah, 1225 13th St., N.W., Washington 5, D.C.
GINSBURG, Helen, 41-90 Frame Pl., Flushing 55, N.Y.
GINZBERG, Eli, Columbia Univ., Cons. of Human Resources, New York 27, N.Y.
GIOVANNI, Sister Mary, Congregation of St. Agnes, 390 E. Division St., Fond du Lac, Wis.
GIRARD, Richard A., New York Univ., Washington Sq. Col., Dept. of Econ., New York, N.Y.
GIRGUS, Andrew, 671 Old York Rd., Somerville, N.J. 08876
GIROLAMO, Joseph P., 11732 Chelsea, Detroit, Mich. 48213
GISHLICK, Herbert E., Rider Col., Dept. of Econ., Trenton, N.J. 08602
GITLOW, Abraham L., Old Nyack Turnpike, Spring Valley, N.Y. 10977
GITTINGER, J. Price, Center for Dev. Plan., 1525 18th St., N.W., Washington, D.C. 20036
GIUNTA, Agatino J., 1804 Monsey Ave., Scranton 9, Pa.
GIUSTI, Gino P., 200 Park Ave., 55th Fl., New York 17, N.Y.
GIVONS, Meredith B., Millbrook Heights, 314 Hill Dr., State College, Pa. 16801
GLADE, Frederic H., American Univ., Sch. of Bus. Admin., Massachusetts and Nebraska Aves., N.W., Washington, D.C. 20016
GLADE, William P., Sch. of Com., Univ., of Wisconsin, Madison, Wis.
GLAESER, Herbert O., 404-D Lumpkin Rd., Ft. Benning, Ga.
GLAESSNER, Oscar E., 735 Harvard Ave., University City, Mo. 63130
GLAESSNER, Philip, 9019 Burdette Rd., Bethesda, Md. 20014
GLAHE, Fred F., Univ. of Colorado, Dept. of Econ., Boulder, Colo. 80304
GLANTZ, Herbert H., 1701 Alberti Dr., Silver Spring, Md.
GLANTZ, Ronald A., Mellon Hall B-21, Boston, Mass. 02163
GLASER, Elmer E., 4714 Brandywine St., N.W., Washington, D.C. 20016
GLASGOW, John M., 4303 College Main, Bryan, Tex.
GLASSBURNER, J. Bruce, Univ. of Calif., Dept. of Econ., Davis, Calif. 95616
GLASSMAN, Alfred R., 161 Irwin St., Brooklyn, N.Y. 11235
GLASSMAN, Michael L., 3609 W. James, Apt. B, McHenry, Ill.
GLASSMAN, Sidney, 2630 Colston Dr., Chevy Chase 15, Md.
GLATT, Joseph, 314 W. 92nd St., New York 20, N.Y.
GLAZE, Bert T., San Jose State Col., Dept. of Econ., San Jose, Calif. 95114
GLAZIER, Ira A., 25 Appleton St., Cambridge, Mass. 01922
GLEASON, Carl P., USAID/PROGRAM, APO New York, N.Y. 09668
GLEASON, Jane, 1707 Cambridge St., Cambridge, Mass. 02138
GLENDENING, Richard N., 502 C Eagle Heights, Madison, Wis. 53705
GLENNAN, Thomas K., Jr., 5140 Amigo Ave., Tarzana, Calif. 91356
GLENNON, Anthony J., Iona Col., New Rochelle, N.Y.
GLICK, Lee E., 5660 Darlington Rd., Pittsburgh, Pa.
GLICK, Milton L., 200 W. Harding Rd., Springfield, Ohio 45504
GLICK, Norman, 1201 S. Court House Rd., Arlington, Va. 22204
GLICKMAN, David L., 21-72 46th St., Astoria, N.Y. 11105
GLIDDEN, Robert T., 617 Carlisle Rd., Batavia, Ill.
GLISSON, Heyward D., 6519 Northam Rd., Washington, D.C. 20031
GLOSTER, Jesse E., 3201 Wheeler St., Houston, Tex. 77004
GLOVER, Charles A., 515 Upper Mountain Ave., Upper Montclair, N.J. 07087
GLOVER, Frederick J., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
GLOVER, John D., 400 Concord Rd., Weston 93, Mass.
GLOVER, Robert S., Auburn Univ., Agric. Econ. Dept., Auburn, Ala. 36830
GLOVER, Robert W., St. Edwards Univ., 200 Doyle Hall, Austin, Tex.
GLUCK, Harold, 2939 Grand Concourse, Bronx 68, N.Y.
GLUCK, Irving, 1 Laurel Lane, Syosset, L.I., N.Y. 11791
GLUSTOFF, Errol, Box 4767, Stanford Univ., Stanford, Calif. 96128
GLYNN, John J., 1605 Henry St., Alton, Ill. 62003
GNANASEKARAN, K. S., 116 S. 43rd St., Philadelphia, Pa. 19104
GOBAR, A. J., 804 Lois Lane, Fullerton, Calif.
GOCMEN, Emin, Government of Turkey, Central Statis. Office, Ankara, Turkey
GODAIRE, John G., 2413 39th Pl., N.W., Washington 7, D.C.
GODDARD, Earl, Univ. of Colorado, Sch. of Bus., Boulder, Colo. 80302
GODDARD, Frederick O., Duke Univ., Dept. of Econ., Durham, N.C. 27706
GODDARD, Haynes C., 408 W. First St., Apt. 5, Bloomington, Ind. 47401
GOEBEL, Richard H., Jr., 1 Horizon Rd., Fort Lee, N.J. 07025
GOERTZ, Herbert, Hidden Valley Orchard, Tunbridge, Vt. 05077
GOETZ, Charles J., 1674 Valley Rd., Champaign, Ill.
GOETZ, Helen M., 126 N. Stewart Ave., Tucson, Ariz. 85716
GOFFMAN, Irving J., Col. of Bus. Admin., Univ. of Florida, Dept. of Econ., Gainesville, Fla.
GOGERTY, David C., RAND Corp., 1700 Main St., Santa Monica, Calif. 90406
GOIZUETA-MIMO, Felix, 3818 Chestnut St., Apt. A-602, Philadelphia, Pa. 19104
GOKTURK, Sadik, 380 Riverside Dr., 5-E, New York, N.Y. 10025
GOLAY, Frank H., 109 N. Sunset Dr., Ithaca, N.Y.
GOLD, Andrew J., 829 Simpson St., Evanston, Ill. 60201
GOLD, Bela, 209 Lytton Ave., Pittsburgh 13, Pa.
GOLD, David A., 940 E. 106th St., Brooklyn 36, N.Y.
GOLD, Ronald B., 1746 Northwest Blvd., Columbus, Ohio 43212
GOLD, (Mrs.) Sonia S., 209 Lytton Ave., Pittsburgh 13, Pa.
GOLD, Stephen S., Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
GOLDBERG, Benjamin, 149 W. Chester St., Long Beach, L.I., N.Y.
GOLDBERG, Delphis C., 2800 Quebec St., N.W., Washington 8, D.C.
GOLDBERG, Irwin U., 450 E. 63rd St., New York 21, N.Y.
GOLDBERG, Joseph P., 707 Stonington Rd., Silver Spring, Md. 20902
GOLDBERG, Kalman, Dept. of Econ., Bradley Univ., Peoria, Ill.
GOLDBERG, Michael A., 2474 B Virginia St., Berkeley, Calif. 94709
GOLDBERG, Samuel P., Greenacres, 32 Brook Rd., Valley Stream, L.I., N.Y.
GOLDBERG, Simon A., Dominion Bur. of Statis., Ottawa, Ont., Canada
GOLDBERG, Victor P., 2013 Yale Sta., New Haven, Conn.
GOLDBERGER, Arthur S., Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706

GOLDEN, Mal, 706 N. Central Ave., Campbell, Calif. 95008
GOLDEN, Philip, 1316 New Hampshire Ave., N.W., Washington 36, D.C.
GOLDENBERG, Louis, 505 E. 82nd St., New York, N.Y.
GOLDENTHAL, Adolph J., Ford Motor Co., The American Rd., Room 1151, Dearborn, Mich.
GOLDFEIN, Jacob, 385 Grand St., Apt. L 1106, New York 2, N.Y.
GOLDFELD, Stephen M., Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
GOLDFIELD, Edwin D., Bur. of the Census, Statis. Reports Div., Washington, D.C. 20233
GOLDFINGER, Nathaniel, 306 Hamilton Ave., Silver Spring, Md. 20901
GOLDHAMMER, Bernard, c/o Bonneville Power Admin., P.O. Box 3621, Portland, Ore. 97208
GOLDIN, Hyman H., 12 Cedar St., Swampscott, Mass. 01375
GOLDIN, Kenneth D., State Univ. of New York, Econ. Dept., Crosby Hall, Buffalo, N.Y. 14214
GOLDMAN, (Mrs.) Irene S., 1918 W. Hood Ave., Chicago, Ill. 60626
GOLDMAN, Marshall I., 17 Midland Rd., Wellesley, Mass. 02181
GOLDMAN, Morris R., 9626 Evergreen St., Silver Spring, Md.
GOLDMAN, Steven M., Univ. of California, Dept. of Econ., Berkeley, Calif. 94720
GOLDMANN, Hans, Nacional da Industria, Dep. Econ. da Confed., Rua Santa Luzia 685-3, Rio de Janeiro, Brazil
GOLDNER, William, Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
GOLDSMITH, Raymond W., Yale Univ., Dept. of Econ., 37 Hillhouse Ave., New Haven, Conn.
GOLDSTAUB, Jesse, c/o USAID Mission, American Embassy, Managua, Nicaragua
GOLDSTEIN, Harold, 7012 Wilson Lane, Bethesda, Md. 20034
GOLDSTEIN, Harold M., 30 Baskin Rd., Lexington, Mass. 01054
GOLDSTEIN, Henry N., Washington State Univ., 343 Todd Hall, Pullman, Wash. 99163
GOLDSTEIN, Jon, Pomona Col., Dept. of Econ., Carnegie Bldg. 210, Claremont, Calif.
GOLDSTEIN, Mortimer, 405 Hinsdale Lane, Silver Spring, Md. 20901
GOLDSTEIN, Richard L., Univ. of Rochester, Sch. of Medicine, Rochester, N.Y.
GOLDSTEIN, Sidney, 311 Irwin St., Silver Spring, Md.
GOLDSTINE, Abner D., California State Div. of Sav. and Loan, 3440 Wilshire Blvd., 3rd Fl., Los Angeles, Calif. 90005
GOLDSTONE, Seymour Edward, 531 Stinchcomb Dr., Columbus, Ohio 43202
GOLDSTUCKER, Jac L., De Paul Univ., Dept. of Mktg., Chicago, Ill.
GOLDY, Daniel L., 3605 Albemarle, N.W., Washington 8, D.C.
GOLEMBE, Carter H., American Bankers Asso., 815 Connecticut Ave., N.W., Washington, D.C. 20006
GOLIGHTLY, T. H., 376 Parkview, Elmhurst, Ill.
GOLLADAY, Frederick L., 554 Elmwood Ave., Evanston, Ill. 60202
GOLS, A. George, Arthur D. Little, Inc., 35 Acorn Park, Cambridge, Mass.
GOMBERG, Mandel, 3325 Wilder St., Skokie, Ill.
GOMBERG, William, Wharton Sch., Univ. of Pennsylvania, Dept. of Ind., Philadelphia, Pa.
GOMEL, Thomas W., 1636 So. Winona Ct., Denver 19, Colo.
GOMEZ, (Mrs.) Ana Mas, Esplanada 1210, Lomas, Mexico 10, D.F., Mex.
GOMEZ, Henry, Universidad Catolica Andres Bello, Apartado 422, Caracas, Venezuela
GOMEZ, Michael A., III, 83-33 Austin St., Apt. 2P, Kew Gardens, N.Y. 11415
GONZALES, Teddy R., 1302 Division Ave., San Antonio, Tex.
GONZALES-MALAXECHEVARRIA, Angel, Inter-American Development Bank, Washington, D.C.
GONZALEZ, Alfonso, Univ. of South Florida, Soc. Sci. Div., Tampa, Fla. 33620
GONZALEZ, Alfredo, P.O. Box 665, Aguadilla, P.R.
GONZALEZ, Joaquin, 10519 Calumet Dr., Silver Spring, Md.
GONZALEZ, Luis M., 2301 N.E. 20th Ave., Ft. Lauderdale, Fla.
GONZALEZ, Richard J., 48 Tiel Way, Houston, Tex. 77019
GONZALEZ-COFINO, Roberto, 9601 Byeforde Rd., Kensington, Md. 20795
GOOD, Delmar G., 509 S. 4th St., Champaign, Ill.
GOOD, Dorothy, 105 Bayard Lane, Princeton, N.J. 08540
GOODALE, Denis, 8420 117th St., Edmonton, Alta., Canada
GOODART, James, 402 Garfield, Apt. 3, Laramie, Wyo.
GOODE, James S., Mo. Pac. R.R. Co., Traffic Res., Rm. 1701, St. Louis, Mo.
GOODE, Richard B., 2939 Van Ness St., N.W., Apt. 1248, Washington, D.C. 20008
GOODE, Rudyard Byron, 6308 Arvilla, N.E., Albuquerque, N.M.
GOODELL, George S., 2678 Mt. Holyoke Rd., Columbus, Ohio 43221
GOODEN, Orville T., 36 Meadowbrook Dr., Conway, Ark. 72032
GOODGION, Gilbert, West Stockholm, N.Y. 13696
GOODING, Edwin C., Res. Dept., Fed. Res. Bank, 30 Pearl St., Boston, Mass.
GOODING, Elmer R., Univ. of Kansas, Dept. of Econ., Lawrence, Kans. 66045
GOODMAN, Charles S., Wharton Sch., Univ. of Pennsylvania, Philadelphia 4, Pa.
GOODMAN, David E., 227 Whitecotes Lane, Chesterfield, Derbyshire, England
GOODMAN, Hilliard H., 3700 Massachusetts Ave., N.W., Apt. 303, Washington, D.C. 20016
GOODMAN, I. Bernard, Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
GOODMAN, Kennard E., 3705 Strandhill Rd., Shaker Heights, Ohio 44122
GOODMAN, Oscar R., Sch. of Bus., Northwestern Univ., Evanston, Ill.
GOODMAN, Richard J., c/o Great Plains Wheat, Inc., 1030 15th St., N.W., Suite 1008, Washington, D.C. 20005
GOODMAN, Sam, 12 Hidden Valley Dr., Suffern, N.Y. 10901
GOODMAN, Seymour S., Tulane Univ., Dept. of Econ., New Orleans, La. 70118
GOODMAN, Seymour S., 135 N. Abingdon St., Arlington, Va. 22203
GOODMAN, Stephen H., 2835 Yale Sta., New Haven, Conn. 06520
GOODRICH, Carter, Univ. of Pittsburgh, Dept. of History, Pittsburgh, Pa. 15213
GOODSTEIN, Marvin E., Univ. of the South, Dept. of Econ., Sewanee, Tenn. 37375
GOODWIN, Albert F., Jr., 31 Kingsboro Ave., Gloversville, N.Y.
GOODWIN, Craufurd D. W., Dept. of Econ., Duke Univ., Durham, N.C.
GOODWIN, Dorothy C., Dept. of Econ., Univ. of Connecticut, Storrs, Conn. 06268
GOODYEAR, A. W., Prentice-Hall, Inc., Col. Div., Hudson Terrace Bldg., Englewood Cliffs, N.J. 06320
GOOTZEIT, Michael J., 1303 N. 17th St., Lafayette, Ind. 47904
GORBET, Frederick W., Duke Univ., Dept. of Econ., Box 42, Durham, N.C. 27706
GORDMAN, Howard S., 1307 Battle View Drive N.W., Atlanta 27, Ga.
GORDON, Benjamin, 2856 Davenport St., N.W., Washington, D.C. 20008
GORDON, Bernard, 81 E. 91st St., Brooklyn, N.Y. 11212
GORDON, Daniel, 4042 Chestnut St., Philadelphia 4, Pa.
GORDON, Donald F., Dept. of Econ., Univ. of Washington, Seattle 5, Wash.
GORDON, Edgar J., American Embassy, Buenos Aires, State Dept., Washington, D.C.
GORDON, Guy G., Col. of Bus. Admin., Univ. of Washington, Seattle 5, Wash.
GORDON, Howard Scott, Carleton Univ., Ottawa, Ont., Canada
GORDON, Jerome B., Manpower and Pop. Prog.,

Bur. of Ap. Soc. Res., Columbia Univ., 605 W. 115th St., New York, N.Y. 10025
GORDON, Kenneth, Georgetown Univ., Dept. of Econ., Washington 7, D.C.
GORDON, Kenneth F., 48 Valley Circle, Rochester, N.Y. 14622
GORDON, Kermit, 7311 River Rd., Bethesda, Md. 20034
GORDON, Lester, Harvard Univ., Center for Int. Affairs, Dev. Advisory Serv., 6 Divinity Ave., Cambridge 38, Mass.
GORDON, Lincoln, 1915 23rd St., N.W., Washington, D.C. 20008
GORDON, (Mrs.) Margaret S., 984 Creston Rd., Berkeley 8, Calif.
GORDON, Monte Jeffrey, Bache and Co., Services Div., 36 Wall St., New York, N.Y. 10005
GORDON, Myron J., Univ. of Rochester, Col. of Bus., Rochester, N.Y. 14627
GORDON, Nathan N., 5215 31st Rd. N., Arlington, Va.
GORDON, Randal H., 4232 Caruth Blvd., Dallas 25, Tex.
GORDON, Richard L., Pennsylvania State Univ., 301 Engineering D., University Park, Pa. 16802
GORDON, Robert A., 984 Creston Rd., Berkeley 8, Calif.
GORDON, Robert J., 26-A Olney St., Watertown, Mass. 02172
GORDON, Sanford D., 41 Union St., Oneonta, N.Y. 13820
GORDON, Sara, Univ. of Hawaii, Dept. of Econ., 2540 Maile Way, Rm. 359, Honolulu, Hawaii
GORDON, Simeon E., 97-12 103rd Ave., Ozone Park, N.Y.
GORDON, Wendell C., Univ. of Texas, Econ. Dept., Austin, Tex. 78712
GORDON, William E., Atlanta Univ., Grad. Sch., Atlanta, Ga.
GORE, Jack H., Colorado Sch. of Mines, Dept. of Econ., Golden, Colo.
GORHAM, James Edgar, 820 Mission St., S. Pasadena, Calif.
GORHAM, John P., 1510 Jessup Ave., Bronx 52, N.Y.
GORMAN, William M., New Inn Col., East End North Leigh, Witney, Oxfordshire, England
GORMBLEY, William P., Jr., Ford Found., 477 Madison Ave., New York, N.Y. 10022
GORMELEY, Patrick J., Duke Univ., Dept. of Econ., Durham, N.C. 27706
GORMLEY, Edward Paul, 1409 Holdridge Ter., Wayzata, Minn. 55391
GORMLEY, Robert J., Wadsworth Pub. Co., 10 Davis Dr., Belmont, Calif. 94002
GORSKI, Roman S., P.O. Box 222, Mars Hill, N.C. 78542
GORT, Michael, State Univ. of N.Y. at Buffalo, Dept. of Econ., Crosby Hall, Library Circle, Buffalo 14, N.Y.
GORTER, Wytze, Univ. of Hawaii, Grad. Sch., 2540 Maile Way, Honolulu, Hawaii 96822
GOSFIELD, Amor, 2929 Serena Rd., Santa Barbara, Calif. 93105
GOSHAY, Robert C., Univ. of California, Grad. Sch. of Bus., Berkeley, Calif.
GOSS, Bernard, 1710 Deerfield, Austin, Tex. 78741
GOSS, Bert C., Hill and Knowlton, Inc., 150 E. 42nd St., New York 17, N.Y.
GOSSLING, William F., Univ. of Illinois, 309 Mumford Hall, Urbana, Ill.
GOSTOMSKI, Adam, Vicolo Illuminati 4, Assisi, Perugia, Italy
GOTT, George T., Union College, Lincoln 6, Neb.
GOTTLIEB, Elias, 19 North Dr., Livingston, N.J. 07039
GOTTLIEB, Manuel, 3051 N. Prospect, Milwaukee, Wis.
GOTTSCHALK, Richard J., 5166 N. Lovers Lane Dr., Apt. B-13, Milwaukee, Wis. 53225
GOTTSEGEN, Jack J., 3533 Cumberland St., N.W., Washington 8, D.C.
GOUDRIAAN, Dr. Ir. Jan, Univ. of Pretoria, P.O. Box 486, Pretoria, S. Africa
GOUELI, Ahmed A., c/o Mr. A. Sharara, 162 Gist El-Bahr St., Apt. 7, Hadiek Shoubra, Cairo, Egypt
GOUKE, Cecil G., P.O. Box 398, Grambling, La.
GOULD, Burton M., 925 25th St. N.W., Apt. 821, Washington, D.C. 20037
GOULD, Jay M., Market Statistics, 630 3rd Ave., New York 17, N.Y.
GOULD, Joseph S., c/o Jack H. Smith, 25 Liberty St., Albion, N.Y. 14411
GRABNER, John R., Jr., Ohio State Univ., Dept. of Bus. Org., Columbus, Ohio 43210
GRABRYAN, George M., P.O. Box 945, Athens, Ala. 35611
GRADEN, Thurston H., Fram Corp., Providence 16, R.I.
GRADIN, Theodore I., 605 Third Ave., New York, N.Y. 10016
GRADY, (Mrs.) Eleanor H., 2419A Prospect St., Berkeley 4, Calif.
GRADY, William H., New England Tel. and Tel. Co., 185 Franklin St., Boston, Mass. 02107
GRAHAM, Benjamin, 611 N. Maple Dr., Beverly Hills, Calif.
GRAHAM, Charles R., Waterloo Lutheran Univ., Dept. of Econ. and Bus. Admin., Waterloo, Ont., Canada
GRAHAM, Douglas Hume, Getulio Vargas Found., IBRE/CAE, Caixa Postal 4081, Rio de Janeiro, G.B., Brazil
GRAHAM, Harry E., 416 Chamberlain Ave., Madison, Wis. 53705
GRAHAM, John F., Dept. of Econ., Dalhousie Univ., Halifax, N.S., Canada
GRAHAM, John J., 122 Bowdoin St., Boston 8, Mass.
GRAHAM, John Richard W.F., 3515 Durocher St., Apt. 51, Montreal, P.Q., Canada
GRAHAM, Lawrence P., 531 Lookout Dr., San Antonio, Tex. 78228
GRAHAM, Philip Larmon, 311 Luther Lane, Kirkwood, Mo. 63122
GRAHAM, Reginald A., 1303 Collegewood Dr., Ypsilanti, Mich. 48197
GRAHAM, Stanley K., Univ. of Michigan, Dept. of Publications, Ann Arbor, Mich. 48104
GRAHAM, William M., 2778 N. Wakefield St., Arlington, Va. 22207
GRAHAM, William R., P.O. Box 4, Pinetop, Ariz.
GRAM, Harold A., Valparaiso Univ., Valparaiso, Ind. 46383
GRAMBSCH, Paul V., Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
GRAMLEY, Lyle E., 1006 La Grande Rd., Silver Spring, Md.
GRAMM, Warren S., Washington State Univ., Dept. of Econ., Pullman, Wash. 99163
GRAMPP, William D., 5426 Ridgewood Ct., Chicago, Ill. 60615
GRANICK, David, Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison 6, Wis.
GRANT, Arthur, Roosevelt Univ., Econ. Dept., 430 S. Michigan Ave., Chicago, Ill. 60605
GRANT, Paul B., Loyola Univ., 820 N. Michigan Ave., Chicago, Ill. 60611
GRANT, Robert, Flintbacken 10, Stockholm Sv., Sweden
GRANT, Stanley R., 956 Fifth Ave., New York 21, N.Y.
GRANT, Stephen K., 55 Massachusetts Ave., Dedham, Mass. 02026
GRANTHAM, George W., 812 Orange St., New Haven, Conn. 06511
GRASBERG, Eugene, 1720 Que St., N.W., Washington, D.C. 20009
GRASSHOFF, Sven Ulrich, 401 S. Van Dorn St., Apt. 304, Alexandria, Va. 22304
GRATEHOUSE, Donald R., 11414 Woodson Ave., Kensington, Md. 20795
GRAUBARD, Leon S., 164 Quinobequin Rd., Waban 68, Mass.
GRAU-CANALIAS, Jorge A., Apartado #776, San Jose, Costa Rica
GRAUE, Erwin, 304 E. A St., Moscow, Idaho 83843
GRAVES, F. Malcolm, 40 Clinton St., Apt. PH-E, Brooklyn Heights, N.Y. 11201
GRAVES, Levern F., 930 S. Pepper St., Anaheim, Calif.
GRAY, Albert L., Jr., 28 Third Ave., Berea, Ohio 44017
GRAY, Burton C., 1545 E. 60th St., Chicago, Ill. 60637
GRAY, Clive S., c/o Ford Found., P.O. Box 1081, Nairobi, Kenya, E. Africa

GRAY, Edward R., 3501 Williamsburg Lane, Washington 8, D.C.
GRAY, H. Peter, 15,500 Griggs Ave., Detroit, Mich. 48238
GRAY, Harry L., 2901 18th St., N.W., Apt. 303, Washington, D.C. 20009
GRAY, Horace M., Univ. of Illinois, Col. of Com., Dept. of Econ., Urbana, Ill.
GRAY, (Mrs.) Jean M., 15,500 Griggs Ave., Detroit, Mich. 48238
GRAY, John A., 2155 Cram Pl., Apt. 36, Ann Arbor, Mich. 48105
GRAY, Peter J., 35 E. Hartsdale Ave., Hartsdale, N.Y. 10530
GRAY, Ralph, DePauw Univ., Box 37, Greencastle, Ind. 46135
GRAY, Robert D., California Inst. of Tech., Ind. Rela. Ctr., Pasadena, Calif.
GRAY, (Mrs.) Sally H., DePauw Univ., Box 37, Greencastle, Ind. 46135
GRAY, (Mrs.) Virginia M., 3501 Williamsburg Lane, N.W., Washington 8, D.C.
GRAYBEAL, David M., Drew Theological Seminary, Madison, N.J. 07940
GRAYBEAL, Ronald S., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
GRAYSON, Gerald H., 651 Vanderbilt St., Brooklyn 18, N.Y.
GRAYSON, Henry W., Univ. of Hawaii, Dept. of Bus. Econ. and Statis., Honolulu 14, Hawaii
GRAYSON, Leslie E., California Texas Oil, 380 Madison Ave., New York 17, N.Y.
GRAYSON, (Mrs.) Ruth B., 10607 Cavalier Dr., Silver Spring, Md.
GREANEY, Walter T., Jr., 14 Pond Circle, Jamaica Plain, Mass. 02344
GREAVES, Percy Laurie, Jr., P.O. Box 298, Dobbs Ferry, N.Y. 10522
GRECKEL, (Mrs.) Fay E., Evermann Apt. 427, Bloomington, Ind.
GREEF, Albert O., Univ. of Connecticut, Storrs, Conn. 06268
GREEN, (Mrs.) Barbara L., 616 N. Main St., Wheaton, Ill.
GREEN, Christopher, 543 W. Mifflin St., Madison, Wis. 53703
GREEN, George D., Univ. of Minnesota, Dept. of History, Minneapolis, Minn.
GREEN, George R., 902 Nora Dr., Silver Spring, Md. 20904
GREEN, George Warren, Mktg. Strategy, Inc., 3421 Detroit St., Dearborn, Mich.
GREEN, H. A. John, Univ. of Manchester, Dept. of Econ., Dover St., Manchester 13, England
GREEN, H. Albert, 5323 29th Ave., Washington, D.C. 20031
GREEN, James L., Dept. of Econ., Univ. of Georgia, Athens, Ga.
GREEN, Paul Martin, 601 W. Delaware Ave., Urbana, Ill.
GREEN, Paul T., 4705 Henry Hudson Pkwy., Riverdale, N.Y. 10071
GREEN, R. Earl, Sch. of Ind. Mgt., Georgia Inst. of Tech., Atlanta 13, Ga.
GREEN, R. Jeffrey, Univ. of Illinois, Bur. of Econ. and Bus. Res., Urbana, Ill. 61803
GREEN, Ralph T., 4017 Northview Lane, Dallas 29, Tex.
GREEN, Theodore G., 41 Daniel St., Newton Centre, Mass. 01355
GREENBAUM, Harry, South Dakota State Univ., Dept. of Econ., Brookings, S.D.
GREENBAUM, Stuart I., Fed. Res. Bank, Kansas City, Mo. 64106
GREENBERG, Bennett H., 13926 Hesby St., Sherman Oaks, Calif.
GREENBERG, David H., 13 Mt. Auburn, Cambridge, Mass. 02138
GREENBERG, Edward, Washington Univ., Dept. of Econ., St. Louis, Mo.
GREENBERG, Leon, 1821 Sanford Rd., Silver Spring, Md.
GREENBLAT, Edward J., 74 Riverside Dr., Apt. 1F, New York, N.Y. 10024
GREENE, Benjamin B., Jr., 58 Lake St., Boston, Mass.
GREENE, David G., Rutgers Univ., Dept. of Econ., New Brunswick, N.J. 08903
GREENE, David Herbert, 45 Shornecliffe Rd., Newton, Mass. 01355
GREENE, (Mrs.) Esther, 3733 California Ave., Pittsburgh, Pa. 15212
GREENE, Joseph J., Box 21, Sta. A, Hattiesburg, Miss.
GREENE, Kenneth V., 61-43 76th St., Middle Village, N.Y. 11379
GREENE, Lyford N., 2 Herkimer Rd., Scarsdale, N.Y.
GREENE, Margaret L., 415 E. 85th St., Apt. 9B, New York, 28, N.Y.
GREENE, Robert Lee, Univ. of Florida, Dept. of Econ., Col. of Bus., Gainesville, Fla.
GREENE, William R. C., 3733 California Ave., Pittsburgh, Pa. 15212
GREENFIELD, Harry, 2155 Paulding Ave., New York 62, N.Y.
GREENHUT, Melvin, Florida State Univ., Tallahassee, Fla.
GREENING, (Mrs.) Eleanor H., 1 Sherman Circle, Somerset, N.J.
GREENING, George E., One Sherman Circle, Somerset, N.J. 08873
GREENOUGH, William C., Teachers Ins. and Annuity Asso., 730 Third Ave., New York, N.Y. 10017
GREENSLADE, Rush V., 7901 Radnor Rd., Bethesda, Md. 20034
GREENSPAN, Alan, 80 Pine St., New York 5, N.Y.
GREENSPAN, Benjamin, 60 E. 42nd St., New York, N.Y. 10016
GREENSPAN, Harry J., 6512 E. Halbert Rd., Bethesda, Md. 21811
GREENSPAN, S. George, S. George Greenspan Co., 101 Park Ave., New York, N.Y. 10017
GREENSPUN, Nathaniel, 6407 Dahlonega Rd., Washington, D.C. 20016
GREENSTON, Peter M., 2111 21st Ave., S., Minneapolis, Minn. 55404
GREENWALD, Douglas, 250 E. 73rd St., New York, N.Y.
GREENWALD, William I., 23-25 Bell Blvd., Bayside, N.Y. 11360
GREENWOOD, Joen, Wellesley Col., Dept. of Econ., Wellesley, Mass. 02181
GREENWOOD, Michael J., Kansas State Univ., Dept. of Econ., Waters Hall, Manhattan, Kans.
GREER, Douglas F., 1131 Dryden Rd., Ithaca, N.Y. 14850
GREGG, Dorothy, United State Steel Corp., 71 Broadway, New York, N.Y. 10006
GREGORY, G. Robinson, Sch. of Natural Resources, Univ. of Michigan, Ann Arbor, Mich.
GREGORY, Karl D., 17416 Griggs St., Detroit, Mich. 48221
GREGORY, Paul M., Dept. of Econ., Box 6193, University, Ala. 35486
GREGORY, Peter, Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
GREGORY, Wade F., 4620 W. 23rd St., Arlington, Va. 22207
GREIF, Edwin C., Univ. of Vermont, Dept. of Com. and Econ., Burlington, Vt.
GREIF, Jurgen H., 35 E. 30th St., New York, N.Y. 10016
GRELLA, Frank C., Univ. of Hartford, P.O Box 1948, Hartford, Conn.
GRENNES, Thomas J., 5433 S. Cornell Ave., Chicago, Ill. 60615
GRESHAM, Harold Dean, Tuscarora, Md. 21790
GRESSENS, O., 2310 Ewing Ave., Evanston, Ill. 60201
GRETHER, Ewald Theophilus, Univ. of California, 310 Barrows Hall, Berkeley, Calif. 94720
GREY, Arthur L., Jr., Univ. of Washington, Dept. of Urban Plan., Seattle, Wash. 98105
GREY, Kathleen M., 2501 N. Underwood St., Arlington, Va. 22213
GREY, Leo, Sec. 67, V. A. Center, Hampton, Va. 23367
GREYTAK, David, 6223 Louthwood, Apt. E., St. Louis, Mo. 63105
GRIECO, Francisco de A., Rua Sendador Vergueiro, 159, Apto. 502, Rio de Janeiro, Brazil
GRIFFIN, Arthur Gwynn, Davidson College, Davidson, N.C. 28036
GRIFFIN, James B., Wayne State Univ., Econ. Dept., Detroit, Mich. 48202
GRIFFIN, John I., 120 E. 34th St., New York, N.Y. 10016

GRIFFIN, John J., Jr., Marist Col., Dept. of Bus. and Econ., Poughkeepsie, N.Y. 12601
GRIFFIN, Keith B., Magdalen College, Oxford, England
GRIFFITH, Edwin C., Washington and Lee Univ., 29 Sellers Ave., Lexington, Va.
GRIFFITH, John R., Jr., Millikin Univ., Dept. of Bus. and Ind., Decatur, Ill.
GRIFFITH, Mildred E., 20 Jefferson St., Greenville, Pa. 16125
GRIGALUNAS, Thomas A., 22 Norton St., Dorchester, Mass. 02125
GRIGG, Vernon H., P. T. Stanvac Indonesia, Medan Merdeka Selatan 18, Djakarta, Indonesia
GRIGSBY, William G., 711 Briarcliffe Rd., Upper Darby, Pa.
GRIKSCHEIT, Gary M., 865 S. First, Ann Arbor, Mich.
GRILICHES, Zvi, Dept. of Econ., Univ. of Chicago, Chicago, Ill. 60637
GRIMALDI, Paul L., 2292 Bathgate Ave., New York, N.Y. 10458
GRIMBERG, Juan C., 3802 King Arthur Rd., Annandale, Va. 22003
GRIMOLDI, Alberto L., Av. Libertador Gral, San Martin 2202 5°, Buenos Aires, Argentina
GRIMSHAW, (Mrs.) Josephine, 119 Sheldrake Blvd., Toronto 12, Ont., Canada
GRISKIVICH, Peter G., American Petroleum Inst., Com. on Public Affairs, 1271 Ave. of the Americas, New York, N.Y. 10020
GRISSOM, John Kenneth, c/o Luther S. Grissom, Route 1, Rock Island, Tenn. 38581
GRISWOLD, John A., Amos Tuck Sch. of Bus. Admin., Dartmouth Col., Hanover, N.H. 03755
GRODSKI, Gertrude V., Central Michigan Col., Dept. of Soc. Sci., Mt. Pleasant, Mich. 48858
GROEPPER, Gerald W., 3029 Hartzell St., Evanston, Ill.
GROESBECK, James D., 1312-C Brackenridge Apts., Lake Austin Blvd., Austin, Tex. 78703
GROKE, Paul O., Col. of Bus., Northern Illinois Univ., De Kalb, Ill. 60115
GROMEKA, Vasily, 150 West End Ave., Apt. 15-F, New York, N.Y. 10023
GRONING, John C., 1000 Clifton Hills Ave., Cincinnati 20, Ohio
GROSS, Alvin C., Pittsburgh Plate Glass Co., 1 Gateway Center, Pittsburgh, Pa. 15222
GROSS, Andrew C., 611 Acton Rd., Columbus, Ohio 43214
GROSS, Arthur, 2639 W. Somerset St., Philadelphia, Pa. 19132
GROSS, Donald, 1207 Gavrin Blvd., Franklin Square, L.I., N.Y. 11010
GROSS, Jack L., Media Statistics, Inc., 509 Madison Ave., New York, N.Y. 10022
GROSS, James A., 45 Rollinson Rd., Worcester, Mass.
GROSS, (Mrs.) Norma J., 320 Croydon Ave., Apt. 325, Ottawa 14, Ont., Canada
GROSS, Walter, Univ. of Georgia, Dept. of Mktg., Col. of Bus. Admin., Athens, Ga.
GROSSACK, Irvin M., American Embassy AID, APO New York, N.Y. 09675
GROSSCHMID, Geza B., 3115 Ashlyn St., Pittsburgh, Pa. 15204
GROSSCUP, George C., Sch. of Bus. Admin., Miami Univ., Oxford, Ohio 45056
GROSSE, Robert Nathan, 9002 Honeybee Lane, Bethesda, Md. 20034
GROSSE, W. Jack, 837 Wakefield, Cincinnati, Ohio 45208
GROSSET, Serge, Duquesne Univ., Pittsburgh 19, Pa.
GROSSETT, William K., Bell Tel. Co. of Pa., 1 Parkway, 13th Fl., Philadelphia, Pa. 19102
GROSSLING, Bernardo F., 10908 Bucknell Dr., Apt. 1341, Silver Spring, Md. 20902
GROSSMAN, Andrew, 54 Hall Pl., Yonkers, N.Y. 10705
GROSSMAN, Gregory, Dept. of Econ., Univ. of California, Berkeley 4, Calif.
GROSSMAN, Herschel I., Brown Univ., Dept. of Econ., Providence, R.I. 02912
GROSSMAN, Jack, William Esty Co., Inc., 100 E. 42nd St., New York 17, N.Y.
GROSSMAN, Leroy J., 1217 Olivette Dr., Olivette, Mo. 65064
GROSSMAN, M. A., 24011 Bessemer St., Woodland Hills, Calif. 91364
GROSSMAN, Morton C., Ford Found., 477 Madison Ave., New York 22, N.Y.
GROSSMAN, Philip, 11707 Mentone, Silver Spring, Md.
GROTEWOLD, Andreas, Dept. of Geography, Univ. of Missouri, Columbia, Mo.
GROVE, David L., Blythe and Co., Inc., 14 Wall St., New York, N.Y.
GROVE, Ernest W., 5429 S. 5th St., Arlington 4, Va.
GROVES, G. Stanley, P.O. Box 474, Lennoxville, P.Q., Canada
GROVES, Harold M., Dept. of Econ., Univ. of Wisconsin, Madison, Wis.
GROVES, Jay V., West Virginia Univ., Dept. of Econ., Morgantown, W. Va. 26506
GROVES, Theodore F., Jr., 2321 LeConte Ave., Apt. 7, Berkeley, Calif. 94709
GRUBBS, Kenneth R., Box 762, Tech. Sta., Ruston, La.
GRUBER, Alan R., Electro-Optical Systems, Inc., 300 N. Halstead St., Pasadena, Calif. 91107
GRUBER, Josef, Lintnerstrasse 12, 805 Freising, West Germany
GRUBER, William H., Massachusetts Inst. of Tech., Sch. of Ind. Mgt., Cambridge, Mass. 01922
GRUCHY, Allan G., Dept. of Econ., Univ. of Maryland, College Park, Md.
GRUEBEL, Herbert G., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
GRUEBELE, James W., Univ. of Illinois, Dept. of Agric. Econ., 433 Mumford Hall, Urbana, Ill.
GRUEN, (Mrs.) Berta A., 3303 Turner Lane, Chevy Chase, Md. 20015
GRUEN, Claude, 2906 Forest Ave., Berkeley, Calif.
GRUEN, Edward D., 3303 Turner Lane, Chevy Chase, Md. 20015
GRUENBERG, Gladys W., 801 Jackson Ave., St. Louis 30, Mo.
GRUENDEMANN, Paul A., Univ. of Southern California, Sch. of Bus., University Park, Los Angeles, Calif. 90007
GRUNBERG, Emile, Univ. of Akron, Dept. of Econ., Akron, Ohio 44304
GRUNEWALD, Donald, Rutgers Univ., Sch. of Bus., 26 James St., Newark, N.J. 07102
GRUNFEL, Judith, 3701 Connecticut Ave., N.W., Washington 8, D.C.
GRUNWALD, Joseph, Brookings Institution, 1775 Massachusetts Ave., N.W., Washington, D.C. 20036
GRUNWALD, Kurt, Union Bank-Israel, P.O. Box 873, Jerusalem, Israel
GRUSKY, Reuben, Glen Mar Park, 5907 Carlton Lane, Washington 16, D.C.
GRYGIEL, John A., Santa Fe Railway Co., 114 Sansome St., San Francisco 4, Calif.
GUAY, Fernand P., 1523 Michigan Ave., N.E., Washington 17, D.C.
GUBINS, Samuel, 3711 Greenmount Ave., Baltimore, Md. 21218
GUDE, Walter E., 540 Broad St., Newark 1, N.J.
GUDIN, Eugenio, Rua da Candelaria, 19, Rio de Janeiro, Brazil
GUEDES-VIEIRA, Manuel J. da S., Av. 5 de Outubro 194-3-D, Lisbon 1, Portugal
GUERIN, Joseph R., 2419 Bryn Mawr, Ardmore, Pa. 19003
GUERRA, Jose A., 8804 Second Ave., Silver Spring, Md.
GUERRA, Modesto A., 206 E. Locust #5, San Antonio, Tex. 78212
GUESAVAS, Vural, Inspector of Finance, Maliye Mufettisi, Ankara, Turkey
GUETZKOW, Harold, Northwestern Univ., 1834 Sheridan Rd., Evanston, Ill. 60201
GUFFEY, Loren, Arkansas State Teachers Col., Dept. of Econ., Conway, Ark. 72032
GUILD, Lawrence R., 239 S. Van Ness Ave., Los Angeles 4, Calif.
GUILFOIL, John Donald, 3603 21st Ave., Astoria, N.Y. 14710
GUINN, Oliver F., Washburn Univ., Dept. of Econ. and Bus., Topeka, Kans. 66621
GUISINGER, Steve, 17 Rutland St., Cambridge, Mass.
GUJARATHI, Damodar N., City Col. of New York, Baruch Sch. of Bus., Dept. of Econ., 17

Lexington Ave., New York, N.Y. 10010
GULDBERG, Hans H., 2 McCartney Ave., Chatswood, N.S.W., Australia
GUMPERZ, Julian, 630 3rd Ave., New York 17, N.Y.
GUNADI, Kakung, Dj. Samarinda 4, Djakarta, Indonesia
GUNDERSON, Sherman E., Wisconsin State Col., 800 Algoma Blvd., Oshkosh, Wis. 54902
GUNLOGSON, Lee, 635 Mertens Ave., Syracuse 1, N.Y.
GUNN, Grace T., 2420 39th St., N.W., Washington 7, D.C.
GUNN, John McKenzie, Dept. of Econ., Washington-Lee Univ., Lexington, Va.
GUNN, Joseph R., III, Asia Found., c/o American Embassy, Kabul, Afghanistan
GUNNING, Walter E., 69-25 66th St., Glendale, N.Y. 11227
GUNTHER, William D., 1331 Nancy Hanks Rd., Apt. 5, Lexington, Ky. 40504
GUNTON, R. J., Univ. of Queensland, Dept. of Econ., St. Lucia, Brisbane, Queens., Australia
GUNTZEL, Corinne, 314 E. Stoughton, Champaign, Ill. 61822
GUP, Benton E., 1654 Anita Place No. 3, Cincinnati, Ohio 45237
GUPTA, Harish C., Univ. of Tennessee, Col. of Bus., Dept. of Econ., Knoxville 16, Tenn.
GUPTA, Suraj B., Univ. of Western Ontario, Dept. of Econ., London, Ont., Canada
GURLEY, John G., Dept. of Econ., Stanford Univ., Stanford, Calif. 96128
GURLEY, (Mrs.) Yvette, Dept. of Econ., Stanford Univ., Stanford, Calif. 96128
GURTUNCA, Ali, Kambiyo Burdur Muavini, Istanbul, Turkey
GUSS, Leonard M., 619 N. Yakima, Tacoma, Wash.
GUSTAFSON, Eric, Univ. of California, Dept. of Econ., Davis, Calif. 95616
GUSTAFSON, George A., P.O. Box 483, Burlingame, Calif.
GUSTAFSON, Robert L., Dept. of Agric. Econ., Michigan State Univ., East Lansing, Mich.
GUSTUS, Warren J., 329 S. 18th St., Philadelphia, Pa.
GUTCHESS, Franklin J., 2232 Glasgow Rd., Alexandria, Va. 22307
GUTENBERG, Arthur W., P.O. Box 278, San Gabriel, Calif.
GUTH, Herbert J., 5204 Wapakoneta Rd., Washington 16, D.C.
GUTHMANN, Harry George, 718 Noyes St., Evanston, Ill. 60201
GUTHRIE, Harold W., Univ. of Illinois, 414 David Kinley Hall, Urbana, Ill.
GUTHRIE, John A., Sch. of Bus. Admin., Washington State Univ., Pullman, Wash.
GUTHRIE, Paul N., Univ. of North Carolina, Dept. of Econ., Chapel Hill, N.C.
GUTIERREZ, Luis E., Apdo. Postal 1144, San Salvador, El Salvador
GUTIERREZ-T., Luis F., Calle 33 #17-51, Bogota, Colombia
GUTMANN, Franz, 4008 N. Downer Ave., Milwaukee, Wis. 53211
GUTMANN, Peter, City Col. of New York, 17 Lexington Ave., New York, N.Y.
GUTOWSKI, Armin, Parkstr. 39, 62 Wiesbaden, West Germany
GUTOWSKY, Albert R., Arizona State Univ., Dept. of Econ., Tempe, Ariz.
GUTTENTAG, Jack M., 300 W. Sedgwick St., Philadelphia 19, Pa.
GUTTER, Malcolm D., 27 Hinckley Walk, Golden Gateway Center, San Francisco, Calif. 94111
GUY, George, 161 F. Ramos St., Cebu City, Philippines
GUYTON, Percy L., Jacksonville Univ., Div. of Bus. Admin., Jacksonville, Fla. 32211
GUZE, John J., 163-26 21st Rd., Whitestone, N.Y. 11357
GUZELIMIAN, Puzant K., 345 W. 58th St., Apt. 5B, New York 19, N.Y.
GUZMAN-SOTO, Vincente, Government Dev. Bank for P.R., Box 4591, San Juan, P.R.
GYLYS, Julius A., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202

HAAS, John Ross, 433 E. 51st St., New York, N.Y. 10022
HAAS, Richard D., Duke Univ., Dept. of Econ., Durham, N.C. 27006
HAAS, Theodore O., 46 Meadows Lane, Haverford, Pa. 19041
HAASE, Herta, 3941 Langley Ct., N.W. #581, Washington, D.C. 20016
HAAVE, Ronald B., c/o McGraw-Hill, Inc., 330 W. 42nd St., New York, N.Y. 10036
HABACIVCH, William, Univ. of Delaware, Dept. of Econ., Newark, Del. 19711
HABER, Sheldon E., 10302 Leslie St., Silver Springs, Md.
HABER, William, Barton Hills, 530 Hillspeer Rd., Ann Arbor, Mich.
HABERER, John F., IBM Co., Limited, 844 Don Mills Rd., Don Mills, Ont., Canada
HABERKORN, Arnold H., 10013-B Spearfish, Ellsworth AFB, S.D.
HABERLER, Gottfried, Harvard Univ., 326 Littauer Center, Cambridge, Mass. 01922
HABERSTROH, Chadwick J., Case Inst. of Tech., Div. of Org. Sci., Cleveland, Ohio 44106
HACHETTE, Dominique, Univ. Catolica de Chile, Centro de Investigaciones Econ., Santiago, Chile
HACHMAN, Frank C., 1166 E. 6th South, Salt Lake City, Utah 84102
HACKER, Louis M., Columbia Univ., 711 Hamilton Hall, New York 27, N.Y.
HACKETT, John T., Cummings Engine Co., 1000 5th St., Columbus, Ind. 47201
HACKMAN, Joseph, 430 S. Michigan Ave., Chicago, Ill. 60605
HACKMANN, Duane G., Wichita State Univ., Dept. of Econ., Wichita, Kan. 67208
HADAR, Josef, Case Inst. of Tech., Div of Org. Sci., University Circle, Cleveland, Ohio 44106
HADDAD, Edward G., 6513 Millwood Rd., Bethesda, Md. 20034
HADJIYANIS, Demos P., St. Thomas Col., Dept. of Econ., 2115 Summit Ave., St. Paul, Minn. 55101
HADLEY, Eleanor, 807 S.W. 207th Pl., Seattle, Wash 98166
HADLEY, Garland R., Univ. of Missouri, Off. of Res. Admin., 211 Jesse Hall, Columbia, Mo. 65202
HADY, Thomas F., USDA, Comm. Facilities Br., EDD—ERS, Washington, D.C. 20250
HAENER, Albert, 5922 Le Doux Rd., Los Angeles 56, Calif.
HAEUSLER, Robert W., 150 E. 19th St., Brooklyn, N.Y. 11226
HAFERBECKER, Gordon, Wisconsin State Univ., Stevens Point, Wis. 54481
HAGATONG, Armando F. deO., Santa Cruz de Benfica, Rua de Casquilha, 1, Lisbon 4, Portugal
HAGEDORN, George G., 155 Daniel Rd., N., Massapequa, N.Y.
HAGEMAN, Robert F., State University Col., Dept. of Econ., 1300 Elmwood, Buffalo, N.Y. 14222
HAGEMANN, John K., Amer. Embassy, APO New York 09205
HAGEN, Everett E., Center for Int Studies, 50 Memorial Dr., Cambridge, Mass. 01922
HAGENSTEIN, Perry R., 9600 Accord Dr., Potomac, Md. 20854
HAGGIPAVLOU, Paul, 272 Phylis Str., Athens 816, Greece
HAGRAS, Kamal M., 3 Midan El-Tharir, Cairo, Egypt
HAHN, Fred, 780 West End Ave., New York, N.Y. 10025
HAHN, L. Albert, 36, Av. Georges-Mandel, Paris 16, France
HAHN, Philip J., 35 Withers Dr., Youngstown, Ohio 44512
HAHN, Walter L., Col. of St. Rose, Albany, N.Y. 12203
HAIDACHER, Richard C., Purdue Univ., Dept. of Agric. Econ., West Lafayette, Ind. 47907
HAIDER, Agha S., Univ. of Wisconsin, Dept. of Agric. Econ., Madison, Wis.
HAILSTONES, Thomas J., Xavier Univ., Econ. Dept., Cincinnati, Ohio
HAIMANN, Theo, Sch. of Com. and Fin., St. Louis Univ., St. Louis, Mo.
HAIMES, James, 150 E. 18th St., New York 3, N.Y.
HAINES, Claude J., Amarlite Corp., P.O. Box 1719, Atlanta 1, Ga.

HAINES, George H., 10844 LaSalle Ave., Huntington Woods, Mich. 49748
HAINES, Walter W., Skyview Acres, Pomona, N.Y. 10979
HAINSWORTH, Geoffrey B., Williams Col., Dept. of Econ., Williamstown, Mass.
HAITANI, Kanji, Southern Illinois Univ., Dept. of Econ., Carbondale, Ill. 62903
HAKALA, Donald R., 21227 Ellen Dr., Parkview, Ohio 44126
HAKAM, A. N., Univ. of Ghana, Econ. Dept., Legon, Ghana
HAKER, Martin J., 17-B Ridge Rd., Greenbelt, Md. 20770
HALAAS, Eugene T., 1052 Pontiac, Denver, Colo.
HALADEY, Edward G., 711 Halstead Rd., Wilmington, Del. 19803
HALBOUTY, Thomas J., 606 W. 17th, Apt. 413, Austin 1, Tex.
HALD, Earl C., U.N. Mission in Cyprus, United Nations, New York, N.Y.
HALD, (Mrs.) Marjorie W., U.N. Mission in Cyprus, Grand Central Sta., P.O. Box 20, New York, N.Y.
HALDANE, Erik A. R., 1860 Clydesdale P., N.W., Washington, D.C. 20009
HALDEMAN, K. Bobb, 318 S. 19th St., Philadelphia 3, Pa.
HALDI, John, 420 E. 72nd St., Apt. 20-C, New York, N.Y. 10021
HALE, Carl W., Fed. Res. Bank of Dallas, Dallas, Tex.
HALE, E. Alan, 5431 Baja Dr., San Diego, Calif. 92115
HALE, Gerald D., Univ. of Kentucky, Agric. Econ. Dept., Lexington, Ky. 40506
HALE, Jack R., 63 S. Wright Ave., Fairborn, Ohio 45324
HALE, Robert L., 560 Carter St., New Canaan, Conn. 06840
HALE, (Mrs.) Rosemary D., 270 Butler Dr., Lake Forest, Ill. 60045
HALE, Walter S., P.O. Box 503, Red Bluff, Calif. 96080
HALEVI, Naday, 19 Hoofien St., Shikun Ledugma, Ramat Avir, Tel Aviv, Israel
HALEY, Bernard F., 638 Salvatierra St., Stanford, Calif. 96128
HALFTER, Faith G., 222 N. Grove Ave., 2-B, Oak Park, Ill. 60302
HALFYARD, Richard M., Inst. for Defense Anal., E.P.S.D., 400 Army-Navy Dr., Arlington, Va. 22202
HALIORIS, Sarandis, Philippoupoleos 3, Nea Smyrne, Athens, Greece
HALL, Archibald S., Dept. of Econ., Univ. of Nebraska, Lincoln 8, Neb.
HALL, Bernard, Col. of Bus. Admin., Kent State University, Kent, Ohio 44240
HALL, Bill D., 4439 Clifford Ave., Cincinnati 36, Ohio
HALL, Challis A., Jr., Meadow Rd., North Branford, Conn. 06471
HALL, Charles P., Temple Univ., Col. of Bus. Admin., Philadelphia, Pa. 19122
HALL, Clyde L., Dept. of Econ., Austin Col., Sherman, Tex.
HALL, Frances L., 217 8th St., S.E., Washington, D.C. 20003
HALL, Franklin P., 10332 Parkman Rd., Silver Springs, Md.
HALL, George R., The RAND Corp., Logistics Dept., 1700 Main St., Santa Monica, Calif.
HALL, H. Lawrence, 5545 Burning Tree Dr., La Canada, Calif. 91011
HALL, Marshall, Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
HALL, Michael A., Trinity Col., Dept. of Statis, Dublin 2, Ireland
HALL, Robert E., Massachusetts Inst. of Tech., Dept. of Econ., Cambridge, Mass.
HALL, Warren C., Jr., Ohio Univ., Athens, Ohio 45701
HALL, W. Scott, 103 Irvine Rd., Lexington, Ky.
HALLBERG, Milton C., Pennsylvania State Univ., Dept. of Agric. Econ. and Rural Soc., Weaver Bldg., University Park, Pa. 16802
HALLENBECK, Harvey G., Jr., 16716 Frontenac Ter., Derwood, Md. 20752
HALLER, William, Jr., Hundred Acre Pond, West Kingston, R.I. 02892
HALLEY, Donald M., Tulane Univ., Sch. of Bus. Admin., New Orleans, La. 70118
HALLEY, Richard B., Portland State Col., 1620 Southwest Park Ave., Portland 1, Ore.
HALLINAN, Timothy, USOM/Saigon AD/PE, c/o Amer. Embassy, APO San Francisco 96243
HALLOWELL, Burton C., Wesleyan Univ., North Col., Middletown, Conn.
HALM, George N., Tufts Univ., Medford, Mass. 02052
HALPERIN, A., Bank Leumi le-Israel, Econ. Res. Dept., Tel-Aviv, Israel
HALPERIN, Ricardo A., 100 La Salle St., Apt. 11-C, New York, N.Y. 10027
HALPIN, Charles A. J., Jr., 111 Walnut St., Jenkintown, Pa. 19046
HALTERMAN, Jean C., 709 N. 3rd St., Vincennes, Ind. 47591
HALTERMAN, John F., 17 Pueblo Vista Rd., Santa Barbara, Calif. 93103
HALTMIER, Robert F., 97-25 118th St., Richmond Hill, N.Y. 13144
HALVERSON, George C., 149 N. Gordon Way, Los Altos, Calif.
HALVORSEN, Marcia L., Spelman Col., Dept. of Hist. and Soc. Sci., Atlanta 3, Ga.
HALVORSON, Harlow W., Univ. of Wisconsin, 305 Agric. Hall, Madison 6, Wis.
HAM, Clarence L., San Fernando Valley State Col., Dept. of Econ., Northridge, Calif.
HAMADOU, Rachid, Lot. Raphael—3rd Impasse Bainem, Algiers, Algeria
HAMANN, (Mrs.) Lila T., Beverly Pl. and Briar Lane, Lake Forest, Ill. 60045
HAMANN, Walter C., Beverly Pl. and Briar Lane, Lake Forest, Ill. 60045
HAMBERG, Daniel, State Univ. of New York at Buffalo, Dept. of Econ., Buffalo, N.Y.
HAMBRIGHT, William E., 3730 Sunnyvale Ave., Sacramento, Calif. 95821
HAMBURG, Morris, Univ. of Pennsylvania, Wharton Sch., Philadelphia, Pa. 19104
HAMBURGER, William, Univ. of North Carolina, Carroll Hall, Chapel Hill, N.C.
HAMELMAN, Paul W., Col. of Com., West Virginia Univ., Morgantown, W.Va.
HAMERMESH, Daniel S., 3405 Yale Sta., New Haven, Conn. 06520
HAMILTON, Daniel C., 4512 S. Birmingham Ave., Tulsa, Okla. 74105
HAMILTON, David B., Jr., Univ. of New Mexico, Dept. of Econ., Albuquerque, N.M.
HAMILTON, Earl J., 1438 Bunker Ave., Flossmoor, Ill. 60422
HAMILTON, Henry G., Univ. of Florida, 162 McCarty Hall, Gainesville, Fla.
HAMILTON, James L., 2200 Elder St., Carolee Apts. #10, Durham, N.C. 27705
HAMILTON, Kenneth N., Jr., 39 Whitney Dr., Berkeley Heights, N.J.
HAMILTON, Richard, York Univ., Dept. of Econ., 2275 Bayview Ave., Toronto 12, Ont., Canada
HAMILTON, Roger S., Northeastern Univ., Boston, Mass.
HAMM, (Mrs.) Beula W., Stephen F. Austin State Col., Dept. of Bus. Admin. and Econ., Box 6212, SFA Station, Nacobdoches, Tex.
HAMM, William G., 510 Lawrence St., Apt. 3, Ann Arbor, Mich. 48104
HAMMAN, Robert L., Pennsylvania Military Col., Div. of Bus. Admin., Chester, Pa.
HAMMELTON, Raynal L., 3509 Lemon Ave., Long Beach 7, Calif.
HAMMER, Albert R., Jr., 705-B Nimitz, China Lake, Calif. 96105
HAMMER, Frederick S., 150 Elderberry Rd., Mineola, N.Y.
HAMMER, Homer H., 4404 N. 44th St., Tacoma 7, Wash.
HAMMER, Robert B., IBM Corp., 112 E. Post Rd., White Plains, N.Y.
HAMMERBACHER, Irene, 6068 Metropolitan Ave., Brooklyn 27, N.Y.
HAMMOND, Charles, Jr., 1705 Harvard St., N.W., Washington 9, D.C.
HAMMOND, Charles E., 6015 North 22nd Rd., Arlington, Va. 22205
HAMMOND, J. D., Pennsylvania State Univ., Col. of Bus. Admin., Boucke Bldg., University Park, Pa. 16802
HAMMOND, Leigh H., North Carolina State

Col., Dept. of Agric. Econ., P.O. Box 5576, State College Sta., Raleigh, N.C. 27607
HAMOVITCH, William, Queens Col., Flushing 67, N.Y.
HAMOWY, Ronald, 63-84 Saunders St., Rego Park, N.Y. 13679
HAMPTON, Robert N., 1451 Ambleside Dr., Deerfield, Ill. 60015
HAMURDAN, Yusaf O., c/o State Plan. Org., Yeni Meclis, Ankara, Turkey
HAMZAOUI, Abdul-Aziz, 1, Rue Bourbonnais, Mégrine-Coteaux, Tunis, Tunisia
HAN, Charles C. S., Socony Mobil Oil Co., Inc., 150 E. 42nd St., New York, N.Y. 10017
HAN, Hsin-Pao, 530 Riverside Dr., Apt. 5G, New York 27, N.Y.
HAN, Kee Chun, Col. of Com. and Econ., Yonsei Univ., Seoul, Korea
HAN, Pyo Byung, Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta 13, Ga.
HAN, Vo Xuan, Univ. of Hue, 3, Le Loi, Hue, Vietnam
HANC, George, 14 Ridge Rd., Cresskill, N.J. 07626
HANCHETT, Paul E., 1617 Hyde Park Blvd., Chicago, Ill. 60615
HANCOCK, Keith, Univ. of Adelaide, Bedford Park Plan. Off., Adelaide, South Australia, Australia
HANCOCK, Richard A., 2101 Glenhaven, #4, Abilene, Tex. 79603
HANCOCK, Robert S., School of Bus. Admin., Univ. of Minnesota, Minneapolis, Minn.
HANCOCK, Tom, 2314 Woodview Dr., Lansing 10, Mich.
HANCZARYK, Edwin W., 3331 Fairhill Dr., S.E., Washington 23, D.C.
HAND, George H., Southern Illinois Univ., Carbondale, Ill.
HANDA, Jagdish C., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore, Md. 21218
HANDA, M. L., Univ. of Western Ontario, Univ. Col. of Arts and Sci., Dept. of Econ. and Soc., London, Ont., Canada
HANDA, Trilok N., 16, Prince Arthur Rd., London, N.W. 3, England
HANDEL, Sidney S., 914 N. Beverly Glen Blvd., Los Angeles, Calif. 90024
HANDSAKER, (Mrs.) Marjorie L., 717 West Lafayette St., Easton, Pa. 18042
HANDSAKER, Morrison, 717 W. Lafayette St., Easton, Pa. 18042
HANDY, Uvan, 8134 Underhill Dr., St. Louis, Mo. 63114
HANER, Frederick T., 46 Giralda Walk, Naples Island, Long Beach, Calif. 90803
HANEY, Emil B., Jr., Univ. of Wisconsin, 310 King Hall, Madison, Wis. 53706
HANGA, Fred, 951 Western Ave., Albany, N.Y.
HANIESKI, John F., Purdue Univ., Dept. of Econ., Lafayette, Ind.
HANIS, Edward H., Indiana Univ., Dept. of Econ., Ballantine Hall, Bloomington, Ind.
HANKE, Steve H., Univ. of Colorado, Econ. Dept., Boulder, Colo. 80304
HANKS, James J., 5503 Chevy Chase Pkwy., N.W., Washington, D.C. 20015
HANLEY, James F., Creighton Univ., Dept. of Econ., Omaha, Neb. 68131
HANLEY, John P., 126 Champlin Ave., Liberty, N.Y. 12754
HANLINE, Manning H., Central Michigan Univ., Sch. of Bus. Admin., Mount Pleasant, Mich. 48858
HANLON, J. William, Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
HANNA, Frank A., 2239 Cranford Rd., Durham, N.C.
HANNAFORD, John W., 2501 Ethel, Muncie, Ind.
HANNOUN, Mtanous, 520 Bell St., Reno, Nev.
HANNUM, William H., Syracuse Univ., Col. of Bus. Admin., Syracuse, N.Y. 13210
HANNUSH, Basim A., Econ. and Social Off. in Beirut, P.O. Box 4656, Beirut, Lebanon
HANON, Bernard, 129 E. 65th St., New York, N.Y. 10021
HANOVER, Nelson, 225 Cooledge Ave., Watertown, Mass. 01083
HANRAHAN, George D., St. Louis Univ., 3674 Lindell Blvd., St. Louis 8, Mo.
HANSEN, Alvin H., 56 Juniper Rd., Belmont, Mass. 02178
HANSEN, Bent, OECD, 2 rue Andre-Pascal, Paris XVI^{e}, France
HANSEN, Bertrand L., 1103 Saratoga Dr., Fairborn, Ohio 45324
HANSEN, David E., Eaton and Howard, Inc., Boston, Mass. 02110
HANSEN, Eldred K., 1554 N. 49th St., Milwaukee 8, Wis.
HANSEN, Kenneth I., 1618-A Spartan Village, East Lansing, Mich. 48823
HANSEN, Niles M., Univ. of Texas, Dept. of Econ., Austin 12, Tex.
HANSEN, Reed R., Washington State Col., Todd Hall, Pullman, Wash.
HANSEN, Richard H., R.R. 1, Milan, Ill. 61264
HANSEN, W. Lee, Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53705
HANSEN, William N., 6327 Lochdale St., Vancouver, B.C., Canada
HANSON, Arthur W., 15 Charlesbank Rd., Newton, Mass. 02158
HANSON, Bertil, 1902 Brushcliff Rd., Pittsburgh, Pa. 15221
HANSON, Eric J., 8306 139th Street, Edmonton 3, Alta., Canada
HANSON, James A., Brown Univ., Dept. of Econ., Providence, R.I. 02912
HANSON, James W., Plandome Manor, 44 Colonial Dr., Manhasset, N.Y.
HANSSON, Karl-Erik, 6 Peter Cooper Rd., Apt. MG, New York 10, N.Y.
HANUSHEK, Eric Alan, 264 Harvard St., Apt. 7, Cambridge, Mass. 02139
HANWELL, George L., Henstedt-Rehn bei Ulzburg, am Heidberg 31 b, Holstein, West Germany
HANWIECK, Gerald A., Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
HAQ, Mohammad I., 2/74-N P.E.C.H.S., Karachi 29, Pakistan
HARBELL, John B., 28 Los Amigos, Orinda, Calif. 94563
HARBERGER, Arnold C., Univ. of Chicago, Chicago, Ill. 60637
HARBESON, Robert W., Univ. of Illinois, 323 David Kinley Hall, Urbana, Ill.
HARBISON, Frederick H., Ind. Rela. Sec., Princeton Univ., Princeton, N.J. 08540
HARDBECK, George W., 621 Hillcrest Dr., Knob Noster, Mo. 65336
HARDEN, Warren R., Illinois State Univ., Dept. of Soc. Sci., Normal, Ill. 61761
HARDESTY, Thomas M., Univ. of Maryland, Dept. of Econ., College Park, Md. 20742
HARDGRAVE, Gerald D., 14 Coatsland Dr., Jackson, Tenn. 38301
HARDIE, Ian W., Univ. of Connecticut, Dept. of Agric. Econ., Storrs, Conn. 06268
HARDIE, Robert S., Box 7, RFD 1, Ozark, Mo. 65721
HARDIN, Einar, Michigan State Univ., Sch. of Labor and Ind. Rela., East Lansing, Mich.
HARDIN, Lowell S., Dept. of Agric. Econ., Purdue Univ., Lafayette, Ind.
HARDT, John P., 8028 Cindy Lane, Bethesda, Md. 20034
HARDY, J. Hazen, Jr., 751 Braeburn Lane, Penn Valley, Narberth, Pa. 19072
HARDY, Rudolph W., 43 Cypress Rd., Wellesley Hills, Mass. 02181
HARE, Michael Jan, Univ. of Toronto, Dept. of Polit. Econ., Toronto, Ont., Can.
HAREN, Claude C., 1615 Kenyon St. N.W., Washington 10, D.C.
HARGREAVES, Herbert W., 237 Cassidy Ave., Lexington, Ky.
HARGROVE, Merwin M., Col. of Bus. Admin., University of Tulsa, Tulsa 4, Okla.
HARING, H. Albert, Indiana Univ., Sch. of Bus., Bloomington, Ind. 47401
HARING, Joseph E., Dept. of Econ., Occidental Col., Los Angeles 41, Calif.
HARING, Robert C., Univ. of Alaska, Col. of Bus. Econ. and Gov., College, Alaska 99735
HARKAVY, Oscar, 15 Split Rock Lane, New Rochelle, N.Y.
HARLAN, Charles L., 4722 N. 26th St., Arlington, Va.
HARLOW, Robert L., Carleton Col., Northfield, Minn. 55057

HARMAN, Alvin J., Apt. No. 1308, Westgate, Cambridge, Mass. 02139
HARMON, David P., Jr., 105-B Taylor Ave., East Brunswick, N.J. 08816
HARMON, Elmer M., 409 Woodland Pl., Leonia, N.J. 07737
HARMON, Richard P., 187 Beech St., Eastchester, N.Y. 10707
HARMS, Louis T., 6603 N. 12th St., Philadelphia, Pa. 19126
HARMSTON, Floyd K., 2807 W. Henley Drive, Columbia, Mo. 65201
HARMSTONE, Richard C., 3671 Hudson Manor Ter. Apt. 15M, New York (Riverdale), N.Y.
HARNETT, Donald L., Indiana Univ., Sch. of Bus., Bloomington, Ind.
HAROIAN, Berch, 20-68 26th St., Astoria, N.Y. 14710
HARPER, Donald V., Sch. of Bus. Admin., Univ. of Minnesota, Minneapolis 14, Minn.
HARPER, Joel W. C., 4200 Cathedral Ave., N.W., Washington, D.C. 20016
HARRAL, Clell G., c/o Regional Tran. Survey, 8, Lindsay St., Calcutta 16, India
HARRELL, Cleon, North Carolina State Univ., 140 Harrelson Hall, Raleigh, N.C.
HARRIES, Hu, 204-8631 109th St., Edmonton, Alta., Canada
HARRIGAN, Arthur W., Jr., 3 Buxton Rd., Chatham, N.J. 07928
HARRIMAN, Gerald E., 2513 Altura Dr., Cincinnati 39, Ohio
HARRINGTON, John J., Jr., 36 Oakview Ave., Maplewood, N.J. 07040
HARRINGTON, (Mrs.) Lillian G., 2901 18th St., N.W., Washington, D.C. 20009
HARRINGTON, Robert W., Indiana State Col., Terre Haute, Ind.
HARRIS, Bernice, 125 Christopher St., New York, N.Y. 10014
HARRIS, Britton, 114 W. Rittenhouse St., Philadelphia 44, Pa.
HARRIS, Cureton, P.O. Box 85, Princeton Junction, N.J. 08550
HARRIS, Curtis C., Jr., Dept. of Com, Div. of Reg. Econ., Washington 25, D.C.
HARRIS, Donald J., 40 Turnure St., Bergenfield, N.J. 07621
HARRIS, Donald J., Univ. of Illinois, Dept. of Econ., Urbana, Ill. 61803
HARRIS, Edmond S., 6614 Harlan Pl., N.W., Washington, D.C. 20012
HARRIS, George A., c/o White Motor Co., 100 Erieview Plaza, Cleveland, Ohio 44114
HARRIS, George T., Col. of Bus. Admin., Univ. of Omaha, Omaha, Neb.
HARRIS, James G., Univ. of Oregon, Dept. of Econ., Eugene, Ore. 97403
HARRIS, John B., 2816 Edgewood Ave., Richmond, Va. 23222
HARRIS, John R., 1715 Wesley Ave., Evanston, Ill. 60201
HARRIS, John T., 2877 Woodland Park Dr., N.E., Atlanta, Ga. 30329
HARRIS, Paul E., 845 Center Way, S.W., Birmingham 4, Ala.
HARRIS, Paul H., 422 N. Rockford Ave., Rockford, Ill.
HARRIS, Philip, City Col. of the City, Univ. of New York, Dept. of Bus. Admin., 17 Lexington Ave., New York, N.Y. 10010
HARRIS, Randolph L. P., 77 Sacramento St., Somerville, Mass. 02143
HARRIS, Robert, 2912 Westcott St., Falls Church, Va.
HARRIS, Robert E., 3341 Dent Pl., N.W., Washington, D.C. 20007
HARRIS, Roger N. S., O.E.C.D. 94me Chardon Lagache, Technical Action Div., Paris 16, France
HARRIS, Seymour E., Univ. of California, San Diego, Admin. Bldg., La Jolla, Calif. 92038
HARRIS, Stephen E., 1334 Ft. Stevens Dr., N.W., Washington, D.C. 20011
HARRIS, Stephen L., 7920 Cote St., Luc Rd., Apt. 200, Montreal 29, Que., Canada
HARRIS, Thomas D., Univ. of Manitoba, Dept. of Econ., Winnipeg, Man., Canada
HARRIS, Thomas J., 327 N.W. 18th St., Oklahoma City, Okla.
HARRIS, W. Carlton, Univ. of Pennsylvania, Wharton Sch. of Fin. and Com, Philadelphia 4, Pa.
HARRIS, William G., 2001 N. Adams St., Apt. 1033, Arlington, Va.
HARRISON, Ada M., Carleton Col., Northfield, Minn. 55057
HARRISON, Arthur J., 27 West 55 St., New York, N.Y. 10019
HARRISON, Frank, 433 Locust St., Winnetka, Ill.
HARRISON, Harlan A., Amer. Embassy USAID, APO New York 09676
HARRISON, Ike H., Texas Christian Univ., Sch. of Bus., Ft. Worth, Tex.
HARRISON, Joseph W., 7 Mott Rd., Baldwinsville, N.Y. 13027
HARRISON, Kelly M., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich.
HARRISON, Reese L., Jr., P.O. Box 477, San Antonio, Tex. 78206
HARRISON, Richard S., 7244 Paxton Ave., Chicago, Ill. 60649
HARRISS, C. Lowell, Columbia Univ., 712 Hamilton Hall, New York, N.Y. 10027
HARROD, R. F., Christ Church, Oxford, England
HARRON, Meyer J., 10707 Margate Rd., Silver Spring, Md.
HARSANYI, John C., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
HARSHBARGER, Richard B., Manchester Col., Dept. of Bus. and Econ., North Manchester, Ind. 46962
HART, Albert G., Columbia Univ. Dept. of Econ., 503 Fayerweather Hall, New York 27, N.Y.
HART, Alex L., Box 152, Greenlawn, L.I., N.Y. 11740
HART, Donald J., Univ. of Florida, Sch. of Bus. Admin., Gainesville, Fla.
HART, Henry J., American Oil Co., 910 S. Michigan Ave., Chicago, Ill. 60680
HART, James A., De Paul Univ., 25 E. Jackson Blvd., Chicago, Ill. 60604
HART, Joe W., 1423 Walnut St., Apt. 2, Berkeley, Calif. 94709
HART, John N., 298 Aurora St., Hudson, Ohio 44236
HART, Maurice I., 19 Bonnett, Larchmont, N.Y. 10538
HART, Orson H., Indian Waters Dr., New Canaan, Conn.
HARTE, Samuel, 305 East 40th St., New York 16, N.Y.
HARTER, (Mrs.) Charlotte T., Oregon State Col., Dept. of Econ., Corvallis, Ore.
HARTER, L. G., Jr., Oregon State Col., Dept. of Econ., Corvallis, Ore.
HARTLE, Douglas G., 128 Glenrose Ave., Toronto 7, Ont., Canada
HARTLEY, Gerard V., 245 Rumsey Rd., Yonkers, N.Y. 10701
HARTLEY, Joseph R., Indiana Univ., Bryan Hall 135, Bloomington, Ind.
HARTLEY, Michael J., Univ. of Pennsylvania, Econ. Res. Services Unit, Dept. of Econ., Philadelphia, Pa. 19104
HARTLEY, William B., 103 Eagle Hgts., Apt. L, Madison, Wis. 53705
HARTMAN, George E., Univ. of Cincinnati, Col. of Bus. Admin., Cincinnati 21, Ohio
HARTMAN, L. M., Colorado State Univ., Dept. of Econ., Ft. Collins, Colo.
HARTMAN, Paul T., Univ. of California, Dept. of Econ., 250 Barrow Hall, Berkeley, Calif. 94720
HARTMAN, Robert W., Brandeis Univ., Dept. of Econ., Waltham, Mass. 02154
HARTUNG, Bruno J., Wheeling Col., Econ. Dept., Wheeling, W.Va.
HARTWIG, Richard T., 125 E. Prospect, Marquette, Mich. 49855
HARTZ, Clyde D., 4133 Oak, Apt. 4, Kansas City, Mo. 64111
HARVEY, Andrew S., 1431 Le Marchant St., Halifax, N.S., Canada
HARVEY, Bartlett, 1506 Turkey Run Rd., McLean, Va. 22101
HARVEY, Curtis E., 8220 Hamilton Spring Ct., Bethesda, Md. 20034
HARVEY, (Mrs.) Jacquelin E., Harvey-Reynolds, Inc., Cambridge, Md. 21613

HARVILL, Richard A., Univ. of Arizona, Tucson, Ariz.
HARWITZ, Mitchell, State Univ. of New York at Buffalo, Dept. of Econ., Crosby Hall, Library Circle, Buffalo, N.Y. 14214
HARWOOD, D. G., Jr., 4404 Boxwood Rd., Raleigh, N.C.
HARWOOD, Edward C., Amer. Inst. for Econ. Res., Great Barrington, Mass. 01230
HASEK, Carl W., 2170 McClellan Pkwy., Sarasota, Fla.
HASELTINE, Robert W., State Univ. of New York, Dept. of Soc. Studies, Geneseo, N.Y. 14454
HASEMAN, Wilber C., Univ. of Missouri, Bus. & Public Admin. Sch., Columbia, Mo.
HASENBERG, Werner, 1515 Ogden St. N.W., Washington, D.C. 20010
HASHIMI, Rasool M. H., Southern Illinois Univ., Dept. of Econ., Edwardsville, Ill. 62025
HASHIMOTO, Masanori, 420 W. 119th St., Apt. 52, New York, N.Y.
HASHMI, Sajjad Ahmad, 6919 N. Sheridan, Apt. 202, Chicago, Ill.
HASHMI, Sharafat, Univ. of Southern California, Pakistan Project, University Park, Los Angeles, Calif. 90007
HASKELL, John R., P.O. Box 213, Durham, N.H. 03824
HASKINS, Bill D., Modesto Junior Col., Modesto, Calif.
HASSAN, Mostafa F., Univ. of Miami, Dept. of Econ., Coral Gables, Fla. 33926
HASSANEIN, Medhat A., Sherwood Courts Apts., 4417 Pine St., Apt. 402-A, Philadelphia, Pa. 19104
HASSELL, Edward W., 1008 N. Potomac St., Arlington, Va.
HASSEN, Raja, 900 North Ave. E., Haskell, Tex. 79521
HASSON, Joseph A., 6405 Camrose Ter., Bethesda, Md. 21811
HASTINGS, Delbert C., Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
HASTINGS, Hudson B., 135 Milbrook Rd., Hamden 18, Conn. 06439
HASTINGS, V. Stevens, 4 Upton Lane, Yardley, Pa. 18854
HATANAKA, Michio, Univ. of Rochester, Dept. of Econ., Rochester, N.Y.
HATCHER, Joseph W., 736 W. Dempster St., Apt. 209, Mt. Prospect, Ill. 60057
HATCHETT, Texanna L., 3133 N.W. 12th St., Oklahoma City, Okla. 73107
HATKIN, Sidney, Dept. of Econ. Dev., State Office Bldg., Annapolis, Md. 21401
HATTON, Charles S., 1900 S. Eads St., Apt. 905, Arlington, Va. 22202
HATTWICK, Richard E., Univ. of Houston, Col. of Bus. Admin., Dept. of Econ., Cullen Blvd., Houston 4, Tex.
HAUGE, Gabriel S., c/o Manufacturers Hanover Trust Co., 350 Park Ave., New York, N.Y. 10022
HAUPT, Jerry R., Sarnoff Res. Center, RCA Labs., Princeton, N.J. 08540
HAUPTMANN, Jerzy, Park Col., Dept. of Polit. Sci., Parkville, Mo. 64152
HAUSE, John C., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
HAUSER, Francis L., 4000 Massachusetts Ave., N.W., Washington, D.C.
HAUSER, Karl H., Dettweilerstrasse 5, Kronberg/Ts., Germany
HAUSGAARD, Olaf, 67 W. Tremont Ave., Bronx, N.Y. 10453
HAUSMAN, Leonard J., 1402 Regent St., Apt. 561, Madison, Wis. 53711
HAUS-SOLIS, Juan, 4530 Connecticut Ave. N.W., Apt. 712, Washington, D.C. 20008
HAUSWALD, Edward L., Univ. of Nebraska, Bur. of Bus. Res., 310C Soc. Sci. Bldg., Lincoln, Neb. 68508
HAVEMAN, Robert H., Grinnell Col., Div. of Soc. Studies, Grinnell, Iowa 50112
HAVENS, R. Murray, P.O. Box 1609, University, Ala. 35486
HAVENS, Richard W., Industrial Valley Bank & Trust Co., 1518 Walnut St., Philadelphia, Pa. 19102
HAVER, Cecil B., Macdonald Col. of McGill Univ., Dept. of Agric. Econ., Macdonald College, Que., Canada
HAVILAND, W. E., Canadian Pulp and Paper, 2280 Sun Life Bldg., Montreal 2, Que., Canada
HAVRILESKY, Thomas, 1772 Roger Ave., Glenview, Ill
HAWES, Maurice L., 3215 Ellicott St. N.W., Washington 8, D.C.
HAWK, Hugh K., 38 Spindletree Rd., Country Club Sec., Levittown, Pa.
HAWKES, George R., 20944 Arminta St., Canoga Park, Calif.
HAWKINS, Clark A., Florida State Univ., Sch. of Bus., Tallahassee, Fla. 32306
HAWKINS, Dale R., Weber Col., Ogden, Utah
HAWKINS, Everett D., Univ. of Wisconsin, 307 Soc. Sci. Bldg., Madison, Wis. 53706
HAWKINS, Robert G., 360 W. 22nd St. Apt. 7-M, New York, N.Y. 10011
HAWKINS, Thomas B., 336 Capp St., San Francisco, Calif.
HAWKINSON, James R., Northwestern Univ., Sch. of Bus., Evanston, Ill. 60201
HAWTREY, Ralph G., 29 Argyle Rd., London W. 8, England
HAY, George A., Northwestern Univ., Dept. of Econ., Evanston, Ill. 60201
HAY, George A., Jr., Reed Col., Dept. of Econ., Portland 2, Ore.
HAY, Keith A. J., Carleton Univ., Dept. of Econ., Ottawa 1, Ont., Canada
HAY, Richard K., Box 7, Emmett, Kans. 66422
HAYASHI, Paul M., Arlington State Col., Div. of Bus. and Econ., Arlington, Tex. 76010
HAYCOCK, Edwin B., 2305 Lynwood Dr., Salt Lake City, Utah 84109
HAYDEL, Belmont F., Jr., Amer. Consulate Gen. (Sp), APO New York 09676
HAYDEN, Francis G., Box 7614, University Sta., Austin, Tex.
HAYDEN, Jack L., Airport Sta., Box 90722, Los Angeles, Calif. 90009
HAYDON, Randall B., 912 Crain Ave., Kent, Ohio
HAYEK, F. A., Rechts-und Staatsw. Fakult. der Universitat, Werthmannplatz 1, Freiburg 1 B, Germany
HAYENGA, Marvin L., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich.
HAYES, David G., 5701 N. 35th St., Arlington, Va. 22207
HAYES, Francis J., Loyola Col., Montreal 28, Que., Canada
HAYES, Frederick O'R., 2117 Paul Springs Rd, Alexandria, Va. 22307
HAYES, James L., Duquesne Univ., Sch. of Bus. Admin., 600 Forbes Ave., Pittsburgh, Pa.
HAYES, Joseph M., A-1, Hayes Bldg., Presidential Gardens, Alexandria, Va. 22300
HAYES, Marion, 2853 Ontario Rd. N.W., Apt. 201, Washington, D.C. 20009
HAYES, Samuel P., 468 Riverside Dr., Apt 62-A, New York, N.Y. 10027
HAYES, William A., De Paul Univ., 25 E. Jackson Blvd., Chicago, Ill. 60604
HAYMES, Harmon H., 7516 Rockfalls Dr., Richmond, Va.
HAYN, Rolf, 21 Primrose Ave. W., White Plains, N.Y. 10607
HAYNES, Charles R., Siena Col., Loudonville, N.Y. 13659
HAYNES, Lawrence W., 5802 Melvern Dr., Bethesda, Md. 20034
HAYNES, Thomas E., 1271 Asbury Ave., Winnetka, Ill.
HAYNES, William W., Harvard Bus. Sch., Soldiers Field, Boston 63, Mass.
HAYTHORNE, George V., 2190 Alta Vista Dr., Ottawa 8, Ont., Canada
HAYWOOD, Charles F., Bank of America N. T. and S. A., Box 3415, Rincon Annex, San Francisco 20, Calif.
HAZARD, (Mrs.) Dorothy C., 4710 Langdrum Lane, Chevy Chase, Md. 20015
HAZEN, N. William, 110-45 Queens Blvd., Forest Hills 75, N.Y.
HAZLETON, Jared E., 17 Clark Rd., Bedford, Mass.
HAZLEWOOD, Leyland R., 737 Putnam Ave., Brooklyn 21, N.Y.
HAZLITT, Henry, 65 Drum Hill Rd., Wilton, Conn. 06897
HAZZAN, Benjamin D., 7461 Kingsley Rd., Apt. 407, Cote St. Luc. 29, Que., Canada

HEAD, John G., Australia Nat. Univ., Sch. of Gen. Studies, Dept. of Acctg. and Pub. Fin. P.O. Box 4, Canberra, A.C.T., Australia
HEADINGTON, Robert C., Arizona State Univ., Tempe, Ariz.
HEADLEY, Oliver M., Univ. of Bridgeport, Col. of Bus. Admin., Bridgeport, Conn. 06602
HEADY, Earl O., Iowa State Col., Dept. of Econ. and Soc., Ames, Iowa
HEALEY, Derek T., Univ. of Adelaide, Dept. of Econ., Box 498-D, G. P. O., Adelaide, South Australia
HEALEY, Philip B., Waterloo Univ. Col., Waterloo, Ont., Canada
HEALY, Chester F., Jr., Sacramento State Col., Div. of Bus. Admin., 6000 Jay St., Sacramento 19, Calif.
HEALY, Francis X., Jr., 428 Haywood Rd., Ambler, Pa. 19002
HEALY, James A., D'Youville Col., Buffalo 1, N.Y.
HEALY, John J., 30-12 44th St., Long Island City, N.Y. 11103
HEALY, Kent T., Chestnut Hill Rd., R. 3, Killingworth, Conn. 06417
HEALY, Robert G., 6736 Oglesby Ave., Chicago, Ill. 60649
HEASLIP, Allan M., Petersburg Rd., Hackettstown, N.J. 07840
HEASTON, William R., Creighton Univ., Dept. of Bus. Admin., Omaha 31, Neb.
HEATH, John B., Bd. of Trade-Gen. Div., 1, Victoria St., London, S.W. 1, England
HEATH, Loyd C., Univ. of Washington, Dept. of Actg., Col. of Bus. Admin., Seattle 5, Wash.
HEATH, Milton S., 208 Glenburnie St., Chapel Hill, N.C.
HEATHERINGTON, Donald F., 45 E. End Ave., Apt. 6-H, New York 28, N.Y.
HEAVNER, Robert O., 3068A Concord St., Hill AFB, Utah
HECK, Harold J., Georgetown Univ., Sch. of Foreign Ser., Washington, D.C. 20007
HECK, Victor C., Mercer Univ., Macon, Ga.
HECKMAN, Harry W., 3837 Grand Ave., Western Springs, Ill. 60558
HECKMANN, Irvin L., Jr., Creighton Univ., Col. of Bus. Admin., Omaha, Neb. 68131
HECOX, William H., 308 Westwood Plaza, Box 326, Los Angeles, Calif. 90024
HEDAYATI, Seyed A. A., Bank Markazi Iran, Econ. Res. Dept., Tehran, Iran
HEDDEN, (Mrs.) Elizabeth, 7 Mullens Lane, Bernardsville, N.J. 07924
HEDGES, Charles A., Sacramento State Col., Econ. Dept., Sacramento, Calif. 95819
HEDGES, Irwin R., 3648 N. Oakland St., Arlington, Va. 22207
HEDGES, J. Edward, Indiana Univ., 359 Psych. Bldg., Bloomington, Ind. 47405
HEDGES, (Mrs.) Janice N., 3648 N. Oakland St., Arlington, Va. 22207
HEDGES, Robert A., Univ. of Illinois, 323 David Kinley Hall, Urbana, Ill. 61801
HEDLEY, Douglas D., Michigan State Univ., Room 41, Agric. Hall, East Lansing, Mich. 48823
HEDRICK, Charles B., Proctor and Gamble Co., Ind. Rela. Dept., I.T.C. Bldg., Ivorydale, Ohio 43436
HEDRICK, James L., Op. Res., Inc., 1400 Spring St., Silver Spring, Md.
HEDTKAMP, Gunter, 6312 Laubach Krs., Beethovenstr. 16, Giessen, Germany
HEDTKE, Kenneth A., 5454 Clinton Ave. S., Minneapolis, Minn. 55419
HEE, Olman, 7918 Legation Rd., Hyattsville, Md.
HEEBNER, A. Gilbert, 2360 Boyd Rd., Huntingdon Valley, Pa. 19006
HEER, Clarence, P.O. Box 627, Chapel Hill, N.C. 27514
HEEREM, Douglas L., 205 Riverside Park, Iowa City, Iowa
HEERTJE, Arnold, Cort van der Lindenlaan 39, Naarden, The Netherlands
HEFFERNAN, Henry G., Boston Col., Chestnut Hill, Mass. 02167
HEFFERNAN, John T., Nichols Col., Dudley, Mass. 01343
HEFFERNAN, Neil T., Jr., 35 B New St., Worcester, Mass.
HEFFNER, Herbert F., 45 Linden Ter., Leonia, N.J. 07737
HEFLEBOWER, Richard B., Northwestern Univ., Dept. of Econ., Evanston, Ill.
HEIBY, Ernest P., 5311 Neville Ct., Bush Hill Woods, Alexandria, Va. 22310
HEIDBREDER, (Mrs.) Eliz. M., 9705 Cockran Lane, Bethesda, Md. 20034
HEIDELL, James M., 14 Fenimore Rd., Scarsdale, N.Y.
HEIEN, Dale M., 707 22nd St., N.W., Washington, D.C.
HEIFNER, Richard G., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich.
HEIL, John J., 2408 Hayden Dr., Silver Spring, Md.
HEILBRONER, Robert L., 1075 Park Ave., New York, N.Y. 10028
HEILBRUN, James, 151 Central Park West, Apt. 2-N, New York, N.Y. 10023
HEILBRUNN, Martin, 77 E. 12th St., New York, N.Y. 10003
HEILPERIN, Michael A., 11, rue Michel Chauvet, Geneva, Switzerland
HEIM, Peggy, 2601 Woodley Pl., N.W., Washington 8, D.C.
HEIMANN, Eduard, Wirtschafts und Socialwissenschaftl, Facultaet Univ., Hamburg, Germany
HEIMBACH, Ernest E., 55 W. 11th St., New York 11, N.Y.
HEIMER, Roger C., 6050 Overbrook Ave., Philadelphia, Pa. 19131
HEIMLICH, Norman, 2322 Ekers Ave., Montreal 26, Que., Canada
HEIN, John, 33 Liberty St., Room 916, New York 45, N.Y.
HEINBERG, John D., 833 Midland, Apt. 6, Madison, Wis. 53715
HEINE, Irwin M., 4000 Massachusetts Ave. N.W., Washington 16, D.C.
HEINEKE, John, 1006 33rd Ave., Rock Island, Ill.
HEINEMANN, Hans, First Manhattan Co., 30 Wall St., New York, N.Y. 10005
HEINLEIN, Albert C., 127 Linden Ave., Kent, Ohio
HEINS, A. James, Univ. of Illinois, Dept. of Fin., Urbana, Ill.
HEISE, Paul A., 2223 N. Quantico St., Arlington, Va. 22205
HEISLER, (Mrs.) Ruth, 301 E. 21st St., New York, N.Y.
HEITMAN, Charles A., 2030 N. Adams St., Arlington, Va. 22201
HEITMANN, George, c/o Fred Heitmann, South Kartright, N.Y. 13842
HEKMAN, Louis H., 1400 Dunham, S.E., Grand Rapids, Mich. 49506
HELBING, Albert T., Montana State Univ., Missoula, Mont. 59801
HELD, R. Burnell, 3913 Montrose Driveway, Chevy Chase, Md. 20015
HELDMAN, Herbert H., 201 Park Ave. S., Room 607, New York 3, N.Y.
HELFGOTT, Roy B., 135 W. 56th St., New York 19, N.Y.
HELFINSTINE, Rex D., South Dakota State Univ., Econ. Dept., Brookings, S.D. 57007
HELFRICH, Ralph W., Jr., 713 W. Salem, Indianola, Iowa 50125
HELIKER, George B., Montana State Univ., Dept. of Econ., Missoula, Mont. 59801
HELLBORN, Ludwig S., Box 26, Tripoli, Libya
HELLEINER, Gerald Karl, Univ. of Toronto, Dept. of Polit. Econ., 100 St. George St., Toronto 5, Ont., Canada
HELLER, Heinz R., Univ. of California, Dept. of Econ., Los Angeles, Calif. 90024
HELLER, Keith R., Box 4056, University Sta., Minneapolis 14, Minn.
HELLER, (Mrs.) Shirley P., Finstat, Inc., 50 Broad St., New York, N.Y. 10004
HELLER, Walter P., Inst. of Mathematics Studies in Soc. Sci., Serra House, Stanford, Calif. 94305
HELLER, Walter W., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
HELLIE, James, 753 W. Main, Whitewater, Wis. 53190
HELLIWELL, John F., Nuffield Col., Oxford, England

HELLMAN, Richard, 212 Waterway Dr., Falls Church, Va.
HELLMUTH, William F., Jr., 144 Shipherd Circle, Oberlin, Ohio 44074
HELLWEG, Douglas R., 9212 Medicine Lake Rd., Minneapolis, Minn. 55427
HELM, Florence, 1725 New Hampshire Ave., N.W., Washington, D.C. 20009
HELMAN, Sanford B., 212 W. 14th St., New York, N.Y. 10003
HELMAN, Vincent M., 4115 S.W. Holden St., Seattle 18, Wash.
HELMBERGER, John D., 6914 Park Ave., Minneapolis 23, Minn.
HELMBERGER, Peter, Dept. of Agric. Econ., Univ. of Wisconsin, Madison 6, Wis.
HELMERS, Glenn, Univ. of Nebraska, 304 Dairy Ind. Bldg., Lincoln, Neb. 68503
HELMREICH, Theodore C., Purdue Univ., Econ. Dept., West Lafayette, Ind. 47178
HELMS, Lloyd A., Bowling Green State Univ., Bowling Green, Ohio 43402
HELMS, Robert B., 1613 Barry Ave., Apt. 9, Los Angeles, Calif. 90025
HELPPIE, Charles E., 802 Courtland St., Ypsilanti, Mich. 48179
HELSING, Paul L., Eastern Washington State Col., Dept. of Econ., Cheney, Wash.
HELZNER, Manuel L., 1817 Franwall Ave., Silver Spring, Md.
HEMMER, Carl J., Columbia Univ., 723 John Jay, New York, N.Y. 10027
HEMMI, Kenzo, Univ. of Tokyo, Dept. of Agric. Econ., Tokyo, Japan
HEMMINGSON, A. Robert, Augsburg Col., Minneapolis 4, Minn.
HEMOND, Robert L., 30 Gladwin Dr., Westfield, Mass. 01085
HEMPHILL, William L., Grad. Col., Princeton, N.J. 08540
HENANN, Harvey, 1170 N.E. 202nd St., N. Miami Beach, Fla. 32462
HENARES, Hilarion Jr., Nat. Econ. Coun., Padre Faura St., Manila, Philippines
HENDERSON, Butler T., 876 Fair St. S.W., Atlanta 14, Ga.
HENDERSON, Charles R., Jr., 2410 Park Pl., Evanston, Ill. 60201
HENDERSON, Dennis R., 1709 E. Beverly Rd., Shorewood 11, Wis.
HENDERSON, H. A., U.S.A.I.D., APO New York, N.Y. 09687
HENDERSON, James M., 2539 Thomas Ave. S., Minneapolis 5, Minn.
HENDERSON, James P., 214 W. 4th St., Pecatonica, Ill. 61063
HENDERSON, John B., 614 Highland Ave., Meadville, Pa. 16335
HENDERSON, John P., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
HENDERSON, Malcolm R., Augusta Col., Dept. of Bus. Admin., Augusta, Ga. 30904
HENDERSON, Richard, Jr., Xerox Corp., Box 1540, Rochester, N.Y. 14603
HENDERSON, Robert V., 517 Earnshaw Dr., Dayton 29, Ohio
HENDERSON, (Mrs.) Sandra R., 208 Grant Ave., Morgantown, W.Va.
HENDERSON, Vivian W., 1209 Fountain Dr., S.W., Atlanta, Ga. 30314
HENDERSON, W. L., Denison Univ., Dept. of Econ., Granville, Ohio 43023
HENDLER, Reuven, 5625 Hempstead Rd., Pittsburgh 17, Pa.
HENDLEY, William R., 8 Hillvale Circle, Knoxville, Tenn. 37919
HENDON, W. S., Texas Christian Univ., Sch. of Bus., Ft. Worth, Tex. 76129
HENDRICKS, H. G., 130 Stanford Dr., San Antonio 12, Tex.
HENDRICKS, J. P., 9 Van Corlear Pl., New York 63, N.Y.
HENDRICKSON, Robert M., 1285 Ave. of the Americas, New York 19, N.Y.
HENDRIKSEN, Eldon S., Washington State Univ., Sch. of Bus. Econ., Pullman, Wash. 99163
HENDRIX, William Elbert, USAID/Agr., APO New York 09675
HENDRY, James B., Int. Bank for Recon. and Dev., 1818 H St., N.W., Washington, D.C. 20433
HENEMAN, Herbert G., 1733 Blair St., St. Paul 4, Minn.
HENGREN, Raymond E., 11920 Glen Mill Rd., Rockville, Md.
HENIG, Harry, 7337 South Shore Dr., Chicago, Ill. 60649
HENLE, Peter, 3210 N. Wakefield St., Arlington 7, Va.
HENNEBERRY, Barbara M., Ball State Univ., Muncie, Ind. 47306
HENNESSY, J. M., FNCB, Casilla 260, La Paz, Bolivia
HENNESSEY, Joseph M., 4 Squonto Rd., Winchester, Mass. 01890
HENNING, Charles N., Univ. of Washington, Col. of Bus. Admin., Seattle 5, Wash.
HENNING, John A., Syracuse Univ., Dept. of Econ., Syracuse, N.Y.
HENRIKSSON, Rolf, Ornstigen 7, Nasby Park, Sweden
HENRY, G. P., 10914 New Hampshire Ave., Silver Spring, Md. 20903
HENRY, George B., 309 Mansfield St., Apt. E., New Haven, Conn.
HENRY, Harold H. G., Lincoln Univ., Oxford, Pa. 19352
HENRY, Jeanne P., 458 Ferne Ave., Palo Alto, Calif. 94306
HENRY, Louis H., 630½ Harrison Ave., South Bend, Ind. 46616
HENRY, William R., North Carolina State Col., Sch. of Agric., Raleigh, N.C.
HENSLEY, Roy J., Lehigh Univ., Dept. of Econ., Bethlehem, Pa.
HEPPEN, Michael J., Univ. of Notre Dame, Notre Dame, Ind. 46556
HERBER, Bernard P., Univ. of Arizona, Dept. of Econ., Tucson, Ariz.
HERBSLEB, James R., Monmouth Col., Monmouth, Ill. 61462
HERDER, Richard J., Federal Reserve Bank, Minneapolis, Minn.
HERDT, Robert W., Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
HERENDEEN, James B., 324A Boucke Bldg., University Park, Pa. 17774
HERENSTEIN, Ira, Standard Statis. Co., 345 Hudson St., New York 14, N.Y.
HERFINDAHL, Orris C., 1755 Massachusetts Ave. N.W., Washington, D.C.
HERKOMMER, Siegfried, 311, Rio Nazas, Col. Mexico, Monterrey, N.L., Mexico
HERLIHY, H. M., 7347 Sheridan Rd., Chicago, Ill. 60626
HERMAN, Charles L., 2711 Grant St., Apt. 3, Houston 6, Tex.
HERMAN, Edward E., State Univ. of New York, Plattsburgh, N.Y. 12203
HERMAN, Leon M., 10027 Brunett Ave., Silver Spring, Md. 20901
HERMAN, Robert, 1280 E. 18th St., Brooklyn, N.Y. 11230
HERMAN, Robert B., 1915 Kalorama Rd., N.W., Washington, D.C. 20009
HERMAN, Robert S., 9 Southwood Dr., Slingerlands, N.Y. 12159
HERMAN, Sidney, Northeastern Univ., Dept. of Econ., Boston, Mass.
HERMANSEN, Svend O., New Mexico State Univ., Dept. of Econ. Box 1060, University Park, N.M. 88070
HERNANDEZ, Nicolas, Dept. of Com., P.O. Box 9177, Santurce, P.R.
HERNANDEZ, Roberto E., 700 S.W. 11th Ave., Apt. 2, Miami, Fla. 33130
HERRICK, Bruce, Univ. of California, Dept. of Econ., Los Angeles, Calif. 90024
HERRICK, Theral T., 2440 Buckingham Rd., Ann Arbor, Mich.
HERRICK, Tracy G., 13209 Station Rd., Brecksville, Ohio 44141
HERRMANN, Robert O., Pennsylvania State Univ., Dept. of Agric. Econ. and Rural Soc., University Park, Pa. 16802
HERRMANN, Roy, 3 Pooks Hill Rd., Washington, D.C. 20014
HERRNSTADT, Irwin L., Northeastern Univ., Dept. of Econ., Boston, Mass. 02115
HERSCH, Arthur J., 2785 Dale Ave., Columbus 9, Ohio
HERSCHEL, Federico J., Pueyrredon 923, 12 Fl., Dtp.C, Buenos Aires, Argentina
HERSEY, Arthur B., Div. of Int. Finance, Bd. of Gov. of the Fed. Res. System, Washington, D.C. 20551

HERSH, Mona S., Texas Woman's Univ., Denton, Tex.
HERSHFIELD, David C., 165 Park Row, New York, N.Y. 10038
HERSKOWITZ, David S., 4314 Rowalt Dr., College Park, Md. 20740
HERSON, Richard J. L., 2 Park Ave., New York 16, N.Y.
HERTZOG, Walter S., Jr., 5357 Brooklyn Ave., Los Angeles 22, Calif.
HERVE, Michel E. A., Brookings Inst., 1775 Massachusetts Ave., N.W., Washington, D.C. 20036
HERVEY, Jack L., Dept. of Agric. Econ., Michigan State Univ., East Lansing, Mich. 48823
HERZ, Henry, 6926 Wilson Lane, Bethesda, Md. 21811
HERZEL, Leo, 231 S. LaSalle St., Room 1955, Chicago, Ill. 60604
HERZL, Theodore, 2869 Bainbridge Ave., Bronx 58, N.Y.
HERZOG, Allen N., 2564 E. 72nd Pl., Chicago, Ill. 60649
HERZOG, John P., Claremont Grad. Sch., Claremont, Calif. 91715
HERZOG, John R., 1142 Nielsen Ct., Ann Arbor, Mich. 48105
HESELTON, Frank R., Jr., 3426 Tulane Dr., Apt. 34, West Hyattsville, Md. 20783
HESIK, Richard J., 524 N. Mills Ave., Claremont, Calif. 91712
HESS, Arleigh P., Jr., Univ. of Pennsylvania, 15 College Hall, Philadelphia, Pa. 19104
HESS, Richard B., R.D. #2, Mechanicsburg, Pa. 10755
HESSER, Leon F., USAID, APO New York 09271
HESSION, Charles H., 1122 E. 31st St., Brooklyn 10, N.Y.
HESTER, Donald D., Yale Univ., Dept. of Econ., Box 2125, Yale Sta., New Haven, Conn.
HESTER, James L., Louisiana Poly. Inst., Sch. of Bus. Admin., Div. of Bus. and Econ. Res., Ruston, La. 71271
HESTON, Alan, Univ. of Pennsylvania, Wharton Sch., Dept. of Econ., Philadelphia 4, Pa.
HETTENA, Ran, 2601 Henry Hudson Pkwy. W., Riverdale, N.Y. 10463
HETTICH, Walter, 129 Livingston St., New Haven, Conn. 06511
HEUNG-KEUN, Oh, Republic of Korea Gov., Econ. Plan. Bd., Bur. of Statis., Seoul, Korea
HEUSER, Henry K., Dept. of State, Khartoum-USAID, Washington, D.C. 20521
HEUSON, William G., Box 8094, Univ. of Miami, Coral Gables, Fla. 33926
HEUSS, Ernst T. V., Burgschmietstrasse 7, 85 Nürnberg, Germany
HEUSTON, Milton C., 17955 Keswick St., Reseda, Calif.
HEWES, Amy, 28 Silver St., South Hadley, Mass. 01075
HEWES, C. Alexander, Jr., 1610 19th St., N.W., Washington 9, D.C.
HEWETSON, Henry W., Univ. of Virginia, Mary Washington Col., Box 1128, Fredericksburg, Va. 22402
HEXNER, Ervin, P.O. Box 875, State College, Pa. 16801
HEXTER, J. Lawrence, Univ. of Maryland, Dept. of Econ., College Park, Md. 20740
HEY, Sally M., Wheaton Col., Dept. of Econ., Norton, Mass. 02766
HEYDINGER, Edward R., Marathon Oil Co., Econ. and Statis. Div., 539 S. Main St., Findlay, Ohio 45840
HEYE, Charles F., Univ. of Hawaii, Col. of Bus. Admin., Hawaii Hall, 2500 Campus Rd., Honolulu, Hawaii 96822
HEYMANN, Hans, Jr., 6407 W. Halbert Rd., Bethesda, Md. 20034
HEYN, Udo E. G., 507 Campus View Apts., Bloomington, Ind. 47401
HEYNE, Paul Theodore, Southern Methodist Univ., Econ. Dept., Dallas, Tex. 75222
HIBBARD, Thomas H., 610 E. 6th St., Claremont, Calif.
HIBDON, James E., Faculty Exchange, Box 166, Univ. of Oklahoma, Norman, Okla.
HICKERSON, Glenn L., 1633 Bedford Lane, Newport Beach, Calif.
HICKMAN, Bert G., The Brookings Inst., 1775 Massachusetts Ave., N.W., Washington, 36, D.C.
HICKMAN, C. Addison, 702 W. Sycamore, Carbondale, Ill.
HICKMAN, W. Braddock, 3440 Avalon Rd., Shaker Heights, Ohio 43947
HICKMAN, William H., 4201 Loazell Ct., Sacramento 25, Calif.
HICKS, Clifford M., Univ. of Nebraska, Soc. Sci. Bldg., Lincoln, Neb.
HICKS, George L., USAID/PO/NPA, APO San Francisco 96528
HICKS, J. R., All Souls Col., Oxford, England
HICKS, Norman L., 34 Park Ave., Hicksville, N.Y.
HICKS, Robert E., R. 2 Elder Rd., Canal Winchester, Ohio 43110
HICKS, W. Whitney, Univ. of Missouri, Dept. of Econ., Columbia, Mo. 65202
HICKS, William T., Box 336, University, Miss. 38677
HIDALGO, Carlos F., Jr., Inter-Amer. Univ., Aguadilla, P.R. 00603
HIDY, (Mrs.) Muriel E., 108 Radcliffe Rd., Belmont, Mass. 02178
HIDY, Ralph W., 108 Radcliffe Rd., Belmont, Mass. 02178
HIEP, Pham Doan, Bur. of Budget and For. Aid, 142, Hong Thap Tu, Saigon, Vietnam
HIERONYMI, Otto, 37 Av. Ste. Cecile, Meyrin, Geneva, Switzerland
HIERONYMUS, William H., 912 Mary, Ann Arbor, Mich. 48104
HIESTAND, Dale L., Columbia Univ., 512 Uris, New York, N.Y. 10027
HIGA, Seiko, c/o Mr. Masaharu Tako, 10-1 Kodanjutaku, Shimoda-cho, Kohoku-ku, Yokohama, Japan
HIGGINS, Benjamin H., Univ. of Texas, Dept. of Econ., Austin 12, Tex.
HIGGINS, (Mrs.) Ilse S., 4021 Davis Pl., N.W., Washington 7, D.C.
HIGH, Hugh, P.O. Box 341, Lumberton, Miss. 39455
HIGHLAND, Ingeborg, 1311 9 Ave. North, Ft. Dodge, Iowa
HIGHTOWER, James K., Univ. Col., 601 N. Lombardy St., Richmond, Va. 23220
HIGIRO, Festo S. G., Columbus Ave., Apt. 10, Sepulveda, Calif. 90007
HIJIKATA, S., 6. Sanbancho, Chiyoda-Ku, Tokyo, Japan
HILASKI, Harvey J., 5050 Silver Hill Ct., Apt. No. 2, Suitland, Md. 21668
HILDEBRAND, George H., Cornell Univ., Dept. of Econ. 252 Goldwin Smith, Ithaca, N.Y. 14850
HILDEBRAND, John H., Jr., 1205 Sunset Dr., Columbia, Mo. 65201
HILDEBRAND, John R., Texas Tech. Col., Sch. of Bus., Dept. of Econ., Lubbock, Tex.
HILDEBRANDT, Elmer J., 5587 Haverhill Rd., Birmingham, Mich. 48010
HILDENBRAND, Howard J., 24720 Ross Dr., Detroit 39, Mich.
HILDES, Emanuel, 163 Elm Dr. E., Levittown, L.I., N.Y. 11756
HILDRED, William M., 1217-A Brackenridge Apts., Lake Austin Blvd., Austin, Tex. 78703
HILDRETH, Clifford G., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
HILEMAN, A. Eugene, Maryville Col., Dept. of Econ., Maryville, Tenn.
HILGART, Arthur A., Jr., The Upjohn Co., Kalamazoo, Mich. 49001
HILGERT, Ronald J., 1616 18th St. N.W., Apt. 1011, Washington 9, D.C.
HILL, Charles R., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
HILL, Daniel A., 1633 Compton Rd., Cleveland, Ohio 44118
HILL, Donald W., Rollins Col., Winter Park, Fla. 32789
HILL, Edmund R., Gettysburg Col., Dept. of Econ., Gettysburg, Pa.
HILL, Forest G., Univ. of Texas, Dept. of Econ., Austin, Tex. 78712
HILL, John S., 524-B Ashdown House, 305 Memorial Dr., Cambridge, Mass.
HILL, Lewis E., 414 Pendleton Rd., Clemson, S.C.
HILL, Peter W., 71 College, Mansfield, Pa. 16933

HILL, Richard A., 857 N. Xenophon, Tulsa 27, Okla.
HILL, Richard M., 1106 McHenry, Urbana, Ill. 61801
HILL, Robert A., 2935 Ashby Ave., Berkeley, Calif. 94705
HILL, Robert E., 900 Briarwood Dr., Carbondale, Ill. 62901
HILL, Robert L., 26-C Princeton Circle West, Apt. 54, Lynchburg, Va. 24503
HILL, Robert S., 5420 Norwich Ave., Van Nuys, Calif.
HILL, Samuel E., RFD 1, West Redding, Conn. 06896
HILL, Samuel T., P.O. Box 2211, East Carolina Col. Sta., Greenville, N.C. 27835
HILL, Walter A., Syracuse Univ., Col. of Bus. Admin., Syracuse, N.Y. 13210
HILLABOLD, Arthur B., Jr., Idaho State Col., Econ. Dept., Pocatello, Idaho
HILLHOUSE, Albert M., Cornell Univ., Sch. of Bus. Admin., Ithaca, N.Y.
HILLING, Helen C., New York Univ., Grad. Sch. of Pub. Admin., New York, N.Y. 10003
HILLINGER, Claude M., Case Inst. of Tech., Div. Organizational Sci., Cleveland, Ohio 44106
HILLMAN, Jimmye S., Univ. of Arizona, Dept. of Agric. Econ., Tucson, Ariz.
HILLS, Charles E., Conn. Dev. Comm., State Office Bldg., Hartford, Conn.
HILMY, Joseph, 12, Pinewood Rd., Aberdeen, Scotland
HILTON, George W., Univ. of California, Dept. of Econ., Los Angeles 24, Calif.
HILTON, Howard, Jr., 5022 Reno Rd., Washington 8, D.C.
HILTON, James G., Dept. of Econ., Univ. of South Carolina, Columbia, S.C.
HILTON, (Mrs.) Mary N., 5022 Reno Rd., Washington 8, D.C.
HILTY, Donald P., Marathon Oil Co., Findlay, Ohio
HILVERS, Earl J., 5545 Lucenna Dr., Cincinnati, Ohio 45238
HIMAN, Hugh K., Wake Forest Col., Sch. of Bus. Admin., Winston-Salem, N.C.
HIMELFARB, Morris, Port of N.Y. Auth., Room 1558, 111 8th Ave., New York, N.Y. 10011
HIMMELSTEIN, Seymour, 430 West 205th St., New York 34, N.Y.
HINCHEY, Brackston, Lowell Tech. Inst., Dept. of Econ., Lowell, Mass.
HINCKLEY, Robert, Cornell Univ., 59-J Hasbrouck Apt., Ithaca, N.Y.
HINDELANG, Thomas J., 14302 Rosemary Ave., Detroit, Mich. 48213
HINDERLITER, Roger H., 6223 Southwood, Apt. E., St. Louis, Mo.
HINDERSMAN, Charles, 304 Eason Dr., Carbondale, Ill. 62901
HINES, Howard H., Nat. Sci. Found., Div. of Soc. Sci., Washington, D.C. 20550
HINES, L. Gregory, Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
HING, Sokhom, 320 E. 23rd St., Apt. 6-P, New York, N.Y. 10010
HINOMOTO, Hirohide, Univ. of Illinois, Dept. of Ind. Admin., 350 Comm (W), Urbana, Ill. 61803
HINRICHS, Harley H., 9595 Persimmon Tree Rd., Potomac, Md. 20677
HINSHAW, Randall W., John Hopkins Bologna Center, Via Belmeloro, 11, Bologna, Italy
HINSON, J. Philip, 53-I Hasbrouck Apts., Ithaca, N.Y. 14850
HIPPERT, Charles H., 2928 Mountview Rd., Columbus, Ohio 43221
HIPPLE, Frederick S., Southern Methodist Univ., Dept. of Econ., Dallas, Tex. 75222
HIRSCH, Abraham, 95 Eastern Pkwy., Brooklyn 38, N.Y.
HIRSCH, Claus W., 27 Orchard Lane, Berkeley Heights, N.J. 07922
HIRSCH, (Mrs.) Edith, 57 E. 88th St., New York 28, N.Y.
HIRSCH, (Mrs.) Eva B., 95 Eastern Pkwy., Brooklyn 38, N.Y.
HIRSCH, Hans G., 6513 Kenhowe Dr., Bethesda, Md. 20034
HIRSCH, (Mrs.) Velma, 5801 Walnut St., Pittsburgh, Pa.
HIRSCH, Werner Z., Univ. of California, Inst. for Gov. and Pub. Affairs, 405 Hilgard Ave., Los Angeles 24, Calif.
HIRSCH, Yale, 451 Grand Ave., Palisades Park, N.J. 07650
HIRSCHBERG, David, 266 Congressional Lane, Rockville, Md. 20852
HIRSCHFELD, Alvin J., 99 Pratt St., Suite 519, Hartford, Conn. 06103
HIRSCHMAN, Albert O., 350 Central Park West, New York 25, N.Y.
HIRSCHTRITT, Ralph, 1712 Republic Rd., Silver Spring, Md.
HIRSEMAN, Paul W., 109 Cambridge Dr., Wilmington 3, Del.
HIRSHLEIFER, Jack, Univ. of California at Los Angeles, Dept. of Econ., Los Angeles 24, Calif.
HITCH, Charles J., 770 Hilldale Ave., Berkeley, Calif. 94708
HITCH, Thomas K., First Nat. Bank of Hawaii, P.O. Box 3200, Honolulu, Hawaii 96801
HITCHINGS, George P., American Airlines, Inc., 633 Third Ave., New York 17, N.Y.
HIXSON, Nathan H., Adjutant Gen. Sch., Ft. Benjamin Harrison, Indianapolis, Ind. 46216
HLIPALA, John, 81 Church St., Spring Valley, N.Y. 10977
HO, Alfred K., 2440 Moreno Dr., Los Angeles 39, Calif.
HO, Franklin Yen Hui, Calif. Poly. Col., Voorhis-Kellogg Camp, Pomona, Calif.
HO, Yhi-Min, Univ. of Houston, Dept. of Econ. and Fin., Houston, Tex. 77004
HOADLEY, Walter E., Armstrong Cork Co., Lancaster, Pa.
HOAG, Howard L., 805 Hayes St., West Lafayette, Ind. 47178
HOAG, Malcolm W., The RAND Corp., 1700 Main St., Santa Monica, Calif.
HOAGLAND, Henry E., 2448 Southway Dr., Columbus, Ohio 43221
HOAN, Buu, c/o Int. Monetary Fund, Washington, D.C.
HOBART, George H., 603 W. Farris Ave., High Point, N.C.
HOBBAH, Reginald V., 6225 Tally Ho Lane, Alexandria, Va. 22307
HOBBIE, Brian R., 36425 Kilarney, Willoughby, Ohio
HOBBS, Fred G., 100 Spring Grove Ave., Dayton, Ohio 45409
HOBSON, Asher, c/o Little Norway, Blue Mounds, Wis.
HOBSON, Ivan L., 1119 Hamilton Dr., Richardson, Tex.
HOCEVAR, Joze, Apartado Este 4481, Caracas, Venezuela
HOCEVAR, Toussaint, Keuka Col., Keuka Park, N.Y. 14478
HOCH, Irving J., Univ. of California, Dept. of Agric. Econ., 207 Giannini Hall, Berkeley, Calif.
HOCHBERG, (Mrs.) Audrey G., 10 Channing Pl., Eastchester, N.Y. 10709
HOCHMAN, Harold M., R.D. #2, Flordon Dr. Charlottesville, Va.
HOCHWALD, Werner, 6910 Cornell Ave., St. Louis 30, Mo.
HOCTER, William J., Fed. Res. Bank, Chicago, Ill. 60690
HOCTOR, Thomas F., 7129 Fairfax Rd., Bethesda, Md. 21811
HODES, Daniel A., Gen. Telephone and Electronics Corp., 730 3rd Ave., New York 17, N.Y.
HODGES, Dorothy J., 312 N. Segoe Rd., Madison 5, Wis.
HODGES, John E., 4201 Cathedral Ave. N.W., Apt. 106W, Washington, D.C. 20016
HODGES, John R., Univ. of Missouri at Kansas City, Kansas City 10, Mo.
HODGES, Julian A., Kansas State Univ., Agric. Econ. Dept., Waters Hall, Manhattan, Kans. 66504
HODGESS-ROPER, John F., Faculty of Econ., Dover St., Manchester 13, England
HODGINS, Cyril D., #A-5316 S. Dorchester Ave., Chicago, Ill. 60615
HODGKINSON, William, Jr., 601 E. 20th St., Apt. 14-F, New York 10, N.Y.
HODGMAN, Donald R., Univ. of Illinois, Dept. of Econ., Urbana, Ill.

HODGSON, Jacqueline L., Univ. of the Americas, Mexico 10, D.F.
HODGSON, Richard C., 408 Hines St., Wilson, N.C. 27893
HODJERA, Zoran S., Int. Monetary Fund, Res. and Statis. Dept., 19th and H St., N.W., Washington, D.C. 20431
HOEFFDING, Oleg, c/o The RAND Corp., 1700 Main St., Santa Monica, Calif.
HOEGSTEDT, Ridgeway, Rutgers Univ., Dept. of Econ., New Brunswick, N.J.
HOEHN, Merrill W., 122 East 10th St., Hinsdale, Ill.
HOEKENDORF, William C., Montana State Univ., Dept. of Econ., Missoula, Mont. 59801
HOEMKE, Randall H., Roberta Park Apts., Apt. A-2, 3605 E. 39th St., Indianapolis, Ind. 46205
HOENACK, Stephen A., 1825 Francisco St., Berkeley, Calif. 94703
HOERNEMAN, Calvin A., Jr., 30 Midway Rd., Pittsburgh 16, Pa.
HOFER, Peter, Westminster Col., Salt Lake City, Utah 84105
HOFER, Ronald K., 607 W. 4th St., Bloomington, Ind.
HOFFENBERG, Marvin, Univ. of California-Los Angeles, Inst. of Gov. and Pub. Affairs, 11252 Soc. Sci. Bldg., Los Angeles, Calif. 90024
HOFFENBERG, Marvin J., 303 Continental Apts., 3415 West End Ave., Nashville, Tenn. 37203
HOFFHEINS, Francis M., 3517 Rodman St., N.W., Washington, D.C. 20008
HOFFLAND, David L., 7603 Seton Hall, La Mesa, Calif. 92041
HOFFMAN, Arnold, 120 Broadway, Room 2650, New York, N.Y. 10005
HOFFMAN, Burton, 2653 Wentworth Rd., Philadelphia, Pa. 19131
HOFFMAN, (Mrs.) Burton, 2653 Wentworth Rd., Philadelphia, Pa. 19131
HOFFMAN, Charles, State Univ. of New York at Stony Brook, Dept. of Econ., Stony Brook, L.I., N.Y. 11790
HOFFMAN, Clifford E., 1628 Telephone Bldg., 1010 Pine St., St. Louis 1, Mo.
HOFFMAN, Fred S., 6309 Dahlonega Rd., Washington 16, D.C.
HOFFMAN, H. Theodore, 18292 Hartwell, Detroit, Mich. 48235
HOFFMAN, Louis R., 3327 Orlando St., N.E., Knoxville, Tenn.
HOFFMAN, Marvin, John Carroll Univ., Sch. of Bus., University Hgts., Cleveland 18, Ohio
HOFFMAN, Michael L., Int. Bank for Recon. and Dev., Room 845, 1818 H Street, N.W., Washington, D.C. 20433
HOFFMAN, Morton, 2703 Rockwood Ave., Baltimore 15, Md.
HOFFMAN, Ronald F., Univ. of Hawaii, Dept. of Econ., 2540 Maile Way, Room 359, Honolulu, Hawaii 96822
HOFFMANN, Eivind, Almevn. 28, Oslo 8, Norway
HOFFMANN, H., Milbertshofener Strasse 7 B, Munich 13, Germany
HOFHEINZ, James F., 10023 Briar Rose, Houston 42, Tex.
HOFLICH, Harold J., Univ. of Arizona, Div. of Econ. and Bus. Res., Tucson, Ariz. 85721
HOFMAN, Cornelius A., Utah State Univ., Dept. of Econ., Logan, Utah 84321
HOFMANN, (Mrs.) Catherine P., 3723 Meridian N., Seattle 3, Wash.
HOFMANN, Robert, 617 Plantmore Dr., Ferguson, Mo. 63026
HOFMEISTER, R. H., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
HOFSTETTER, Charles A., 1236 Maple Ave., Evanston, Ill.
HOGAN, Edward B., USAID/Kampala, State Dept., Washington 25, D.C.
HOGAN, John A., Winding Creek Farm, Box H, Durham, N.H. 03824
HOGAN, John D., 720 E. Wisconsin Ave., Milwaukee, Wis. 53202
HOGAN, Thomas J., 8517 Parliament Dr., Springfield, Va. 22151
HOGAN, Thomas P., 24 Lawn Ave., Lowell, Mass.
HOGAN, Warren P., Newcastle Univ. Col., Dept. of Econ., Tighe's Hill, 2N., N.S.W., Australia
HOGAN, William T., Fordham Univ., Bronx, N.Y. 10458
HOGG, Vincent W., 3701 Jocelyn St., N.W., Washington, D.C.
HOGLE, Paul M., 2993 Hampshire Rd., Cleveland Heights, Ohio 44118
HOGLUND, C. Edward, 209 W. Jackson Blvd., Room 1201, Chicago, Ill. 60606
HOGUET, Peter W., 200 E. 66th St., New York 20, N.Y.
HOHENBERG, Paul M., Stanford Univ., Res. Center in Econ. Growth, 423 Encina Hall, Stanford, Calif. 96128
HOHENSTEIN, Jeffrey J., 82-36 262nd St., Floral Park, L.I., N.Y.
HOHL, Harvey E., Office of the Mayor, Div. of Econ. Dev., 212 City Hall, Milwaukee, Wis. 53202
HOHMAN, Elmo Paul, 625 Haven, Evanston, Ill.
HOHMAN, Richard A., 3018 Webster St., Apt. #3, Omaha, Neb. 68131
HOHNSTEIN, Willard, East Texas State Col., Dept. of Bus. Admin., Commerce, Tex.
HOISINGTON, Sumner J., Jr., 36 Bradfield Ave., Roslindale, Mass. 02131
HOKANSON, John R., 2022 Lakewood St., Suitland, Md. 20023
HOLBEN, Ralph E., 1 River Ridge Rd., Hanover, N.H. 03755
HOLBERT, Neil B., 185 Bronx River Rd., Apt. 6L, Yonkers, N.Y.
HOLBIK, Karel, Boston Univ., Dept. of Econ., 236 Bay State Rd., Boston 15, Mass.
HOLBROOK, Robert S., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
HOLDER, Frederick C., 832 University Pl., Grosse Pointe, Mich. 48138
HOLDREN, Bob R., Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa 50010
HOLDSWORTH, David G., 66 Montague St., Brooklyn, N.Y. 11201
HOLEN, (Mrs.) Arlene S., 6700 Belcrest Rd., Apt. 217, Hyattsville, Md.
HOLESOVSKY, Vaclav, Univ. of Massachusetts, Dept. of Econ., Amherst, Mass. 01003
HOLLAND, Daniel M., 41 Turning Mill Rd., Lexington 73, Mass. 01054
HOLLAND, Edward P., 29 Mageam St., Brunswick, Me. 04011
HOLLAND, Robert C., 5907 Ramsgate Rd., Washington 16, D.C.
HOLLAND, Robert T., 935 Euclid St., Apt. 10, Santa Monica, Calif. 90403
HOLLAND, Thomas E., Univ. of Alabama, Dept. of Econ., University, Ala. 35486
HOLLAND, Thomas W., Geo. Washington Univ., Econ. Dept., Washington 6, D.C.
HOLLAND, Wesley J., 6730 Euclid Ave., Chicago, Ill. 60649
HOLLANDER, Edward D., 1218 16th St., Washington 16, D.C.
HOLLANDER, Samuel, Univ. of Toronto, Dept. of Polit. Econ., 100 St. George St., Toronto 4, Ont., Canada
HOLLANDER, Stanley C., Michigan State Univ., Col. of Bus. and Pub. Serv., East Lansing, Mich. 48823
HOLLANDER, Susan J., 1641 Myamby Rd., Towson, Md. 21204
HOLLANDER, T. Edward, 2 Holland Lane, Monsey, N.Y. 10952
HOLLBACH, Arthur R., 195 Rodney Crescent, Ottawa 8, Ont., Canada
HOLLENHORST, Jerome J., 2032 Herbert St., Murphysboro, Ill. 62966
HOLLERMAN, Leon, Pitzer Hall, Claremont Men's Col., Claremont, Calif.
HOLLEY, Donald W., Northwestern State Col., Box 416, Natchitoches, La. 71457
HOLLEY, (Mrs.) Helen L., 5404 Blackstone Rd. N.W., Washington, D.C. 20016
HOLLIMAN, William M., 2738 E. 3rd St., Tulsa 4, Okla.
HOLLINGER, William C., Harvard Univ., Dev. Advisory Service, 6 Divinity Ave., Cambridge, Mass. 01922

HOLLIS, W. Slater, Memphis State Univ., Memphis, Tenn.
HOLLISTER, William W., 350 West End Ave., New York, N.Y. 10024
HOLLOWAY, Charles S., 2526 11th St. N.W., Washington 1, D.C.
HOLLY, George B., Keystone Junior Col., La Plume, Pa. 18440
HOLM, Donald S., Jr., 106 W. Ridgley Rd., Columbia, Mo.
HOLM, Edwin E., 2405 Beacon Lane, Richmond, Va. 23230
HOLMAN, Gary L., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
HOLMAN, Mary A., 940 Spring Hill Rd., McLean, Va. 22101
HOLMAN, Wayne J., Jr., 1039 Rahway Rd., Plainfield, N.J.
HOLMES, Alan R., Fed. Res. Bank of N.Y., New York 45, N.Y.
HOLMES, David M., 3157 Perlita Ave., Apt. 7, Los Angeles, Calif. 90039
HOLMES, Floyd C., 5909 Onondaga Rd., Washington, D.C. 20016
HOLMES, James M., Purdue Univ., Herman C. Krannert Grad. Sch. of Ind. Admin., Lafayette, Ind. 47907
HOLMES, Ramsey R., ZB8585, Canadian Sch. of Mil. Intelligence, Camp Borden, Ont., Canada
HOLMES, William L., 408 E. Stoughton, Apt. 6-A, Champaign, Ill. 61822
HOLSEN, John A., 444 Clark Ave., Kirkwood, Mo. 63122
HOLSTEIN, Edwin J., Rensselaer Poly. Inst., Dept. of Econ., Troy, N.Y.
HOLT, Charles C., 215 Forest St., Madison, Wis. 53705
HOLT, Richard B., American Tel. and Tel. Co., 195 Broadway, New York 7, N.Y.
HOLT, Robert C., Jr., 411 Granville Dr., Silver Spring, Md. 20901
HOLTMANN, Alphonse G., Jr., 13316 Talbot, Huntington Woods, Mich. 49748
HOLTON, Richard H., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
HOLTZ, James N., 622 Las Casas, Pacific Palisades, Calif. 90272
HOLUBNYCHY, Vsevolod, Box 62, Cathedral Sta., New York 25, N.Y.
HOLY, Vaclav L., 137-05 Franklin Ave., Flushing 55, N.Y.
HOLZMAN, Franklyn D., Tufts Univ., Dept. of Econ., Medford, Mass. 02155
HOMAN, A. Gerlof, P.O. Box 3415, Rincon Annex, San Francisco 20, Calif.
HOMAN, Paul T., 19 Abbey Rd., Brighton 7, Sussex, England
HOMME, Herbert T., 260 Amity St., Amherst, Mass. 01002
HON, Ralph C., Southwestern, Memphis 12, Tenn.
HONG, Byung Doo, 1545 18th St., N.W., Washington, D.C. 20036
HONG, Sunion, 11 Tyler Ave., Hempstead, N.Y. 11550
HONG, Wontack, 126 W. 96th St., Apt. 5-A, New York, N.Y. 10025
HONKALEHTO, Oswald, Colgate Univ., Dept. of Econ., Hamilton, N.Y. 13346
HONNOLD, Junia H., Apt. 4, Buckingham, 229 N. Piedmont St., Arlington, Va.
HOOD, William C., 601 Windermere Ave., Ottawa 13, Ont., Canada
HOOKER, John J., Catholic Univ., Box 108, Washington 17, D.C.
HOOKER, Raymond W., 2016 Sheridan, Laramie, Wyo.
HOOKER, Stephen V., Pennsylvania State Univ., Dept. of Chemistry, University Park, Pa. 16802
HOOKS, Janet M., 708 La Sell Dr., Champaign, Ill.
HOOLEY, (Mrs.) Aurelia S., 11-A Sta. Monica St., Pasay City, Philippines
HOOLEY, Richard W., 11-A Sta. Monica St., Pasay City, Philippines
HOOPER, John W., Univ. of California, Dept. of Econ., La Jolla, Calif. 92038
HOOS, Sidney, Univ. of California, Giannini Found., Berkeley, Calif.
HOOVER, Calvin B., Duke Univ., Econ. Dept., Durham, N.C.
HOOVER, Edgar M., 211 S. Dithridge St., Pittsburgh 13, Pa.
HOOVER, (Mrs.) Ethel D., 4609 Chase Ave., Bethesda, Md. 20014
HOPE, Franklin E., Min. of Dev. and Plan., Statis. Bur., Public Bldg., Georgetown, British Guiana
HOPE, Walter B., 1319 44th Pl., S.E., Washington, D.C. 20019
HOPKINS, Francis W., Indian Neck Point, Branford, Conn.
HOPKINS, Frank E., Univ. of Maryland, Econ. Dept., College Park, Md. 20742
HOPKINS, John A., The Westchester, Apt. 619B, 4000 Cathedral Ave. N.W., Washington 16, D.C.
HOPKINS, Brother Matthew, Archbishop Molloy High Sch., 83-53 Manton St., Jamaica 35, N.Y.
HOPKINS, Thomas D., 162 Bishop St., New Haven, Conn. 06511
HOPKINS, William S., Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
HOPPER, Jack, 1624 Marshall St., Houston, Tex.
HOPPER, Wilbert H., 100 Wilson Lane, Needham, Mass. 02192
HOPPMANN, Erich, Gottfried-Keller-Str. 19, 355 Harburg, Germany (Lahn)
HOPSON, Howard L., 138 E. 11th Ave., Columbus 1, Ohio
HOPUSCH, Edgar L., Univ. of Wyoming, Col. of Com. and Ind., Laramie, Wyo. 82070
HORAN, Martin B., 3415 Mendon Rd., Cumberland, R.I. 02864
HORDON, Harris E., 2533 E. 1st St., Brooklyn, N.Y. 11223
HORIKE, Bunkichiro, 71-15 Higashi-Ku, Kumitachi, Kitatama, Tokyo, Japan
HORIUCHI, Shinsuke, Northern Illinois Univ., Dept. of Econ., Dekalb, Ill.
HORN, Carl E., Amer. Tel. and Tel. Co., 195 Broadway, Rm. 540A, New York, N.Y. 10007
HORN, Richard L., 560 Lincoln Pl., Brooklyn, N.Y. 11238
HORNE, Gilbert R., 2730 Alexandra Ave., Windsor, Ont., Canada
HORNE, McDonald K., Jr., Nat. Cotton Coun., P.O. Box 9905, Memphis, Tenn.
HORNE, Roman L., Route 2, Box 370, McLean, Va. 22101
HORNE, William J., 140 Federal St., Room 839, Boston 7, Mass.
HORNER, Gunther P., 78-40 164th St., Flushing, N.Y. 11365
HORNER, Robert R., 838 N. Shore Dr., Holland, Mich.
HOROWITZ, (Mrs.) Ann R., Indiana Univ., Sch. of Bus., Box B, Bloomington, Ind.
HOROWITZ, C. Morris, 1577 E. 18th St., Brooklyn 30, N.Y.
HOROWITZ, Helen G., 2 McClellan Ave., Amsterdam, N.Y. 12010
HOROWITZ, Ira, Indiana Univ., Sch. of Bus., Box B, Bloomington, Ind.
HOROWITZ, Morris A., Northeastern Univ., Dept. of Econ., Boston 15, Mass.
HOROWITZ, Seymour M., 333 E. 70th St., New York, N.Y. 10021
HORRIGAN, James O., Univ. of Notre Dame, College of Bus. Admin., Notre Dame, Ind. 46556
HORSCH, Lucas J., 840 N. Euclid Ave., Upland, Calif. 91786
HORSFALL, Alexander B., Duck Hill Rd., Billerica, Mass. 01821
HORTON, Byrne J., 7641 N. Eastlake Ter., Chicago, Ill. 60626
HORTON, Donald C., Bur. of the Budget, Washington, D.C.
HORTON, Fenn C., 628 Spazier, Pacific Grove, Calif. 93950
HORTON, Joseph J., Jr., Southern Methodist Univ., Box 266, Dallas 22, Tex.
HORTON, Robert V., 308 Park Lane, West Lafayette, Ind. 47906
HORVATH, Janos, Whitman Col., Walla Walla, Wash. 99362
HORVATH, Joseph C., 7336 Canterbury Rd., Prairie Village, Kans. 66208
HORVATH, Peter, 11 Park Ave., Tarrytown, N.Y. 10591

HORVITZ, Paul M., 9907 Inglemere Dr., Bethesda, Md. 21811
HORWICH, George, Purdue Univ., Dept. of Econ., Lafayette, Ind.
HORWITZ, Bertrand N., Univ. of Rochester, Col. of Bus. Admin., River Campus Sta., Rochester, N.Y. 14627
HORWOOD, O. P. F., Univ. of Natal, Dept. of Econ., King George V Ave., Durban, S. Africa
HOSEK, William R., 724 Kroeger Walk, #106, Goleta, Calif. 93017
HOSELITZ, Bert F., Univ. of Chicago, Div. of Soc. Sci., Chicago, Ill. 60637
HOSS, Salim, Kuwait Fund for Arab Econ. Dev., P.O. Box 2921, Kuwait, Kuwait
HOSTETLER, L. Merle, Union Com. Bank, P.O. Box 5876, Cleveland 1, Ohio
HOTELLING, Harold, Box 628, Chapel Hill, N.C. 27514
HOTSON, John H., Box 117, Green Mountain Falls, Colo. 80819
HOTTENSTEIN, Carl R., 1 Crawford Ter., Riverside, Conn. 06878
HOTTENSTEIN, David, Jr., 5930 N. 9th St., Arlington, Va. 22205
HOTTENSTEIN, Marcus S., 1 W. 72nd St., Apt. 57, New York 23, N.Y.
HOU, Chi-Ming, 798 Wildcat Canyon Rd., Berkeley, Calif.
HOUCHINS, Joseph R., 5708 14th St., N.W., Washington 11, D.C.
HOUCK, James P., Univ. of Minnesota, Dept. of Agric. Econ., Haeker Hall, St. Paul, Minn. 55101
HOUCK, Lewis D., Jr., 440 E. 78th St., New York, N.Y. 10021
HOUGH, Robbin R., 118 Walnut Blvd., Rochester, Mich. 48063
HOUGHTELING, Louise D., 4745 Reservoir Rd., Washington, D.C. 20007
HOUGHTON, Leonard I., Paul Smith's Col., Paul Smiths, N.Y. 12970
HOUK, John T., Jr., 6709 Jo Allen Dr., Falls Church, Va.
HOULE, Joseph, 6614 Woodridge Dr., Woodridge, Ill., P.O. Downers Grove 60515
HOUSEWRIGHT, George M., Carlson Ter., F-106 Fayetteville, Ark. 72701
HOUSKA, (Mrs.) Mary D., Rt. 1, Box 505, Blacksburg, Va. 24060
HOUSLEY, Carl B., Florida State Univ., Box 6454, 2070 Holmes St., Tallahassee, Fla.
HOUSSIAUX, Jacques R., 40, Rue du General Hoche, Nancy, M. et M., France
HOUSTON, A. C., R.D. 5, Carlisle, Pa. 17013
HOUSTON, David B., Pennsylvania State Univ., 120 Boucke Bldg., University Park, Pa. 17774
HOUSTON, Neil T., Stanford Res. Inst., Menlo Park, Calif.
HOUTHAKKER, Hendrik S., Harvard Univ., Dept. of Econ., 209 Littauer Center, Cambridge, Mass. 02138
HOUWINK, Willem, Univ. of Nevada, Dept. of Econ., Reno, Nev.
HOVDA, Howard B., 3820 Faculty Dr., Lynchburg, Va. 24501
HOVDE, Howard T., 145 Barnegat Blvd., Beachwood, N.J. 08722
HOVERLAND, H. Arthur, U.S. Naval Postgrad. Sch., Monterey, Calif.
HOVERMALE, Ruth, Winthrop Col., Box 1486, Home Econ. Dept., Rock Hill, S.C. 29732
HOVEY, Harold A., 2628 S. Arlington Ridge Rd., Arlington, Va. 22202
HOWARD, Arthur L., 9906 Parkwood Dr., Bethesda, Md. 21811
HOWARD, Bion B., Sch. of Bus., Northwestern Univ., Evanston, Ill. 60201
HOWARD, Charles S., Jr., Route 1, Box 126, Luxora, Ark. 72358
HOWARD, David C., Delta Col., Econ. Dept., University Center, Mich. 48710
HOWARD, Sister Jane M., Marygrove Col., Detroit, Mich. 48221
HOWARD, Marshall C., Univ. of Massachusetts, Dept of Econ., Amherst, Mass.
HOWARD, Stanley E., 45 Lake Lane, Princeton, N.J. 08540
HOWARD, Vivian C., Fortune Magazine, Time Life Bldg., Rm. 1826, Rockefeller Center, New York, N.Y.10020
HOWARD, Willard W., Soc. Sci. Dept., Phoenix Col., 1202 W. Thomas Rd., Phoenix 13, Ariz.
HOWE, Charles W., Res. for the Future, Inc., 1775 Massachusetts Ave., N.W., Washington, D.C. 20036
HOWE, W. Gerry, 2044 Sherman Ave., Evanston, Ill. 60201
HOWELL, James E., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 96128
HOWELL, James M., USAID/Chile, c/o Dept. of State Mail Rm., Washington 25, D.C.
HOWELL, Philip L., Univ. of Tulsa, Bus. Admin., Tulsa 4, Okla.
HOWELL, Robert N., P.O. Box 3, State College, Miss. 39762
HOWENSTINE, E. Jay, Int. Labour Off., Geneva, Switzerland
HOWEY, Richard S., Univ. of Kansas, Dept. of Econ., Lawrence, Kans.
HOWLE, Edward S., Univ. of North Carolina, Dept. of Econ., Carroll Hall, Chapel Hill, N.C.
HOWREY, E. Philip, Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
HOYLE, Michael H., Univ. of Chicago, Dept. of Geophysical Sci., Cloud Physics Lab., Chicago, Ill. 60637
HOYT, Elizabeth, Iowa State Univ., Ames, Iowa
HOYT, George C., Univ. of Iowa, Dept. of Bus. Admin., Iowa City, Iowa
HOYT, Homer, 2939 Van Ness St., N.W., Washington, D.C. 20008
HRUBY, Richard J., 1626 Compton Rd., Cleveland Heights 18, Ohio
HSIA, Felix H., South Dakota State Univ., Econ. Dept., Brookings, S.D. 57007
HSIA, Ke Ting, Los Angeles State Col., Dept. of Econ., Los Angeles 32, Calif.
HSIA, Ronald, Univ. of Hong Kong, Dept. of Econ., Hong Kong
HSIAO, (Mrs.) Katherine H. Y., 2320 Putnam St., Terre Haute, Ind.
HSIAO, Liang Lin, 2320 Putnam St., Terre Haute, Ind.
HSIAO, Sheng-tieh, 715 E. Catherine, Ann Arbor, Mich. 48104
HSIEH, Ching-Yao, 5117 45th St., N.W., Washington, D.C. 20016
HSIEH, Richard K. C., 1729 Northern Pkwy., Baltimore, Md. 21212
HSU, Robert C., 2412 Ellsworth St., #9, Berkeley, Calif.
HU, Chong Chul, 4467 Castleman, St. Louis, Mo. 63110
HU, Teh-wei, 101-A Eagle Hgts. Apts., Madison, Wis. 53705
HUAN, Bui T., Rector Hue Univ., 3, Le Loi, Hue, Vietnam
HUANG, Ching-ju, Univ. of North Carolina, Dept. of Econ., Carroll Hall, Box 98, Chapel Hill, N.C. 27515
HUANG, David S., Southern Methodist Univ., Dallas, Tex. 75222
HUANG, K. Loo, Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison, Wis. 53706
HUANG, Stanley Shang-Chien, 187-36 87th Rd., Janacia Estates, N.Y. 11432
HUBBARD, Ivan R., 57 Laurel St., Trumbull, Conn. 06611
HUBBARD, James J., 215-02 94th Rd., Queens Village, N.Y. 12141
HUBBARD, Joshua C., Bryn Mawr Col. Library, Bryn Mawr, Pa. 19010
HUBER, John Henry, 6655 Chilton Ct., McLean, Va. 22101
HUBER, Paul B., Dalhousie Univ., Dept. of Econ. and Soc., Halifax, N.S., Canada
HUBERT, Giles A., Dillard Univ., New Orleans 22, La.
HUDDLE, Donald L., Rice Univ., Dept of Econ. and Bus. Admin., Houston, Tex. 77001
HUDSON, David V., Jr., 4411 Waynesboro Dr., Houston 35, Tex.
HUDSON, Harriet D., 232 Norfolk Ave., Lynchburg, Va.
HUDSON, Hersel W., 215 Overlook Dr., Kent, Ohio 44240
HUDSON, Jesse T., Jr., 362 Valley Green Dr., N.E., Atlanta, Ga. 30305
HUDSON, John F., 1035 Welsh Dr., Rockville, Md. 20852

HUDSON, Lawrence R., St. Lawrence Univ., Dept. of Econ., Canton, N.Y. 13617
HUDSON, Milton W., 99 Strathmore Lane, Rockville Centre, N.Y.
HUDSON, Philip G., 1133 E. Helen St., Tucson, Ariz.
HUDSON, William J., 11214 Bybee St., Silver Spring, Md.
HUE, Nguyen Bich, Dir. Res. Service, Nat. Bank of Vietnam, 17, Ben Chuong Duong, Saigon, Vietnam
HUE, Phan Thi, 367 Phu Dinh (Phu Lam) Saigon, Vietnam
HUEFNER, Ronald J., 227 Bryant Ave., Ithaca, N.Y. 14850
HUEGY, Charles W., 4608 Lage Dr., San Jose, Calif.
HUEGY, Harvey W., 1312 S. Broadway, Urbana, Ill. 61801
HUFF, David L., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles, Calif.
HUFF, Lee W., 1400 S. Joyce, Apt. C1109 Arlington, Va.
HUFFMAN, Donald C., 8224 Menlo Dr., Baton Rouge, La. 70808
HUFFNAGLE, John D., Kent State Univ., Dept. of Econ., Kent, Ohio
HUFFSTUTLER, Clyde E., U.S. Dept. of Labor, GAO Bldg., Rm. 2814, Washington, D.C.
HUGHES, Desmond B., Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
HUGHES, Harold, Capital Univ., Dept. of Econ., and Bus. Admin., 2199 E. Main St., Columbus, Ohio 43209
HUGHES, J. R. T., Purdue Univ., Dept. of Econ., Lafayette, Ind.
HUGHES, James J., 10452 S. Wood St., Chicago, Ill. 60643
HUGHES, John J., Boston Univ., Dept. of Econ., Boston, Mass.
HUGHES, Laura B., 1804 N. Quinn St. No. 3, Arlington 9, Va.
HUGHES, Patrick R. M., 4012 Garfield Ave., St. Louis, Mo. 63113
HUGHES, Rufus B., Jr., USOM/K Nathan, APO 301, San Francisco, Calif. 96301
HUGHES, William, Int. Bank for Recon. and Dev., 1818 H St., N.W., Washington, D.C. 20433
HUGHES, William R., Boston Col., 140 Commonwealth Ave., Chestnut Hill, Boston, Mass. 02167
HUGON, James H., Portland State Col., Sch. of Bus. Admin., Portland, Ore. 97207
HUHNE, Lothar H., Paradies Str., Binningen, Basel, Switzerland
HUHS, John I., 7301 50th, N.E., Seattle, Wash. 98115
HUIZENGA, C. J., Univ. of California, Los Angeles, Grad. Sch. of Bus. Admin., Los Angeles, Calif. 90024
HULANICKI, Leo, 151 Carmel Ave., Pacific Grove, Calif. 93950
HULBERT, Theodore, 466 N. Soule, Laramie, Wyo.
HULBERT, William M., Syracuse Univ., Slocum Hall, 116 College Pl., Syracuse, N.Y. 13210
HULETT, David T., 19 Lloyd Rd., Watertown, Mass. 02172
HULETT, Leland W., Milton Col., Dept. of Bus. Admin., Milton, Wis. 53563
HULL, Alfred N., Thiel Col., Greenville, Pa. 16125
HULL, Eugene L., Trenton State Col., Trenton, N.J. 08625
HULL, Frank L., Jr., 1010 E. Colorado, Urbana, Ill. 61801
HULTMAN, Charles W., Univ. of Kentucky, Dept. of Econ., Lexington, Ky.
HUMBERT, Richard P., 5003 Sentinel Dr., Washington, D.C. 20016
HUMISTON, Raymond E., Tall Trees Lane, Wilton, Conn. 06897
HUMPHREY, David B., 2416 Roosevelt, Berkeley, Calif.
HUMPHREY, Don D., Tufts Univ., Fletcher Sch. of Law and Diplomacy, Medford, Mass. 02052
HUMPHREY, Joseph F., 1838 Westholme Ave., Los Angeles, Calif. 90025
HUND, James M., 1535 Victoria Falls Dr., Atlanta 29, Ga.
HUNG, Fred C., Econ. Res. Center, Univ. of Hawaii, Honolulu 14, Hawaii
HUNG, Nguyen dinh, 291 Ngyuen Thein Thuat, Saigon, Vietnam
HUNGATE, (Mrs.) Frank P., Univ. of Washington, Econ. Dept., Seattle, Wash. 98105
HUNGATE, Robert P., Wisconsin State Univ., Sch. of Bus. Admin., Oshkosh, Wis.
HUNOLD, Albert, Munstergasse 9, 8001 Zurich, Switzerland
HUNSBERGER, George E., University of Ark., Sch. of Bus. Admin., Fayetteville, Ark.
HUNSBERGER, Warren S., Southeast Asia Field Off., Ford Found., 477 Madison Ave., New York, N.Y. 10022
HUNT, Atha, Box 102, Carbondale, Ill.
HUNT, Elgin F., 5404 Kimbark Ave., Chicago, Ill. 60615
HUNT, George T., San Diego Mesa Col., 7250 Artillery Dr., San Diego, Calif. 92111
HUNT, Howard A., 536 Seneca St., Bethlehem, Pa. 18015
HUNT, Jerry G., 1300 30th St., B1-21, Boulder, Colo. 80302
HUNT, Joseph W., Jr., Univ. of Delaware, Econ. Dept., Newark, Del.
HUNT, Pearson, Harvard Sch. of Bus., Soldiers Field, Boston 63, Mass.
HUNT, Shane J., Econ. Growth Center, Box 1987 Yale Sta., New Haven, Conn.
HUNTER, A. Edward, 92 Clarence Ave., S.E., Minneapolis, Minn. 55414
HUNTER, Donald J., Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa 50010
HUNTER, Elmer L., Jr., 311 W. 8th St., Borger, Tex.
HUNTER, (Mrs.) Helen M., Haverford Col., Haverford, Pa. 19041
HUNTER, Holland, Haverford Col., Haverford, Pa. 19041
HUNTER, John M., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
HUNTER, Kenneth H., 204 55th St., Vienna, W.Va. 26101
HUNTER, Laurence C., Univ. of Glasgow, Dept. of Soc. and Econ. Res., 12 Bute Gardens, Glasgow, W. 2, Scotland
HUNTER, Steven J., 2731 N. Norwood St., Arlington, Va. 22207
HUNTINGTON, Virginia R., Arizona State Univ., Dept. of Acctg., Col. of Bus. Admin., Tempe, Ariz.
HUNTLEY, Patrick R., 1605 N. Herndon St., Arlington 1, Va.
HUPP, David, Univ. of California, Dept. of Agric. Econ., Berkeley, Calif.
HUPPELSBERG, Robert R., 344 Richbell Rd., Apt. B-4, Mamaroneck, N.Y.
HUQ, A. Moazzamul, Operations Res. Div., Res. Triangle Inst., Durham, N.C.
HURFF, George B., Univ. of Florida, Col. of Bus. Admin., Dept. of Econ., Gainesville, Fla. 32603
HURLEY, (Mrs.) Evelyn M., 3208 Volta Pl., N.W., Washington, D.C. 20007
HURLEY, John J., 3208 Volta Pl., N.W., Washington, D.C. 20007
HURLEY, William D., 330 Woodland Ave., Westfield, N.J. 07090
HURLEY, William E., South Greenbrier Dr., Box 517, R. R. 1, Yellow Springs, Ohio 45387
HURST, D'Orsey, Hudson House, Ardsley-On-Hudson, N.Y. 10503
HURST, James E., Park Col., Copley Hall, Parkville, Mo. 65275
HURTADO, Carlos, Ricardo Matte Perez 557, Santiago, Chile
HURTER, Arthur P., Jr., Northwestern Univ., Dept. of Ind. Eng. and Mgt. Sci., Evanston, Ill. 60201
HURTUBISE, Andre B., 2747 Maplewood No. 8, Montreal, P.Q., Canada
HURWITZ, Abner, Amer. Embassy, Freetown, Sierra Leone
HURWITZ, Jacob C., 21760 Kenosha, Oak Park, Mich. 48649
HUSAIN, A.F.A., Plan. Comm., Gov. of Pakistan, Karachi, Pakistan
HUSAIN, Ishrat Z., Lucknow Univ., Dept. of Econ., Lucknow, India

HUSBAND, William H., 5070 Lowell St., N.W., Washington 16, D.C.
HUSBY, Ralph D., 1400 S. Prairie Ave., Sioux Falls, S.D.
HUSS, Donald C., Courtney House, 55 W. 14th St., Apt. 14-B New York, N.Y. 10011
HUSSAIN, Matlub, Florida Southern Col., Dept. of Econ., Lakeland, Fla.
HUSSAYNI, Musa Y., 924 West Center, Alma, Mich. 48801
HUSSEIN, Adan A., c/o Hargeisa Local Govt., Hargeisa, Somalia
HUTCHESON, Harold H., IBM World Trade Corp., United Nations Plaza, New York 17, N.Y.
HUTCHINS, John G. B., Cornell Univ., Sch. of Bus. Admin., Ithaca, N.Y.
HUTCHINSON, Edmond C., 9619 Hillridge Dr., Kensington, Md.
HUTCHINSON, Erla A., 330 E. 93rd St., Apt. 4A, New York, N.Y. 10028
HUTCHINSON, Harry D., Univ. of Delaware, Robinson Hall, Rm. 105A, Newark, Del. 19711
HUTCHINSON, Ralph B., California State Poly. Col., Kellogg-Voorhis Campus, Pomona, Calif.
HUTH, Arno G., 41 Central Park West, New York 23, N.Y.
HUTNER, (Mrs.) Frances C., 28 Hibben Rd., Princeton, N.J. 08540
HUTNER, Simeon, 28 Hibben Rd., Princeton, N.J. 08540
HUTSON, Willie S., Bur. of the Census, Agriculture Div., Washington, D.C. 20233
HUTTER, Robert J., 1335 9th St., Boulder, Colo. 80302
HUTTMAN, John P., Foothill Col., Soc. Sci. Dept., Los Altos Hills, Calif.
HUTTON, Winfield, Apt. 6-D, 3065 Sedgwick Ave., New York 68, N.Y.
HYDE, Gary L., 3307 N. 20th St., Arlington, Va. 22207
HYMAN, David N., Princeton Univ., 192 Grad. Col., Princeton, N.J. 08540
HYMANS, Saul H., Univ. of Michigan, Econ. Dept., Ann Arbor, Mich.
HYMER, Bennett, 1652 N. Orchard, Chicago, Ill. 60614
HYMER, Stephen, Dept. of Econ., Econ. Growth Center, Box 1987, Yale Sta., New Haven, Conn. 06520
HYNES, Cecil V., 2991 School House Lane, Poplar 34 East, Philadelphia, Pa. 19144
HYSE, Richard, 135 W. Mohawk St., Oswego, N.Y. 13126
HYSON, Charles D., 7407 Honeywell Lane, Bethesda, Md. 21811

IDEN, George R., U.S. Senate, Joint Econ. Com., Washington, D.C.
IDRISI, A. Salam, 134 Sims Rd., Syracuse 10, N.Y.
IERARDI, Gordon S., 605 Third Ave., New York, N.Y.
IGE, Thomas H., Univ. of Hawaii, Honolulu, Hawaii
IGNATIN, George, Univ. of Texas, Dept. of Econ., Austin, Texas 78712
IGO, Donald J., 6209 Cloud Dr., Springfield, Va. 22150
IHNEN, Loren A., North Carolina State Col., Dept. of Agric. Econ., Raleigh, N.C.
IKEDA, Akihiro, Banco America do Sul S/A, Senador, Feijo 205 SP Br., Sao Paulo, Brazil
IKLE, (Mrs.) Doris M., 195 Rutledge Rd., Belmont, Mass. 02019
ILACQUA, Joseph A., 120 S. Angell St., Providence, R.I. 02906
ILLAN, Jose M., CENDES, Casilla Postal 2321, Quito, Ecuador
ILLIG, Kenneth W., Rt. 1, Box 55, Barrington, Ill. 60010
ILUSORIO, Ramon K., 20 Villamar 1st St., Paranaque, Rizal, Philippines
IM, Ik-Soon, Yonsei Univ., Soudemounkü, Seoul, Korea
IMMACULATIM, Sister M., Immaculata Col., Immaculata, Pa. 19315
IMMERDAUER, Bernard, AID/Recife, Dept. of State, Washington, D.C. 20521
IMPELLIZZERI, Julius S., 630 3rd Ave., New York 17, N.Y.
INFINGER, Marvin Leslie, The Citadel, Dept. of Bus. Admin., Charleston, S.C. 29409
INGBAR, Mary Lee, 41 Montvale Rd., Newton Center, Mass. 01355
INGHAM, George, 10 Redway Rd., Ossining, N.Y. 10562
INGLES, Jose D., Philippine Embassy, Bangkok, Thailand
INGRAHAM, John W., 501 E. 79th St., Apt. 9-F, New York, N.Y.
INGRAM, James C., Univ. of North Carolina Sch. of Bus. Admin., Chapel Hill, N.C.
INGRAM, John W., Southern Railway System, Marketing and Research, Box 1808, Washington 13, D.C.
INGRAM, Marvin L., Richmond Professional Inst., 807 W. Franklin St., Richmond, Va. 23220
INGWERSEN, John, 206 W. Third St., Collinsville, Ill. 62234
INMAN, Buis T., 5528 N. 17th St., Arlington 5, Va.
INMAN, Mark K., Univ. of Western Ontario, London, Ont., Canada
INTRILIGATOR, Michael D., Univ. of California at Los Angeles, Dept. of Econ., Los Angeles, Calif. 90024
INVEISS, Uldis E., Carroll Col., 100 N. East Ave., Waukesha, Wis.
INWOOD, Ernest L., 1957 Sycamore Glen, San Jose, Calif. 95125
IRONS, Watrous H., Fed. Res. Bank, Dallas 2, Tex.
IRSAY, (Mrs.) Sandra L. B., 1100 Delaware Ave., Wilmington, Del. 19806
IRWIN, Donald M., 904 Pemberton Rd., Grosse Pointe Park 30, Mich.
ISAAC, Charles M., 7 E. 14th St., Apt. 307, New York, N.Y. 10003
ISAAC, Michael E., 5614 Broadway, Bronx, N.Y. 10463
ISARAPANDH, Glom, Technical and Planning Office, Ministry of National Develop., Bangkok, Thailand
ISARD, Walter, Univ. of Pennsylvania, Wharton School, Philadelphia 4, Pa.
ISAZA-BOTERO, Rafael, Carrera 6 No. 67-70, Bogota 2, Colombia
ISBISTER, Claude M., 185 Kamloops Ave., Ottawa 10, Ont., Canada
ISBISTER, John, 407-A Butler Ave., Princeton, N.J.
ISCHBOLDIN, Boris, St. Louis Univ., Sch. of Com. and Fin., 3674 Lindell Blvd., St. Louis 8, Mo.
ISHEE, Sidney, Univ. of Maryland, Dept. of Agric. Econ., College Park, Md.
ISHIKAWA, Mamoru, 506 Gettysburg St., Pittsburgh, Pa. 15206
ISING, (Mrs.) Carol E., 204 W. Pennsylvania, Urbana, Ill. 61801
ISING, Thomas J., 204 W. Pennsylvania, Urbana, Ill. 61801
ISLA, Eugenio F., Cespedes 2472, 2º Piso-DTO B, Buenos Aires, Argentina
ISLAM, Nurul, Pakistan Inst. of Dev. Econ., Gov. of Pakistan, Karachi, Pakistan
ISSAWI, Charles, Room 601, Kent Hall, Columbia Univ., New York 27, N.Y.
ITO, Masakichi, 1205 Sekimae 5-chome, Musashino-shi, Tokyo, Japan
ITO, Rikuma, 30230 Woodgate Dr., Southfield, Mich. 48075
IULO, William, Box 112, Col. Sta., Pullman, Wash. 99163
IVERSEN, Lothar I., 1903 Van Buren St., New Holstein, Wis. 53061
IVEZIC, Boris M., 125 Christopher St., Apt. 3-G, New York 14, N.Y.
IVORY, Paul E., Univ. of California, Dept. of Econ., Berkeley 4, Calif.
IVY, Michel M., 12705 Connecticut Ave., Silver Spring, Md.
IWAMOTO, Kiyotoshi, Dept. of Econ., Univ. of Utah, Salt Lake City, Utah
IWANAKA, George M., DePaul Univ., Col. of Com., 25 E. Jackson Blvd., Chicago, Ill. 60604
IWAND, Thomas, Pennsylvania State Univ.,

Econ. Dept., Boucke Bldg., University Park, Pa. 17774
IYENGAR, A. V. N., Dept. of Econ. Affairs, 2nd Fl., Jevan Deep, 10 Parliament St., New Delhi 1, India
IZZO, Lucio, Via Valsoldo 111, Rome, Italy
IZZOLO, John N., 154-04 Willets Point Blvd., Whitestone, N.Y. 11357

JABLONSKI, Louis S., 8447 W. Lisbon Ave., Milwaukee, Wis. 53222
JACKENDOFF, Nathaniel, Temple Univ., Dept. of Fin., Philadelphia 22, Pa.
JACKMAN, Patrick, 6000 Springhill Dr., Greenbelt, Md.
JACKSON, Andrew, 6401 N. 28th St., Arlington, Va.
JACKSON, Elmo L., Dept. of Econ., Univ. of Florida, Gainesville, Fla.
JACKSON, Frank H., Alma Col., Alma, Mich. 48801
JACKSON, Frederic A., Morgan State Col., Baltimore, Md. 21212
JACKSON, Harold L., 2044 University Ave., Berkeley, Calif. 94704
JACKSON, James F., Jr., Oklahoma State Univ., Col. of Bus., Stillwater, Okla.
JACKSON, James R., R.F.D. 1, Wentzville, Mo. 63385
JACKSON, Marvin R., Jr., Arizona State Univ., Dept. of Econ., Tempe, Ariz.
JACOBS, Donald P., 830 Milburn St., Evanston, Ill.
JACOBS, George R., 5008 Elsmere Pl., Bethesda, Md. 21811
JACOBS, John D., 4419 P St., N.W., Washington 7, D.C.
JACOBS, Philip, 5100 Hingston Ave., Montreal 29, P.Q., Canada
JACOBS, Sidney P., 314 Leah Miller Dr., Ft. Walton Beach, Fla. 32548
JACOBS, Stanley, 1746 Jennifer Rd., Apt. D-1, Lexington, Ky. 40505
JACOBSEN, Ian C., 163 Encina Hall, Stanford Univ., Stanford, California 94305
JACOBSON, Allen H., 66 W. Gun Hill Rd., New York, N.Y. 10467
JACOBSON, Sol, 1662 Glenwood Rd., Brooklyn, N.Y. 11230
JACOBY, Henry D., 398 School St., Watertown, Mass. 02172
JACOBY, Neil H., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles 24, Calif.
JACQUEMIN, Alex P., 2227 Dwight Way, Berkeley 4, Calif.
JACQUES, (Mrs.) Francoise P., 41, Ave. de Bozenval, Saint Cloud, (S et O), France
JAEGER, Herman F., 7030th CSW Hq. Sq., Box 632, APO New York, N.Y. 09012
JAEGER, Martin, Univ. of British Columbia, Dept. of Agric. Econ., Vancouver 8, B.C., Canada
JAEGER, Roy H., 150 37th St., Union City, N.J. 07087
JAENICKE, Henry R., 62 Oak Lane, Lancaster, Pa. 17603
JAFFE, Eugene L., 63-22 83rd Pl., Flushing, N.Y. 11379
JAFFE, John, 1175 York Ave., Apt. 9-E, New York 21, N.Y.
JAFFE, Sidney A., 5166 34th St., N.W., Washington, D.C. 20008
JAFFE, William, Northwestern Univ., Econ. Dept., Evanston, Ill. 60201
JAFFEE, Dwight M., Massachusettts Inst. of Tech., Econ. Dept., Cambridge, Mass. 01922
JAFFY, Florence, 40 28th St., San Francisco, Calif. 94110
JAHN, Louis R., 245 Upson Lane, New Brunswick, N.J.
JAHNKE, Wilfred, In den Zeulackern 7, Frankfurt am Main 14, Germany
JAIN, Chaman Lal, 107 Joralemon St., No. 2, Brooklyn, N.Y. 11201
JAIN, Harish C., 5260 Kent St., Apt. 1, Halifax, N.S., Canada
JAKOBS, Henri M. H., 2405 Cherry Ave., Alliance, Ohio 44602
JAKUBAUSKAS, Edward B., Iowa State Univ., Dept. of Econ., Ames, Iowa
JALLOW, Ray, 600 S. Spring St., Los Angeles, California
JAMES, (Mrs.) Anna H., 4562 Penridge Rd., Toledo, Ohio 43615
JAMES, Brian O., 324 Persimmon Rd., Walnut Creek, Calif. 94598
JAMES, Charles M., 6912 Boyer St., Philadelphia 19, Pa
JAMES, Clifford L., 1687 Guilford Dr., Columbus, Ohio
JAMES, Dilmus D., 1419-F Spartan Village, East Lansing, Mich.
JAMES, (Mrs.) Estelle, Univ. of California, Dept. of Econ., Davis, Calif. 95616
JAMES, F. Cyril, Pipers Croft, Devonshire Ave., Amersham, Bucks., England
JAMES, George W., 2883 Halstead Rd., Columbus 21, Ohio
JAMES, John H., Univ. of Florida, Col. of Bus. Admin., Dept. of Mgt., Gainesville, Fla.
JAMES, John M., 2105 20th St., Lubbock, Tex. 79411
JAMES, Lee M., Michigan State Univ., Dept of Forestry, East Lansing, Mich.
JAMES, Norman F., R.R. 1, Box 467, Wharton, N.J. 07885
JAMES, Ralph C., Univ. of California, Dept. of Econ., Davis, Calif. 95616
JAMES, Robert G., 150 E. 42nd St., New York, N.Y. 10017
JAMES, Rosella, 30 Second St. Pike, Huntingdon Valley, Pa. 19006
JAMISON, Conrad C., Security First Nat. Bank, P.O. Box 2097, Terminal Annex, Los Angeles 54, Calif.
JAMISON, John N., Waynesburg Col., Waynesburg, Pa. 15370
JAMJOOM, Mohamed A., 3730 McClintock Ave., Apt. 433, Los Angeles, Calif. 90007
JANASKIE, Crystal R., 307 Harmony Dr., North Aurora, Ill. 60542
JANCAUSKAS, R., 6525 Sheridan Rd., Chicago, Ill. 60626
JANG, young Sik, 40 S. Main Ave., Apt. 4-S, Albany, N.Y. 12208
JANSEN, Cornelis J., 2201 Paul Spring Rd., Alexandria, Va. 22307
JANSEN, Edmund F., Jr., N.C. State Misson to Peru, U.S. Embassy USAID, Lima, Peru
JANSSEN, Hans, Yankton Col., Dept. of Econ., Yankton, S. Dak. 57078
JANSSEN, Melvin Russel, U.S. Dept. of Agric., Econ. Res. Serv., Econ. Devel. Div., Washington, D.C. 20250
JAQUES, David G., 247 Oberlin Dr., Claremont, Calif. 91711
JARACZ, William A., 1197 Radnor Pl., Falls Church, Va.
JARAMILLO, Rodrigo, 8510 16th St., Apt. 419, Silver Spring, Md.
JARENCIO, Cecilia P., 79-11 41st Ave., Elmhurst, N.Y.
JARRETT, James McPherson, 7017 Wilson Lane, Bethesda, Md. 20034
JARRETT, Jay W., 7315 Gary St., Springfield, Va. 22150
JARRETT, Jeffrey, 49 Laurel Dr., Oakmont Park, Scranton, Pa. 18505
JARVIS, Jack B., 1270 S.W. Menlo Dr., Beaverton, Ore. 97005
JASKOLD-GABSZEWICZ, Jean, Le Moulin, Beauvechain, Belgium
JASZI, George, 4910 Cumberland Ave., Chevy Chase, Md. 21620
JAUCH, Heinz, 36 Swiss Ter., Packanack Lake, Wayne, N.J.
JAVAID, K. R., Duke Univ., Dept. of Econ., Durham, N.C. 27706
JAY, Richard E., Box 9467, Mills Col., Oakland 13, Calif.
JAYNES, Edgar N., Fairmont State Col., Fairmont, W.Va. 26554
JEAN, William H., Univ. of Southern California, Sch. of Bus. Admin., University Park, Los Angeles, Calif. 90007
JEFFERIS, Raymond P., Jr., 110 Mulberry Lane, Media, Delaware Co., Pa. 19063
JEFFREY, Arthur D., Univ. of Rhode Island, 136 Woodward Hall, Kingston, R.I. 02836
JEFFREY, K. C., 215 Market St., San Francisco 5, Calif.
JEHRING, John J., 1655 Capital Ave., Madison, Wis. 53705
JEN, Frank C., State Univ. of New York at

Buffalo, Sch. of Bus. Admin., Buffalo, N.Y. 14214
JENCKS, Clinton E., San Diego State Col., Dept. of Econ., San Diego, Calif. 92115
JENKINS, H. David, 829 Colby Ct., De Kalb, Ill. 60115
JENKINS, John W., 8 Peter Cooper Rd., New York, N.Y. 10010
JENKINS, Lloyd A., 25 Fifth Ave., New York 3, N.Y.
JENNESS, Robert A., 1251 Cardero St., #1202, Vancouver, B.C., Canada
JENNINGS, (Mrs.) Donna C., 5536 Kamin St., Apt. 6, Pittsburgh, Pa.
JENNINGS, (Mrs.) Frances N., 26 Tioga Dr., Jericho, L.I., N.Y. 11753
JENNINGS, George, Univ. of Richmond, Box 26, Richmond, Va. 23173
JENNINGS, Henrietta C., 104 W. Market St., Danville, Pa. 17821
JENNINGS, James M., 1858 Chatfield Rd., Columbus 21, Ohio
JENNINGS, Robert M., Colorado State Univ., Dept. of Bus., Fort Collins, Colo. 80521
JENNINGS, Thomas A., Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta, Ga. 30339
JENNY, Hans H., Col. of Wooster, Wooster, Ohio
JENSEN, Bartell, Utah State Univ., Dept. of Econ., Logan, Utah 84321
JENSEN, Finn Bjorn, Lehigh Univ., Col. of Bus. Admin., Bethlehem, Pa.
JENSEN, Fred O., 33 Barrington Rd., Ridgewood, N.J. 07450
JENSEN, George, California State Col., Dept. of Econ., Los Angeles, Calif. 90032
JENSEN, Hans E., Univ. of Tennessee, Dept. of Econ., Knoxville, Tenn. 37916
JENSEN, Harald R., Univ. of Minnesota, Inst. of Agric., Dept. of Agric. Econ., St. Paul, Minn. 55101
JENSEN, Howard C., Corporate Planning, Gen. Elec. Co., 570 Lexington Ave., New York, N.Y. 10022
JENSEN, Howard G., Univ. of Colorado, Sch. of Bus., Boulder, Colo. 80302
JENSEN, James E., 601 Frontenac Ave., Stevens Point, Wis.
JENSEN, Michael C., 5654 S. Drexel, Chicago, Ill. 60637
JENSEN, Richard W., 2825 Glendessary Lane, Santa Barbara, Calif. 93105
JENSEN, Ronald L., Emory Univ., Sch. of Bus., Atlanta 22, Ga
JEONG, Dong-Kuen, Univ. of Hawaii, Mail Box 1360, East-West Center, Honolulu, Hawaii 96822
JEPSEN, Charles H., Hunting Ridge Rd., Greenwich, Conn. 06832
JESKE, William P., Pacific Northwest Bell, 1200 3rd Ave., Room 1516, Exchange Bldg., Seattle 1, Wash.
JESNESS, Oscar Bernard, Univ. of Minnesota, Inst. of Agric., Div. of Agric. Econ., St. Paul, Minn. 55101
JESSUP, Paul F., 415 N. Country Club Dr., McHenry, Ill. 60050
JESSWEIN, Wayne A., Univ. of Illinois, Dept. of Econ., 113 Com. Annex, Urbana, Ill. 61801
JEUCK, John Edward, Univ. of Chicago, Sch. of Bus., Chicago, Ill. 60637
JEVREMOVIC, Savo D., P.O. Box 127, Alfred, N.Y. 14802
JEWETT, Frank I., Humboldt State Col., Div. of Soc. Sci., Arcata, Calif. 05521
JEWETT, Roy B., 2585 Perry Ave. N., Minneapolis, Minn.
JICHA, Oldrich, Bowling Green Univ., Dept. of Bus. Admin., Bowling Green, Ohio 43402
JIMENEZ, Jose M., Apartado 3249, San Jose, Costa Rica
JOACHIMOWSKI, A., Univ. of Saskatchewan, Regina Campus, Div. of Soc. Sci., Regina, Sask., Canada
JOCHIMSEN, Reimut, Duesternbrooker Weg 47, 2300 Kiel, Germany
JOCHNOWITZ, (Mrs.) Rose, 5 Greenway N., Albany 8, N.Y.
JOEL, Clark, Office of Econ. Affairs, USAID, APO San Francisco, Calif. 96352
JOFFE, Jerome, 15 Cranberry St., Brooklyn 1, N.Y.
JOHANSEN, Chester E., 731 Boulevard, Westfield, N.J.
JOHANSEN, John P., 600 W. College Ave., Marquette, Mich. 49855
JOHN, Joshua, 7137 Hamilton, Montreal 20, P.Q., Canada
JOHN, Lewis G., Washington and Lee Univ., Washington Hall 25, Lexington, Va.
JOHNS, Ray M., 4207 Longfellow St., Hyattsville, Md.
JOHNS, Vernon O., 115 Country Club Dr., Blackburg, Va.
JOHNSON, Alvin S., New Sch. of Soc. Res., 66 W. 12th St., New York, N.Y.
JOHNSON, Byron L., 2451 S. Dahlia Lane, Denver, Colo. 80222
JOHNSON, Clarke C., 529 Grad. House, West Lafayette, Ind. 47906
JOHNSON, D. Gale, Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
JOHNSON, David B., 27 University Circle, Charlottesville, Va.
JOHNSON, David B., Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison 6, Wis.
JOHNSON, David E., 2674 17th Ave., San Francisco 16, Calif.
JOHNSON, Dudley D., House 9, Apt. B-4, New Copeley Hill, Charlottesville, Va. 22901
JOHNSON, Dudley W., Univ. of Washington, Col. of Bus. Admin., Dept. of Acctg., Fin., and Statis., Seattle 5, Wash.
JOHNSON, Edwin C., 327 Mansion Dr., Alexandria, Va.
JOHNSON, Edwin L., 2235 N. Albemarle St., Arlington, Va. 22207
JOHNSON, Everett H., 13601 Glen Mill Rd., Rockville, Md.
JOHNSON, G. Griffith, Jr., 6412 Garnet Dr., Chevy Chase, Md. 21620
JOHNSON, Harold L., 2026 N. Akin Dr., N.E., Atlanta, Ga. 30329
JOHNSON, Harry G., Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
JOHNSON, Harry L., Univ. of Texas, Room 129 BEB, Austin, Tex.
JOHNSON, Howard W., 111 Memorial Dr., Cambridge, Mass. 02142
JOHNSON, J. C., Box 204, Pasadena, Calif.
JOHNSON, James Arthur, Dept. of Polit. Econ., McMaster Univ., Hamilton, Ont., Canada
JOHNSON, Jerry L., Dow Chemical Co., Bioproducts Dept., Midland, Mich.
JOHNSON, John L., 113 Madison, Big Rapids, Mich. 49307
JOHNSON, John W., 3834 Port Arthur Rd., Beaumont, Tex. 77705
JOHNSON, Keith B., Univ. of Connecticut, Sch. of Bus., U-31, Storrs, Conn. 06268
JOHNSON, Keith H., Univ. of Illinois, 408 David Kinley Hall, Urbana, Ill. 61803
JOHNSON, Keith W., Pacific Gas and Electric, 245 Market St., San Francisco 6, Calif.
JOHNSON, Leland L., RAND Corp., 1700 Main St., Santa Monica, Calif. 90406
JOHNSON, M. Bruce, Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
JOHNSON, Mathews M., 338 Park Ave., Swarthmore, Pa. 19081
JOHNSON, Milton G., 2906 Radius Rd., Silver Spring, Md. 20902
JOHNSON, Norris O., First National City Bank, 399 Park Ave., New York 22, N.Y.
JOHNSON, Ole S., 3209 N. Preston Dr., Oklahoma City, Okla. 73112
JOHNSON, Oliver R., Col. of Agric., Univ. of Missouri, Columbia, Mo.
JOHNSON, Orace E., Univ. of Missouri, Sch. of Bus., Columbia, Mo. 65201
JOHNSON, Paul R., North Carolina State Col., Dept. of Agric. Econ., Raleigh, N.C.
JOHNSON, Paul V., Purdue Univ., Grad. Sch. of Ind. Admin., West Lafayette, Ind. 47178
JOHNSON, Robert E., 5835 Deerhead Rd., Malibu, Calif.
JOHNSON, Robert E., 6 Skytop Ter., Upper Montclair, N.J. 07043
JOHNSON, Robert E., Jr., 3Q Magie Apts., Faculty Rd., Princeton, N.J.
JOHNSON, Robert P., Univ. of Washington, Dept. of Econ., Seattle, Wash.
JOHNSON, Robert S., 115 Alderman Rd., Charlottesville, Va.

JOHNSON, Robert W., Purdue Univ., Grad. Sch. of Ind. Admin., Lafayette, Ind. 47907
JOHNSON, Roger R., 55 James St., Saco, Me. 04072
JOHNSON, Ronald L., 11825 Gilmore St., Apt. 201, North Hollywood, Calif. 91606
JOHNSON, Russell C., P.O. Box 278, Palos Heights, Ill. 60463
JOHNSON, Sherman E., 118 N. Jackson St., Arlington, Va. 22201
JOHNSON, Stanley R., Univ. of Missouri, Econ. Dept., B. and P. A., Columbia, Mo.
JOHNSON, Thomas F., Amer. Enterprise Inst. for Pub. Policy Res., 1200 17th St., N.W., Washington, D.C. 20036
JOHNSON, Thomas M., Univ. of Illinois, Chicago Circle, Col. of Bus. Admin., Box 4348, Chicago, Ill. 60680
JOHNSON, V. W., 2928 Waterloo Rd., Cedar Falls, Iowa 50613
JOHNSON, Walter E., Box 278, Palos Heights, Ill. 60463
JOHNSON, Walter E., II, P.O. Box 278, Palos Heights, Ill. 60463
JOHNSON, Walter H., 363 High Tee, Willowick, Ohio
JOHNSON, Walter L., Univ. of Missouri, Dept. of Econ., Columbia, Mo.
JOHNSON, William A., 933 Fifth St., Apt. 9, Santa Monica, Calif.
JOHNSTON, Bruce F., Food Res. Inst., Stanford Univ., Stanford, Calif. 96128
JOHNSTON, David R., 6025 Hoeveler St., Pittsburgh, Pa. 15206
JOHNSTON, Everett E., 3308 Rolling Hills Pl. S., Renton, Wash. 98055
JOHNSTON, J. I., Ste. 4,622 Athabasca St., W., Moose Jaw, Sask., Canada
JOHNSTON, James L., 2434 Twentieth St., N.W., Washington, D.C. 20009
JOHNSTON, John, Univ. of Manchester, Dept. of Econ., Manchester 13, England
JOHNSTON, John W., Rice Univ., Dept. of Com., Houston 1, Tex.
JOHNSTON, Myron E., Jr., 4235 Wyncliff Dr., Richmond, Va. 23235
JOHNSTON, Richard S., Univ. of California, Dept. of Agric. Econ., Giannini Hall, Berkeley, Calif.
JOHNSTON, Robert A., San Jose State Col., Dept. of Econ., San Jose 14, Calif.
JOHNSTON, Victor W., 256 Crocus Ave., Ottawa 8, Ont., Canada
JOHNSTON, Warren E., Univ. of California, Dept. of Agric. Econ., Davis, Calif. 95616
JOHNSTONE, James, 175 Putnam Ave., Cambridge, Mass. 01922
JOINER, Charles L., F-I-A Northington, Tuscaloosa, Ala.
JOLLEY, Elmer O., Jr., 180 Meadowbrook Rd., Weston, Mass. 02193
JONAS, Matthew H., Louisiana Dept. of Com. and Ind., P.O. Box 4185, Capitol Sta., Baton Rouge 4, La.
JONAS, Paul, 60 E. 9th St., New York, N.Y. 10003
JONES, A. J., The University, Sch. of Econ. Studies, Leeds, England
JONES, (Mrs.) Alice Hanson, 404 Yorkshire Pl., Webster Groves, Mo. 63119
JONES, (Mrs.) Barbara Ann, 109 E. Clark, Apt. 4, Champaign, Ill.
JONES, Barclay G., Cornell Univ., Dept. of Planning, Ithaca, N.Y.
JONES, Bryce J., 2017 W. 48th Ter., Westwood Hills, Shawnee Mission, Kans.
JONES, Carl R., 50 Esplanade, Pacific Grove, Calif. 93950
JONES, Donald L., 3507 Giles St., Portsmouth, Va. 23707
JONES, Douglas N., Federal Field Com., Box 2492, Anchorage, Alaska 99501
JONES, Eliot, 2140 Santa Cruz Ave., Menlo Park, Calif.
JONES, Ernest C., Office of the Air Attache, APO New York 09662
JONES, Ethel B., 680 Hill St., Apt. 6, Athens, Ga. 30601
JONES, Francis P., Jr., 803 Third St., Newport, Ark.
JONES, Frank W., Bus. Dept., Mankato State Col., Mankato, Minn.
JONES, Frederic B., 82 Triangle St., Amherst, Mass.
JONES, Frederick R., 4896 Chevy Chase Blvd., Chevy Chase, Md. 21620
JONES, George M., Matson Res. Corp., 215 Market St., San Francisco, Calif.
JONES, Herschel F., 9748 S.E. 42nd St., Mercer Island, Wash. 98040
JONES, Homer, 404 Yorkshire Pl., Webster Groves, Mo. 63119
JONES, Jerry Lee, Box 232, Bethany, W. Va. 26032
JONES, John C. H., Univ. of Victoria, Victoria, B.C., Canada
JONES, Lamar B., 604 Broce Dr., Blacksburg, Va. 24060
JONES, Lervy J., 1442 N. Sunset Dr., Cavalier Hills, Tempe, Ariz.
JONES, Martin V., Mitre Corp., Econ. Factors Office, Bedford, Mass.
JONES, Mildred E., 840 Fort Wood St., Chattanooga, Tenn. 37403
JONES, Oliver H., 10417 Burnt Ember Dr., Silver Spring, Md. 20903
JONES, Peter d'A., Smith Col., Dept. of History, Northampton, Mass.
JONES, Phillip E., U.S. Beet Sugar Asso., Tower Bldg., Washington 5, D.C.
JONES, Robert C., Univ. of Pennsylvania, Wharton Sch., Philadelphia 4, Pa.
JONES, Robert E., 22 Friendship Lane, Colorado Springs, Colo. 80904
JONES, Robert V., Box 308, Route 2, Mundelein, Ill. 60060
JONES, Roberto C., Chilpancingo Num. 23, Mexico City 11, D.F., Mexico
JONES, Ronald W., 5105 Knox Lane, Madison, Wis. 53711
JONES, Ronald Winthrop, Univ. of Rochester, Dept. of Econ., Rochester, N.Y.
JONES, Russell R., 231 Center Ave., Greensburg, Pa. 15601
JONES, Ted L., Ohio State Univ., Dept. of Agric. Econ. and Rural Soc., 2120 Fyffe Rd., Columbus, Ohio 43210
JONES, Walter K., 7138 Carroll Ave., Tacoma Park, Md. 20012
JONES, William G., 8508 Pelham Rd., Bethesda, Md. 21811
JONES, William O., 590 San Juan, Stanford, Calif. 96128
JONISH, James E., 5834 S. Trumbull Ave., Chicago, Ill. 60629
JORBERG, Lennart, Trastvagen 8, Lund, Sweden
JORDAN, Amos A., Dept. of Soc. Sci., West Point, N.Y. 10996
JORDAN, Carl P., 1874 Cloverleaf St., Bethlehem, Pa.
JORDAN, John M., Loyola Col., 4501 N. Charles St., Baltimore, Md. 21210
JORDAN, William A., 624 Mayfield Ave., Stanford, Calif. 94305
JORGENSEN, Charles J., Norwich Univ., Econ. Dept., Northfield, Vt. 05663
JORGENSEN, Leon P., Trust Co. of Georgia, P.O. Drawer 4418, Atlanta, Ga. 30302
JORGENSEN, Roy B., 253 Melbourne Ave., Mamaroneck, N.Y.
JORGENSEN, Thorvald W., 2004 Osborn Dr., Silver Spring, Md.
JORGENSON, Dale W., Univ. of California, Dept. of Econ., Berkeley 4, Calif.
JOSEPH, Hyman, 4635 N. Kedzie Ave., Chicago, Ill. 60625
JOSEPH, Myron L., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh 13, Pa.
JOSEPH, Rodney A., 8600 16th St., Apt. 214, Silver Spring, Md.
JOSHI, Murli Dhar, 75 Badshah Bagh, Lucknow 7, U.P., India
JOSKOW, Jules, 80 Pine St., New York 5, N.Y.
JOST, Robert R., Ball State Univ., Dept. of Econ. and Polit. Sci., Muncie, Ind. 47306
JOUN, Young P., California State Col. at Fullerton, Dept. of Econ., Fullerton, Calif. 92631
JOYCE, Jon M., 2219 Payne, Evanston, Ill.
JOYCE, W. Seavey, Boston Col., Col. of Bus. Admin., Chestnut Hill, Mass. 01012
JUCIUS, Michael J., Ohio State Univ., Hagerty Hall, 1775 S. College Rd., Columbus, Ohio 43210

JUCKER-FLEETWOOD, (Mrs.) Erin E., Lerchenstrasse 83, 4000 Basel, Switzerland
JUDD, Frank, Brown and Williamson Tobacco Co., 1600 West Hill St., Louisville, Ky.
JUDD, Perce R., 195 Woodbury Rd., Hicksville, L.I., N.Y. 11801
JUDEK, Stanislaw, Univ. of Ottawa, Ottawa, Ont., Canada
JUDGE, Jerome J., Curry Col., Brush Hill-Dana, Milton, Mass. 01529
JUDSON, T. Andrew, Bishop's Univ., Lennoxville, P.Q., Canada
JUDY, Richard W., Univ. of Toronto, Dept. of Polit. Econ., 100 St. George St., Toronto 5, Ont., Canada
JUERGENSMEYER, John E., Suite 1103, Tower Bldg., Elgin, Ill. 60120
JUERS, Linley E., USDA, Econ. Res. Serv., Room 3047, South Bldg., Washington, D.C. 20025
JUHASZ, Kalman, 4543 Lomina Ave., Lakewood, Calif.
JUMP, Bernard, Jr., Esso Standard Eastern, Inc., Econ. Coordination Dept., 15 W. 51st St., New York, N.Y. 10019
JUN-BO, Kim, Chonnam Nat. Univ., Kwangju, Korea
JUNG, Allen F., Loyola Univ., 820 N. Michigan Ave., Chicago, Ill. 60611
JUNK, Paul E., 916 Maplewood Dr., Columbia, Mo.
JUNKER, Louis J., Western Michigan Univ., Dept. of Econ., Kalamazoo, Mich.
JURBALA, John, U.S. Army Post Orleans, Transportation Div., APO New York 09063
JURGENSEN, Edgard, 431 Riverside Dr., Apt. 7-L, New York, N.Y. 10025
JUSSAWALLA, (Mrs.) M. F., Sohroza Panjagutte, Hyderabad 34, India
JUSTA, Sister Mary, Clarke Col., Dubuque, Iowa
JUSTER, F. Thomas, 123 Essex Ave., Bloomfield, N.J.
JUSTIN, Jules J., 21 E. 40th St., New York 16, N.Y.
JUUL, Halvor, Jr., 8035 Park Lane, Bethesda, Md. 21811

KAATZ, John R., 15 Maywood, Pleasant Ridge, Mich. 48069
KABIS, Zeinhom M., 1107 W. Green, Apt. 636, Urbana, Ill. 61801
KACSER, Pamela, American Univ., Dept. of Econ., Washington, D.C. 20016
KADE, Gerhard, Zabel-Kruger-Damm 189, Berlin-Lubars, Germany
KADOW, Eugene V., Jr., 1861 N. Mountain Ave., Claremont, Calif.
KADRAGIC, Al M., 10 W. 15th St., Apt. 11E, New York 11, N.Y.
KADRAGIC, Catherine S., 10 W. 15th St., Apt. 11-E, New York 11, N.Y.
KAFKA, Alexandre, Univ. of Virginia, James Wilson Dept. of Econ., Charlottesville, Va.
KAFOGLIS, (Mrs.) Madelyn L., Univ. of Florida, Dept. of Econ., Gainesville, Fla.
KAFOGLIS, Milton Z., Univ. of Florida, Dept. of Econ., Gainesville, Fla.
KAGAN, S. Sioma J., Univ. of Oregon, Sch. of Bus. Admin., Eugene, Ore.
KAHAN, Arcadius, Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
KAHANE, Benjamin B., Metro-Goldwyn-Mayer, Inc., Culver City, Calif.
KAHL, Alfred L., Jr., 240 Weymanda Circle, Athens, Ga. 30601
KAHLER, Alfred, New School for Soc. Res., 66 W. 12th St., New York, N.Y. 10011
KAHLER, Clyde M., 900 Weldon Lane, Bryn Mawr, Pa. 19010
KAHLER, Ruel C., 6851 Michael Dr., Cincinnati, Ohio 45243
KAHN, Alan Russell, 14 E. 75th St., Apt. 6D, New York 21, N.Y.
KAHN, Alfred E., Cornell Univ., Dept. of Econ., Ithaca, N.Y.
KAHN, C. Harry, Barnard, Vt. 05032
KAHN, Lewis G., 3419 Greenleaf, Skokie, Ill. 60077
KAHN, Louis B., 573 The Alameda, Berkeley, Calif.
KAHN, Mark L., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
KAHNG, Michael Sun M., Univ. of Minnesota, Morris, Div. of the Soc. Sci., Morris, Minn. 56268
KAIHARA, Motosuke, Kyoto Univ., Col. of Agric., Sakyo-ku, Kyoto, Japan
KAIKOW, Howard L., 221-03 137th Ave., Jamaica, N.Y. 11413
KAIN, John F., Harvard Univ. Dept. of Econ., Cambridge, Mass. 01922
KAISER, Carl W., Univ. of Rhode Island, Sch. of Bus. Admin., Kingston, R.I. 02836
KAISH, Stanley, Rutgers—The State Univ., Newark, N.J.
KAITZ, Hyman B., 7216 Beechwood Rd., Hollins Hills, Alexandria, Va. 22307
KALACHEK, Edward, Washington Univ., Dept. of Econ., Box 208, St. Louis, Mo. 63130
KALAMOTOUSAKIS, George J., 280 Riverside Dr., Apt. 13j, New York 25, N.Y.
KALB, Klaus, Univ. of North Dakota, Dept. of Econ., Grand Forks, N.D.
KALB, Seymour, 464 Garden Blvd., Garden City, L.I., N.Y.
KALCIC, Dismas B., St. Procopius Col., Lisle, Ill. 60532
KALDENBACH, Robert J., 159 Wilson St., Albany, Calif. 94507
KALDENBERG, Ron, Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
KALDOR, N., King's Col., Cambridge, England
KALIA, Krishan K., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
KALIAN, Edward A. K., 625 W. 113th St., New York 25, N.Y.
KALIK, Harvey P., 48 Hillcrest Lane, Willingboro, N.J. 08046
KALINOWSKI, Sister Aaron, Col. of St. Benedict, St. Joseph, Minn. 56374
KALISH, Richard J., State Univ. of New York, State Campus, Grad. Sch. of Pub. Aff., Albany, N.Y. 12226
KALKSTEIN, Howard I., 239 Montgomery St., Highland Park, N.J. 07422
KALLAY-GONYE, L., Allstate Ins. Co., 7447 Skokie Blvd., Skokie, Ill.
KALMANOFF, George, 5402 Wehawken Rd., Washington, D.C. 20016
KALTENBORN, Howard S., W. Waldheim Rd., Pittsburgh 15, Pa.
KAMARCK, Andrew M., Econ. Dept., World Bank, 1818 H St., N.W., Washington, D.C. 20433
KAMEL, Georges, P.O. Box 797, Beirut, Lebanon
KAMER, Pearl, 144-61 68th Ave., Flushing 67, N.Y.
KAMERSCHEN, David R., Washington Univ., Econ. Dept., St. Louis, Mo.
KAMINOW, Ira, Univ. of Rochester, Dept. of Econ., Rochester, N.Y.
KAMINS, Robert M., 2400 Parker Pl., Honolulu 14, Hawaii
KAMM, Jacob O., 4211 Wooster Rd., Cleveland 26, Ohio
KAMNA, Ronald W., 9110 Hassalo N.E., Apt. 120, Portland, Ore. 97220
KAMPMEIER, Roland A., 1742 Crestwood Dr., Chattanooga 5, Tenn.
KAMRANY, Nake M., 1106 Kagawa St., Pacific Palisades, Calif. 90272
KAMYS, (Mrs.) Eleanor H., Cave Hill Farm, Montague, Mass. 01351
KANAMORI, Hisao, 1538, 6, Sugamo, Toshima-ku, Tokyo, Japan
KANAPATHY, Veluppillai, United Malayan Banking Corp., Ltd., P.O. Box 2006, Kuala Lumpur, Malaysia
KANE, Edward J., Boston Col., Dept. of Econ., Chestnut Hill, Mass. 02167
KANE, John Ewing, Univ. of Arkansas, Col. of Bus. Admin., Fayetteville, Ark.
KANE, Joseph A., La Salle Col., Dept. of Econ., Philadelphia 41, Pa.
KANEDA, Hiromitsu, Univ. of California, Dept. of Econ., Davis, Calif. 95616
KANG, Cheul Weun, Westmar Col., Dept. of Econ. and Bus., Le Mars, Iowa 51031
KANG, Moon H., Univ. of Missouri at Rolla, Dept. of Soc. Sci., Rolla, Mo. 65401
KANGAS, J. Eugene, Western Michigan Univ., Kalamazoo, Mich.
KANIA, William Bernard, R.D. 1, Box 80, Uniontown, Pa. 15401

KANNAPPAN, Subbiah, Int. Labour Office, Int. Inst. for Labour Studies, Geneva, Switzerland
KANNAWAT, Chuay, Ministry of Foreign Affairs, Bangkok, Thailand
KANOVSKY, Eliyahu, 1340 53rd St., Brooklyn, N.Y. 11219
KANTER, Herschel Edward, 5726 N. 28th St., Arlington, Va. 22207
KANTNER, Arthur H., 808 Clemont Dr., N.E., Atlanta, Ga. 30306
KANTOR, Harry S., 2806 Woodstock Ave., Silver Spring, Md.
KAO, Charles Hsi Chung, Wisconsin State Univ., Soc. Sci. Div., River Falls, Wis. 54022
KAP-KYUNG, Seo, Univ. of Hawaii, Dept. of Econ. and Bus., Honolulu 14, Hawaii
KAPLAN, A. D. H., 4849 Langdrum Lane, Chevy Chase, Md. 20015
KAPLAN, Allan, 43-10 Kissena Blvd., Flushing 55, N.Y.
KAPLAN, Eugene J., 1936 Marthas Rd., Hollin Hills, Alexandria, Va. 22307
KAPLAN, Jacob J., 139 Grafton St., Chevy Chase, Md. 21620
KAPLAN, Lawrence J., 69-18 225th St., Bayside 64, N.Y.
KAPLAN, Marshall, 101 G St., S.W., Apt. 212, Washington, D.C. 20024
KAPLAN, Mortimer, 3720 39th St., N.W., Washington 16, D.C.
KAPLAN, Noah, 3900 16th St., N.W., Apt. 503, Washington, D.C. 20011
KAPLAN, Norman M., Univ. of Rochester, Col. of Arts, River Campus St., Rochester 20, N.Y.
KAPLAN, R., Cape Gate, Fence and Wire Works, 324 Voortrekker Rd., P.O. Box 17, Parow, Cape, South Africa
KAPLAN, Robert, 3601 Wisconsin Ave., N.W., Apt. 702, Washington, D.C. 20016
KAPP, Karl W., 93 Grellinger Strasse, Basel, Switzerland
KAPP, William R., Pfizer Int., Inc., 235 E. 42nd St., New York 17, N.Y.
KARAGIANNIS, John D., Southern Illinois Univ., Dept. of Econ., Carbondale, Ill. 62903
KARATZAS, George, Susquehanna Univ., Econ. Dept., Selinsgrove, Pa. 17870
KARCHERE, Alvin J., Woods Grove Rd., Westport, Conn.
KARCIC, Berislav, 520 E. 77th St., Apt. 928, New York 21, N.Y.
KARCZ, Jerzy Feliks, Univ. of California, Dept. of Econ., Santa Barbara, Calif. 93106
KARDASZ, Stanley W., 15 Nelson St., Kingston, Ont., Canada
KARDON, Donald R., 323 Old Gulph Rd., Haverford, Pa.
KARDOUCHE, George, Nat. Ind. Conf. Bd., 845 Third Ave., New York, N.Y. 10022
KAREKEN, John H., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
KARES, Peter, State Univ. of New York at Buffalo, Buffalo, N.Y.
KARFIOL, Rose, 912 E. Lee St., Greensboro, N.C. 27406
KARFUNKLE, Richard David, 1410 Fresno Rd., Green Acres, Wilmington, Del. 19803
KARG, Robert L., 125 Beech St., Pittsburgh, Pa. 15218
KARGER, Jack, 210 E. Hickory St., East Rochester, N.Y. 14445
KARKAR, Yaqub N., Univ. of Minnesota, Campus Club, Minneapolis, Minn. 55414
KARLIK, John R., 360 Riverside Dr., Apt. 13B, New York 25, N.Y.
KARP, Mark, Boston Univ., African Res. and Studies Program, 154 Bay State Rd., Boston 15, Mass.
KARPOFF, Edward, 4906 Bangor Dr., Kensington, Md. 20795
KARR, Gerald L., 507 Hancock, Warrensburg, Mo. 64093
KARSTEN, Karl, 7400 Bradley Blvd., Washington, D.C. 20034
KARSTEN, Siegfried G., 2626 Chadwick St., Salt Lake City, Utah 84106
KARTALIA, James, 1102 13th St., New Brighton, Pa. 15066
KARTMAN, Arthur E., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
KARUBIAN, John F., 201 S. Clark Dr., Beverly Hills, Calif.
KARVASARSKY, Arnold, 1609 Ocean Ave., Brooklyn, N.Y. 11230
KASCHINS, Edward A., Luther Col., Decorah, Iowa
KASER, Michael C., St. Antonys College, Oxford, England
KASHANIAN, Manouchehr, c/o Asia Ins. Co., Maidan Ferdowsi, Tehran, Iran
KASHIWAGI, Yusuke, Embassy of Japan, 2514 Mass. Ave., N.W., Washington 8, D.C.
KASHIWAZAKI, T., 995 Ikebukure 2-Chome, Toshima-Ku, Tokyo, Japan
KASNACICH-SCHMID, Johannes, Riesstrasse 72, Graz, Austria
KASOFF, Mark J., Indiana Univ., Dept. of Econ., Bloomington, Ind. 47405
KASS, David, 5966 Rock Hill Rd., Columbus, Ohio 43213
KASSING, David B., Center for Naval Analyses, 1401 Wilson Blvd., Arlington, Va. 22207
KASSOUF, Sheen T., 915 Ceris Pl., Newport Beach, Calif. 92660
KASTELANSKY, Grace S., 275 Seaman St., New Brunswick, N.J.
KASTNER, Harold H., Jr., 1811 5th St., S.E., Winter Haven, Fla. 33880
KASUN, (Mrs.) Jacqueline R., California State Col. at Fullerton, 800 N. State College Blvd., Fullerton, Calif. 92631
KATAYAMA, Sadao, 1-146 Tanaka, Motoyamacho, Higashinada-Ku, Kobe, Japan
KATKOFF, Vladimir, West Virginia Wesleyan Col., P.O. Box 82, Buckhannon, W. Va. 26201
KATONA, George, Univ. of Michigan, Survey Res. Center, Ann Arbor, Mich.
KATZ, Arnold M., 836 Tilden St. I-J, Bronx 67, N.Y.
KATZ, Benjamin J., 287 Beechwood Rd., Oradell, N.J. 07649
KATZ, Bernard S., RD 2, Box 256, Bolton, Conn. 06040
KATZ, David, Shderot Bialik, Shikunezrachi 18, Ramathasharon Israel
KATZ, Harold, 372 Central Park W., New York, N.Y. 11201
KATZ, Jerome, 336 Central Park W., New York 25, N.Y.
KATZ, Julius L., 4435 Springdale St., N.W., Washington 16, D.C.
KATZ, (Mrs.) Leone S., 1708 Bedford Rd., San Marino, Calif. 92069
KATZ, Lora, 255 W. 108th St., New York 25, N.Y.
KATZ, Myron, 10632 S.W. 55th Ave., Portland 19, Ore.
KATZ, Reuben J., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh 13, Pa.
KATZ, S. Stanley, 8315 Brook Lane, #707, Bethesda, Md. 20014
KATZ, Samuel, Fed. Res. Bd., Div. of Res. and Statis., Washington, D.C.
KATZ, Saul M., 1240 Malvern Ave., Pittsburgh 17, Pa.
KATZNER, Donald W., Univ. of Pennsylvania, Wharton Sch., Philadelphia, Pa. 19104
KAUCKI, Eugene F., 2339 W. Addison St., Chicago, Ill. 60618
KAUDER, Emil, Florida Presbyterian Col., St. Petersburg, Fla.
KAUFER, Erich, Gottfried Kellerstr. 19, 3550 Marburg/Lahn, Germany
KAUFFMAN, Kenneth Mark, American Embassy—AID, APO 675, New York, N.Y. 09675
KAUFLER, Sol D., 18309 Lahey St., Northridge, Calif.
KAUFMAN, (Mrs.) Edith R., 1036E Gorgas Lane, Philadelphia 50, Pa.
KAUFMAN, George C., Fed. Res. Bank of Chicago, Research Dept., Chicago, Ill. 60690
KAUFMAN, Henry, Salomon Bros. and Hutzler, 60 Wall St., New York 5, N.Y.
KAUFMAN, Hilary A., 1245 Fanshawe St., Philadelphia, Pa. 19111
KAUFMAN, Jacob J., Pennsylvania State Univ., Dept. of Econ., Boucke Bldg., State College, Pa. 16801
KAUFMAN, Richard H., 53 Woodcut Lane, New Rochelle, N.Y.
KAUFMAN, Robert M., 300 Park Ave., New York, N.Y. 10022
KAUFMANN, Hugo M., Franklin and Marshall Col., Lancaster, Pa.

KAUFMANN, Johan, 56 rue de Moillebeau, Geneva, Switzerland
KAUFMANN, Lynden J., 16814 St. Paul, Grosse Pointe, Mich. 48138
KAUFMANN, Thomas D., 48 Carlson Ave., Larchmont, N.Y. 10538
KAUN, David E., Stevenson Col., Univ. of California, Santa Cruz, Calif. 95060
KAUTZ, Robert W., Univ. of Saskatchewan, Dept. of Econ., Saskatoon, Sask., Canada
KAVALER, Vigdor W., 4905 5th Ave., Pittsburgh 13, Pa.
KAVANAGH, John M., Univ. of California, Inst. of Gov. and Public Affairs, Los Angeles, Calif. 90024
KAVESH, Robert A., 390 Highland Ave., Montclair, N.J.
KAWAGUCHI, Shinji, Osaka Univ., Dept. of Econ., Shibahara, Tokonaka, Osaka, Japan
KAWAKATSU, Shohei, Food and Agric. Org. of U.N., Viale Delle Terme di Caracalla, Rome, Italy
KAWAR, Suheil, 308 Westwood Plaza, Box 309, Los Angeles 24, Calif.
KAY, Bong H., Int. Monetary Fund, 19th and H Sts., N.W., Washington, D.C.
KAY, James R., Univ. of Texas, B.E.O.B. 111, Austin 12, Tex.
KAYE, Norman J., Marquette Univ., Col. of Bus. Admin., Milwaukee 3, Wis.
KAYSEN, Carl, Harvard Univ., Dept. of Econ., Littauer Center, Cambridge, Mass. 01922
KAZMI, Syed Shaukat, Vanderbilt Univ., Box 981, Sta. B, Nashville 5, Tenn.
KEAN, John H., 6218 Redwing Rd., Bethesda, Md 20034
KEAR, John C., 1100 6th St., S.W., Washington, D.C. 20024
KEARE, Douglas Hamilton, 14 E. Stanworth Dr., Princeton, N J. 08541
KEARL, C. D., American Farm Econ. Asso., Dept. of Agric. Econ., Cornell Univ., Ithaca, N.Y. 14850
KEAT, Paul G., IBM Corp., Old Orchard Rd., Armonk, N.Y. 10504
KEATINGE, Richard H., 458 S. Spring St., Los Angeles 13, Calif.
KEBKER, Vant W., Ohio Wesleyan Univ., Drawer 353, Delaware, Ohio 43015
KEDDIE, Wells H., 209 Engineering "E," University Park, Pa. 16802
KEE, Woo Sik, State Univ. of N.Y. at Stony Brook, Dept. of Econ., Stony Brook, L.I., N.Y. 11790
KEEGAN, Thomas F., 3809 Ridge Rd., Annandale, Fairfax Co., Va. 22003
KEEGAN, Warren J., 106 Larch Rd., Cambridge, Mass 02138
KEENA, Joseph W., 99 Park Ave., New York 16, N.Y.
KEENAN, W. Michael, Univ. of California, Sch. of Bus. Admin., Berkeley 20, Calif.
KEENY, Bob M., Univ. of Missouri at Kansas City, Sch. of Bus. Admin., Kansas City, Mo. 64110
KEESING, Donald B., 180 N. Balsamina Way, Menlo Park, Calif. 94026
KEEZER, Dexter M., McGraw-Hill, Inc., 330 W. 42nd St., New York, N.Y. 10036
KEHLENBECK, Henry G., 1015 Ackerman Ave., Syracuse 10, N.Y.
KEHOE, Joseph A., Univ. of Portland, Dept. of Soc. Sci., Portland 3, Ore.
KEHRER, (Mrs.) Helen H., 600 W. Gray St., Elmira, N.Y. 14905
KEHRER, Kenneth C., Elmira Col., Dept. of Econ., Elmira, N.Y. 14901
KEIG, Norman G., Univ. of Florida, Dept. of Econ., Gainesville, Fla.
KEIL, Jean, 28, Rue Jean-Jacques-Rousseau, Strasbourg (Bas-Rhin), France
KEIM, Walter G., 5006 Klingle St., N.W., Washington 16, D.C.
KEINTZ, Rita, 58 College Rd., Chestnut Hill, Mass. 02167
KEIPER, Joseph S., 17 Holden Lane, Madison, N.J. 07940
KEIR, Peter M., 1921 Marthas Rd., Alexandria, Va. 22307
KEISER, Norman F., 15400 Hume Dr., Saratoga, Calif. 95070
KEITH, E. Gordon, 434 Levering Mill Rd., Merion, Pa. 19066
KEITH, John H., 2629 College Ave., Berkeley, Calif. 94704
KEITHAHN, Rollo C., 680 E. State, Mason City, Iowa
KEKISH, Bohdan J., 222 E. 19th St., New York 3, N.Y.
KELCH, Paul, Wisconsin State Univ., Dept. of Econ., Stevens Point, Wis. 54481
KELEJIAN, Harry, 209 D Eagle Heights, Madison, Wis. 53705
KELLAR, I. Harold, Baruch Sch., City College, 17 Lexington Ave., New York 10, N.Y.
KELLER, Frank Leuer, Tulane Univ., Dept. of Econ., New Orleans, La. 70118
KELLEY, Allen C., Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison, Wis. 53706
KELLEY, George T., Univ. of Hartford, Sch. of Bus., Dept. of Econ., Hartford, Conn.
KELLEY, James W., Col. of Bus. Admin., Boston Univ., 685 Commonwealth Ave., Boston 15, Mass.
KELLEY, Paul A., 16 Pennacook St., Manchester, N.H. 03104
KELLEY, Paul L., 440 Westview Dr., Manhattan, Kans. 66502
KELLEY, Ralph W., 9809 26th Ave., Adelphi, Md. 21710
KELLEY, William T., Univ. of Pennsylvania, Wharton Sch., W210 Dietrich Hall, Philadelphia, Pa. 19104
KELLOGG, Lester S., Steepmeadow, Rock Island, Ill. 61201
KELLY, Charles A., 516 Fieldstone Rd., Monroeville, Pa. 18832
KELLY, Eamon Michel, Pennsylvania State Univ., 120 Boucke Bldg., State College, Pa.
KELLY, Erwin Lewis, Jr., Antioch Col., Dept. of Econ., Yellow Springs, Ohio 45387
KELLY, Jerry S., 625 Main St., Reading, Mass. 01867
KELLY, John C., 11 Albie Dr., Hyde Park, N.Y. 12538
KELLY, Michael G., 5020 Doherty Ave., Montreal 29, P.Q., Canada
KELLY, Robert W., USOM/American Embassy, Saigon, South Vietnam, San Francisco, Calif. 96240
KELLY, Thomas A., 2514 McRae Rd., Bon Air, Va 23235
KELLY, Thomas H., Univ. of Natal, P.O. Box 1525, Durban, South Africa
KELLY, William B., Jr., U.S. Mission, K R Delegation, 80 Rue de Lausanne, Geneva, Switzerland
KELLY, William J., 12426 Richmond St., Chester, Va. 23831
KELTY, Paul L., 1215 N. Ft. Myer Dr., Arlington, Va.
KEMMERER, Donald L., 1006 W. Armory Ave., Champaign, Ill.
KEMP, Arthur, 1541 Lafayette Rd., Claremont, Calif
KEMP, Bernard A., 3403 Cypress Dr., Falls Church, Va. 22042
KEMP, William B., 75, Hornfair Rd., Charlton, London, S.E. 7, England
KEMPE, Richard J., 200 E. 16th St., New York, N.Y. 10003
KEMPER, Armand, 2616 Talnot Rd., Baltimore 16, Md.
KEMPEY, William M., 838 52nd St., Brooklyn 20, N.Y.
KEMPFF-MERCADO, Rolando, Casilla 1283, La Paz, Bolivia
KEMPSTER, H. G., Jr., Willamette Tug-Barge, Foot of N. Portsmouth Ave., Portland 3, Ore.
KEMPSTER, John H., 30 Westland Rd., Weston, Mass. 01985
KENADJIAN, Berdj, 910 Crescent Dr., Alexandria, Va. 22302
KENDALL, Leon T., New York Stock Exchange, 11 Wall St., New York, N.Y. 10005
KENDALL, William N. W., 3719 Brandywine St., Washington 16, D.C.
KENDE, George, 2315 Poplar Ave., Evanston, Ill. 60201
KENDRICK, David A., 35 Lee St., #15, Cambridge, Mass. 02139
KENDRICK, David D., 1297 Highland Dr., Moscow, Idaho

KENDRICK, James G., Univ. of Nebraska, Dept. of Agric. Econ., Lincoln, Neb.
KENDRICK, John W., Univ. of Connecticut, Dept. of Econ., Storrs, Conn. 06268
KENDRICK, M. Slade, 225 Willard Way, Ithaca, N.Y.
KENEN, Peter Bain, Columbia Univ., Dept. of Econ., 718 Hamilton Hall, New York 27, N.Y.
KENESTRICK, Jean, 1551 Doone Rd., Columbus 21, Ohio
KENNEDY, Charles J., 5919 Cullen Dr., Lincoln, Neb. 68506
KENNEDY, Donald D., 4301 Massachusetts Ave., Washington 16, D.C.
KENNEDY, Francis J., 306 Dunboyne Pl., Thibodaux, La. 70301
KENNEDY, Jane M., 3519 Link Valley, #47, Houston, Tex. 77025
KENNEDY, Jay B., Box 2211, c/o Aramco, Dhahran, Saudi Arabia
KENNEDY, John W., Univ. of North Carolina at Greensboro, Greensboro, N.C.
KENNEDY, Joseph B., 1735 K St., N.W., Suite 508, Washington, D.C. 20006
KENNEDY, Kieran A., 8 Upper Pembroke St., Dublin, Ireland
KENNEDY, Miles, c/o Ford Found., 8, Rowdon St., Calcutta-16, India
KENNEDY, Russel E., Asesores de Pensiones, S.A., Av. Insurgentes Sur 11-50 Piso, Mexico 6, D.F., Mexico
KENNEDY, William F., Univ. of California, Santa Barbara, Calif. 93106
KENNERLY, Craig T., 2221 Elliston Pl., Apt. 8, Nashville, Tenn. 37203
KENNEY, James E., 247 Harwood Ave., Syracuse 3, N.Y.
KENT, Leonard, Univ. of Illinois, Chicago Circle, 2526 University Hall, Chicago, Ill. 60680
KENT, Raymond P., Univ. of Notre Dame, Col. of Bus. Admin., Notre Dame, Ind. 46556
KENWORTHY, Max R., Hq. 3rd Brigade, 3rd Infantry Div., APO New York 09162
KEOWN, William H., Univ. of Oklahoma, Dept. of Bus. Mgt., Norman, Okla.
KERAN, Michael W., Fed. Res. Bank of St. Louis, P.O. Box 442, St. Louis, Mo. 63166
KERBY, William C., Portland State Col., P.O. Box 751, Portland, Ore. 97207
KERCHNER, Robert B., Room 214, B. and P.A., Columbia, Mo.
KEREKES, Gabriel T., c/o Goodbody and Co., 2 Broadway, New York 3, N.Y.
KEREN, Michael, 11 Lake Pl., New Haven, Conn.
KERKOVICH, Thomas M., 182 Chestnut St., Brooklyn 8, N.Y.
KERLEY, David Moorman, 720 Eden Ter., Rock Hill, S.C. 29730
KERLIN, Julie O., 1114 Shipman Lane, McLean, Va. 22101
KERMANI, Taghi, 4871 Sampson Rd., Youngstown 5, Ohio
KERN, Edward D., Jr., Auburn Univ., Dept. of Agric. Econ., Auburn, Ala. 36830
KERR, Clark, 8300 Buckingham Dr., El Cerrito, Calif.
KERR, Erik S., 131 Notch Hill Rd., Apt. 202, Kingston, Ont., Canada
KERR, John C., Jr., 10 Duryea Rd., Montclair, N.J.
KERRIDGE, Isaac C., Jr., 2440 Inwood Dr., Houston, Tex. 77019
KERRY, Albert S., Jr., 422 Medical Arts Bldg., Seattle, Wash.
KERSHAW, Joseph A., Calvert House, Apt. 325, 2401 Calvert St., N.W., Washington, D.C. 20008
KERSTENETZKY, Isaac, Caixa Postal #4081, Rio de Janciro 8b, Brazil
KERTON, Robert R., Duke Univ., Dept. of Econ., Durham, N.C. 27706
KESSEL, Rueben A., Univ. of Chicago, Sch. of Bus., Chicago, Ill. 60637
KESSLER, A., P.O. Box 981, Jerusalem, Israel
KESSLER, Hans T., 845 West End Ave., Apt. 12E, New York 25, N.Y.
KESSLER, Martin, 11 Riverside Dr., New York, N.Y.
KESTER, Henry I., Sch. of Bus., Univ. of Colorado, Boulder, Colo.
KESTER, William H., 8131 Roxburgh Dr., St. Louis 5, Mo.
KESTLE, Wendell Russell, 1441 E. Abila St., Torrance, Calif. 90502
KETCHUM, Marshall Dana, Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
KETTERLING, Virgil H., 833 S. 20th St., Arlington, Va. 22202
KETTLER, Weldon D., 5810 Capello, Houston, Tex. 77035
KETTUNEN, Lauri, Maat. Tal. Tutkimuslaitos, Helsinki-Rukkila, Finland
KEY, Euell Dwayne, Texas Tech. Col., Dept. of Econ., Lubbock, Tex. 79409
KEYES, (Mrs.) Lucile S., 2605 31st St., N.W., Washington 8, D.C.
KEYES, Robert W., Western Canada Steel, Ltd., 450 S.E. Marine Dr., Vancouver 15, B.C., Canada
KEYSERLING, Leon H., 1001 Connecticut Ave., N.W., Washington, D.C. 20036
KEYSERLING, (Mrs.) Mary Dublin, 1001 Connecticut Ave., N.W., Washington, D.C. 20036
KEYSERLING, Michael S., USS Chipola (AO-63), c/o Fleet Post Office, San Francisco, Calif.
KHACTU, Dominic N., Univ. of North Dakota, Dept. of Econ., Grand Forks, N.D.
KHAN, Abdul Ghafoor, Central Statistical Office, Karachi, Pakistan
KHAN, Agha N. A., North Carolina State Univ., Dept. of Agric. Econ., Raleigh, N.C. 27607
KHAN, Akhtar H., Board of Governors, Pakistan Rural Dev. Academy, Comilla, East Pakistan
KHAN, C. Rafiq A., Central Mortgage and Housing Corp., Econ. Res. Dept., Head Office, Ottawa 7, Ont., Canada
KHAN, Eaqub Ali, East Pakistan Bur. of Statis., Eden Bldgs., Dacca, Pakistan
KHASIGIAN, Amos, 7625 W. 92nd St., Los Angeles, Calif. 90045
KHATCHADOURIAN, George M., 4570 MacArthur Blvd., N.W., Washington, D.C.
KHAZZOOM, Daniel, New York Univ., Grad. Sch. of Arts and Sci., Dept. of Econ., Washington Sq., New York, N.Y.
KHEIN-EL-DINE, Hanaa A., 315-B Ashdown House, 305 Memorial Dr., Cambridge, Mass. 02139
KHLENTZOS, Michael T., 11 Denslowe Dr., San Francisco, Calif. 94132
KHOSLA, Sukh Dev, 3709 Chestnut St., Philadelphia, Pa. 19104
KHOT, Sid M., 102, IV Main, Malleswaram, Bangalore 3, India
KHOUJA, Mohamad W., 2912 Claremont Ave., #31, Berkeley, Calif. 94705
KHOURY, Joseph E., 145 Orient Way, Rutherford, N.J.
KI, Lee Sun, Project Management Div., Econ. Planning Bd., Seoul, Korea
KICHLINE, James L., 6413 62nd Pl., Riverdale, Md. 20840
KIDD, Larry G., Phildo Corp.—WDL, 3825 Fabian Way, Palo Alto, Calif. 94303
KIDDER, (Mrs.) Alice H., 20 Pearl St., Sommerville, Mass. 02145
KIDDER, David E., 20 Pearl St., Sommerville, Mass. 02145
KIDDER, Frederick Elwyn, Apartado 3294, Mayaguez, P.R. 00708
KIDNEY, (Mrs.) Juliet F., 7315 Summit Ave., Chevy Chase, Md. 20015
KIEFER, Alexander F., 6711 Brigadoon D., Bethesda, Md. 20034
KIEFER, Maxine E., 5302 18th St. N., Arlington 5, Va.
KIEFER, Renata G., Univ. of California, Dept. of Econ., Berkeley, Calif. 94720
KIEFERLE, Hans J., Asso. of Canadian Med. Cols., 75 Albert, Ottawa 4, Ont., Canada
KIEFFER, Richard J., 619 Kuhlman Ct., Columbia, Mo.
KIEKE, Kermit A., Texaco, Inc., P.O. Box 1045, Oklahoma City, Okla.
KIEN, Tran Van, Dir. of Cabinet, Ministry of Nat. Educa., 64, Le Thanh Ton, Saigon, Vietnam
KIERANS, Eric W., 165 Grand-Allee East, Apt. 1103, Quebec, P.Q., Canada
KIERNAN, Charles J., 14 Fairbanks St., Hillside, N.J. 07642

KIEVAT, Irene M., Michigan State Univ., Dept. of Econ., East Lansing, Mich.

KIHLSTROM, Richard E., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455

KI-HYUK, Park, Yonsei University, Seoul, Korea

KIKUCHI, Susumu, Okayama Univ., Sch. of Engineering, Tsushima, Okayama, Japan

KIKUKAWA, Hiroshi, Ryukyu Noren, 284, Kohagura, Naha, Okinawa

KILBRIDE, Bernard J., Univ. of Notre Dame, P.O. 161, Notre Dame, Ind. 46556

KILBRIDE, Wade R., Qtrs. 6302A, U.S. Air Force Academy, Colo. 80840

KILBY, Edwin L., Jr., 1222 Merchant Lane, McLean, Va. 22101

KILDEBECK, James, 212 Ancha Vista Dr., Torrington, Wyo.

KILDOYLE, Patrick P., 42 Bank St., New York 14, N.Y.

KILEY, Edward V., 1616 P St., N.W., Washington, D.C. 20036

KILIAN, Donald J., 429 Stockton Ave., Roselle, N.J. 07068

KILICER, Ahmet Z., Selanik Caddesi 7/13, Ankara (Y-8), Turkey

KILLEEN, John F., Loyola Univ., 7101 W. 80th St., Los Angeles 45, Calif.

KILLINGSWORTH, Charles C., Michigan State Univ., Sch. of Labor and Ind. Rela., East Lansing, Mich.

KILLOUGH, Hugh B., 515 Krueger Pkwy., Stuart, Fla. 33494

KILLOUGH, (Mrs.) Lucy W., 515 Krueger Pkwy., Stuart, Fla. 33494

KILMER, Harlan N., 1735 Highland Pl., Berkeley, Calif.

KILPATRICK, Robert W., Cornell Univ., Dept. of Econ., Ithaca, N.Y.

KIM, Chaiho, Univ. of Santa Clara, Sch. of Bus., Santa Clara, Calif.

KIM, Choon, 365 Main St., Amherst, Mass. 01002

KIM, Dal Hyun, Kyuny Hee Univ., Econ. Dept., # San 4, Hoiki-dong, Dongdaimum-Ku, Seoul, Korea

KIM, Dong Soo, Bank of Korea, Res. Dept., Seoul, Korea

KIM, Hak-Yul, 10-21 Hewha Dong, Chong Ro Ku, Seoul, Korea

KIM, Hyong Chun, San Fernando Valley State Col., Dept. of Econ. and Statis., Northridge, Calif.

KIM, Hyung K., Univ. of Minnesota, Dept. of Bus. and Econ., Duluth, Minn. 55812

KIM, In Su, San 9—4 Bon-Dong, Yongdungpo-Ku, Seoul, Korea

KIM, Jai Man, Kansas Wesleyan Univ., Div. of Soc. Sci., Salina, Kansas

KIM, Key Whan, 2141 Oregon St., Berkeley, Calif. 94705

KIM, Ki Hoon, Univ. of Connecticut, Dept. of Econ., Storrs, Conn. 06268

KIM, Kye Chil, 50 Fremont St., Bridgeport, Conn 06605

KIM, Seung H., 1 Washington Square Village, New York, N.Y. 10012

KIM, Shinyung, 236 Park Pl., Des Moines, Iowa 50312

KIM, Soo-Keun, Vanderbilt Univ., Box 1130, Sta. B, Nashville, Tenn. 37203

KIM, Sung J., Univ. of Pennsylvania, Wharton Sch., Dept. of Regional Sci., Philadelphia 4, Pa.

KIM, Sung Woo, #633 International House, Berkeley 4, Calif.

KIM, Tae Won, 1035 E. College, Iowa City, Iowa

KIM, Young Kyun, Economic Planning Bd., Seoul, Korea

KIMBALL, Milo, 43 Mercury Ct., West Springfield, Mass. 01089

KIMBELL, Larry J., 5600 Exeter, Austin, Tex. 78723

KIMZEY, Bruce W., 48-C N. Fairway, Pullman, Wash. 99163

KINCAID, Randall R., Jr., 861 Louise Circle, Durham, N.C. 27705

KINDAHL, James K., 5550 Blackstone Ave., Chicago, Ill. 60637

KINDLEBERGER, Charles P., Massachusetts Inst. of Tech., Cambridge, Mass. 01922

KING, Alfred M., Walker Mfg. Co., Racine, Wis.

KING, Boyd F., 4021 Benton St., N.W., Apt. 304, Washington, D.C. 20007

KING, Clay B., Chico State Col., Dept. of Econ., Chico, Calif. 95927

KING, David A., Univ. of Arizona, Dept. of Watershed Mgt., Tucson, Ariz. 85721

KING, Donald A., Col. of the Holy Cross, Dept. of Econ., Worcester, Mass. 01610

KING, Donald F., Philadelphia Electric Co., 211 S. Broad St., Philadelphia 5, Pa.

KING, Edmund R., Eastman Kodak Co., 343 State St., Rochester, N.Y. 14650

KING, Frank Henry Haviland, 1408 Kentucky St., Lawrence, Kans. 66044

KING, John L., 635 Glendale St., Carlisle, Pa. 17013

KING, (Mrs.) Katherine A., 635 S. H St., Lakeview, Ore. 97630

KING, Martin L., American Univ., Sch. of Bus. Admin., Washington, D.C. 20016

KING, Richard A., North Carolina State Col., Dept. of Agric. Econ., Raleigh, N.C.

KING, Richard C., 2330 Euclid Heights Blvd., Cleveland Heights 6, Ohio

KING, Richard T., 6032 S. Blackstone Ave., Chicago, Ill. 60637

KING, Robert Wayne, 4109 Byrd Ct., Kensington, Md. 20795

KING, Stephen A., 3813-B Victory Blvd., Burbank, Calif

KING, Timothy, Queen's Col., Cambridge, England

KINGKADE, William Bruce, 48 Iselin Ter., Larchmont, N.Y. 10538

KINGSBURY, Kenneth M., U.S. Dept. of Labor, 2200 Federal Office Bldg., Kansas City, Mo. 64106

KINGSTON, Jerry L., Pennsylvania State Univ., Dept. of Econ., University Park, Pa.

KINNARD, William N., Jr., Univ. of Connecticut, Sch. of Bus. Admin., Bus. Dept., Storrs, Conn. 06268

KINNE, R. Duggan, 6285 N.E. Radford Dr., Seattle, Wash.

KINNEY, (Mrs.) Ann R., 1231 31st St., N.W., Washington, D.C. 20007

KINOSHITA, Kazuo, 61 Nango-cho, Nishinomiya, Hyogo-Ken, Japan

KINSELLA, Thomas J., 57 Union Street, Apt. 2, Montclair, N.J. 07042

KINSEY, Robert K., 2000 F. St., N.W., Washington 6, D.C.

KIPNIS, Gregory M., 290 Riverside Dr., New York, N.Y. 10025

KIRBY, E. Stuart, Col. of Advanced Tech., Gosta Green, Birmingham 4, England

KIRBY, Frederick C., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202

KIRBY, Frederick M., 2400 Queenston Rd., Cleveland, Ohio 44118

KIRBY, Glenn, Jr., Southern Methodist Univ., Dept. of Econ., Dallas 22, Tex.

KIRCHER, Harry B., 348 Sunset Dr., Edwardsville, Ill. 62025

KIRCHNER, Lucille, 3435 Sciotangy Dr., Columbus, Ohio 43221

KIRK, Robert J., 1202 Scott, Ames, Iowa

KIRK, Roy V., Rensselaer Poly. Inst., Sch. of Mgt., Troy, N.Y.

KIRK, Stuart C., 1111 N. Dartmouth, No. 204, Claremont, Calif. 91711

KIRKPATRICK, Arthur L., Univ. of Hawaii, Col. of Bus., Dept. of Bus. Econ., Honolulu, Hawaii 96822

KIRKPATRICK, Forrest H., Wheeling Steel Corp., Wheeling, W. Va.

KIRKPATRICK, (Mrs.) Nora B., Univ. of Hawaii, Col. of Bus., Dept. of Bus. Econ., Honolulu, Hawaii 96822

KIRKPATRICK, Thomas Owen, 2719 June, N.E., Albuquerque, N.M. 87112

KIRSCH, Arthur J., California State Col. at Los Angeles, Dept. of Econ., Los Angeles 32, Calif.

KIRSCH, Harry Daniel, 135-37 225th St., Laurelton 13, N.Y.

KIRSCH, Leonard, Harv. Univ., Russian Res. Center, 1737 Cambridge St., Cambridge, Mass. 01922

KIRSHEN, Himy B., Univ. of Massachusetts, Sch. of Bus. Admin., Amherst, Mass.
KIRSHENBAUM, Paul S., 723 Crown St., Brooklyn 13, N.Y.
KIRZNER, Israel M., 699 Montgomery St., Brooklyn 13, N.Y.
KISCH, Herbert, Michigan State Univ., Dept. of Econ., East Lansing, Mich.
KISHI, Gloria E., State Univ. Col., Potsdam, N.Y. 13676
KISOR, Manown, Jr., Standard Statistics Co., 345 Hudson St., New York 14, N.Y.
KISSELGOFF, Avram V., 601 W. 113th St., New York 25, N.Y.
KITAMURA, Hiroshi, Econ. Comm. for Asia and Far East, U.N., Santitham Hall, Rajadamnern Ave., Bangkok, Thailand
KITCHELL, Eunice, Trinity Univ., Dept. of Econ., San Antonio 12, Tex.
KITCHEN, Harry M., 7 Osler Ct., Apt. 201, Dunas, Ont., Canada
KITCHEN, James E., Waterloo Lutheran Univ., Econ. Dept., Waterloo, Ont., Canada
KITTLESON, Bob R., Div. of Soc. Sci., Humboldt State Col., Arcata, Calif. 95521
KITTREDGE, Edmund W., Ministerial Rd., Wakefield, R.I.
KITTRELL, Edward R., Northern Illinois Univ., Dept. of Econ., DeKalb, Ill. 60115
KJELLBERG, Sten C., Toronto-Dominion Bank, Dept. of Econ. Res., King and Yonge Sts., Toronto, Ont., Canada
KLAASEN, Adrian J., Hope Col., Dept. of Econ. and Bus. Admin., Holland, Mich.
KLAASEN, Thomas A., 563 Lawndale Ct., Holland, Mich.
KLABSUBA, Richard Wesley, 2240 Lake St., San Francisco, Calif. 94121
KLAMAN, Saul B., Nat. Asso. of Mutual Savings Banks, 200 Park Ave., New York 17, N.Y.
KLANG, Goodman L., Kingsborough Com. Col. of City Univ. of New York, Econ. Dept., Oriental Blvd., Manhattan Beach, Brooklyn, N.Y. 11235
KLAPPHOLZ, Kurt, London Sch. of Econ., Houghton St., London, W.C. 2, England
KLARAGARD, Sever, Mapes, N.D. 58349
KLARMAN, Herbert E., Johns Hopkins University, Sch. of Hygiene and Health, 615 N. Wolfe St., Baltimore 5, Md.
KLASSON, Charles R., Univ. of Texas, Col. of Bus. Admin., Dept. of Mgt., Austin, Tex.
KLAUCKE, Walter R., 206 W. 21st St., New York, N.Y. 10011
KLAYMAN, Maxwell I., 6351 Phillips Ave., Pittsburgh, Pa. 15217
KLEBANER, Benjamin J., 322 Central Park W., New York, N.Y. 10025
KLEEMAN, M. R., 21 Rosemere Ave., West Caldwell, N.J. 07006
KLEIGER, Linda, 15145 Dickens St., Apt. 4, Sherman Oaks, Calif. 95681
KLEIN, J. Walter, St. Francis Col., Loretto, Pa. 15940
KLEIN, John J., Fordham Univ., Grad. Sch. Arts and Sci., New York 58, N.Y.
KLEIN, Joseph Jerome, 60 E. 42nd St., New York, N.Y. 10017
KLEIN, (Mrs.) Judith L. V., 4008 Ingersol Dr., Silver Spring, Md. 20902
KLEIN, Lawrence R., 1317 Medford Rd., Wynnewood, Pa. 19096
KLEIN, Michael A., 6821 Sheridan Rd., Apt. 601, Chicago, Ill. 60626
KLEIN, Philip A., Pennsylvania State Univ., 120 Boucke Bldg., University Park, Pa. 16802
KLEIN, Richard E., 3332 Broadmore Dr., Bay City, Mich.
KLEIN, Robert M., P.O. Box 6113, Alexandria, Va. 22306
KLEIN, Roger, Michigan State Univ., Dept. of Econ., Marshall Hall, East Lansing, Mich.
KLEIN, Rudolf J., 280 Riverside Dr., New York, N.Y.
KLEIN, Samuel N., 5528 29th St., N.W., Washington 15, D.C.
KLEIN, Sidney, c/o Asia Found., No. 11 Alley 5, Lane 1274, Chung Cheng Lu, Taipei, Taiwan
KLEIN, (Mrs.) Sonia A., 1317 Medford Rd., Wynnewood, Pa. 19096
KLEIN, Thomas A., 2221 Densmore Dr., Toledo, Ohio 43606
KLEIN, Thomas Martin, 4008 Ingersol Dr., Silver Spring, Md. 20902
KLEIN, Walter H., Villanova Univ., Villanova, Pa. 19085
KLEINBLOESEM, Cornelis H., c/o Dept. of Commerce, Rotterdam Port Authority, 3 Redeerijstraat, Rotterdam 1, The Netherlands
KLEINE, Herman, 1301 S. Scott St., Arlington, Va.
KLEINMAN, David C., Ford Motor Co., Econ. Office, The American Rd., Dearborn 8, Mich.
KLEINMAN, David T., 319 W. 105th St., Apt. 3C, New York, N.Y. 10025
KLEINSORGE, Paul L., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
KLEMME, Randall T., Northern Natural Gas Co., 2223 Dodge St., Omaha 2, Neb.
KLEVORICK, Alvin Keith, Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
KLIMAN, M. L., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
KLINE, Donald S., 4 Second St., Delhi, N.Y. 13753
KLINE, Sam A., 2606 Highland Ave., Santa Monica, Calif. 90405
KLING, Richard H., 311 N. Thomas, #3, Arlington, Va. 22203
KLINGAMAN, David C., 1619 Greenleaf Lane, Charlottesville, Va. 22903
KLINGMAN, (Mrs.) Jane S., Univ. of Wisconsin, Sch. of Com., Madison, Wis. 53706
KLINKMULLER, Erich, Potsdamerstrasse 10 B, Berlin—Lichterfelde, West Germany
KLISE, Eugene S., 208 Oakhill Dr., Oxford, Ohio 45056
KLOCK, David J., 1425 Clifton Park Rd., Schenectady, N.Y. 12309
KLONSKY, Herman, Long Island Univ., Brooklyn 1, N.Y.
KLONTZ, Harold E., Auburn Univ., Dept. of Econ., Auburn, Ala. 36830
KLOPSTOCK, Fred H., 33 Liberty St., New York, N.Y.
KLOS, Joseph James, Oklahoma State Univ., Econ. Dept., Stillwater, Okla.
KLOSE, Gil, Earlham Col., Dept. of Econ., Richmond, Ind.
KLOSS, John, 9131 Wayburn, Detroit 24, Mich.
KMENTA, Jan, Michigan State Univ., Dept. of Econ., East Lansing, Mich.
KMITCH, Francis A., Kansas State Teachers Col., Div. of Soc. Sci., Emporia, Kans. 66802
KNAPP, John L., 106-B Sunset Ave., Charlottesville, Va. 22903
KNAPP, Robert W., Univ. of Calif., Grad. Sch. of Bus. Admin., Los Angeles, Calif. 90024
KNAPPEN, Laurence S., 2512 Ft. Scott Dr., Arlington, Va.
KNAUERHASE, Ramon, Univ. of Connecticut, Dept. of Econ., U-63, Storrs, Conn. 06268
KNEAFSEY, James T., 2201 Dorset Rd., Columbus, Ohio 43221
KNEESE, Allen V., Resources for the Future, Inc., 1755 Massachusetts Ave., N.W., Washington, D.C.
KNETSCH, Jack L., Resources for the Future, Inc., 1755 Massachusetts Ave., N.W., Washington 36, D.C.
KNIFFIN, David L., 215 S. 46th St., Philadelphia 39, Pa.
KNIFFIN, Fred W., 733 Holmes St., State College, Pa. 16801
KNIGHT, Edward, 1605 Oakcrest Dr., Alexandria, Va.
KNIGHT, Frank H., Univ. of Chicago, Chicago, Ill.
KNIGHT, Geraldine P., 1801 Ave. of the Stars, Century City-Gateway West, Los Angeles, Calif.
KNIGHT, Harry R., 190 Loomis St., Burlington, Vt. 05401
KNIGHT, Melvin M., Univ. of California, Dept. of Econ., Berkeley 4, Calif.
KNIGHT, Peter T., 827 University Ave., Palo Alto, Calif. 94301
KNIGHT, Richard, Columbia Univ., Conservation of Human Resources, 413 W. 117th St., New York, N.Y.
KNIGHT, Robert E., 81 Garfield St., Cambridge, Mass. 02138
KNIGHT, Robert E. L., Univ. of Maryland, Dept. of Econ., College Park, Md.

KNIGHT, W. R., Georgia State Col., 33 Gilmer St. S.E., Atlanta 3, Ga.
KNIPE, James L., P.O. Box 9907, Washington, D.C. 20015
KNOLL, Marjorie M., Pennsylvania State Univ., Col. of Home Econ., University Park, Pa. 16802
KNOLLMEYER, Lewis E., 12433-B Turner Circle, Omaha, Neb. 68123
KNOOP, Sister Catherine T., Mount St. Mary Col., 12001 Chalon Rd., Los Angeles 49, Calif.
KNOOP, Henri, Lovanium Univ., B.P. 833, Leopoldville XI, Congo Republic
KNOPF, Henry A., Niagara Univ., Niagara University, N.Y. 14109
KNOPF, Kenyon Alfred, Grinnell Col., Dept. of Econ., Grinnell, Iowa 50112
KNORR, K. E., Center of Int. Studies, Princeton Univ., Princeton, N.J. 08540
KNORR, Philip N., Univ. of Arizona, Dept. of Watershed Mgt., Col. of Agric., Tucson, Ariz.
KNOTTS, Ulysses S., Jr., 502 S. 51st St., Omaha, Neb.
KNOWLES, James W., 6005 Berkshire Dr., Bethesda, Md. 21811
KNOWLTON, Stuart L., Ford Motor Co., The American Rd., Dearborn, Mich.
KNOWLTON, William A., OSD Spec. Asst., 3E941 Pentagon, Washington, D.C. 20301
KNOX, Frank A., Queen's Univ., Dept. of Econ., Kingston, Ont., Canada
KNOX, Hugh W., 14 Bradley Rd., Chapel Hill, N.C.
KNOX, Robert L., Arizona State Univ., Dept. of Econ., Tempe, Ariz. 85281
KNUDSEN, John W., Univ. of Minnesota, Dept. of Econ., B.A. Tower, Minneapolis, Minn.
KNUDSON, Edward C., 508 Lee Street, Evanston, Ill.
KNUSEL, Jack, 951 W. Charles St., Whitewater, Wis. 53190
KNUTSON, Ronald S., 5222 S.E. Center St., Portland, Ore.
KOBAYASHI, Kenzo, 5-5 Kita Uneo, 2-chome, Taito-ku, Tokyo, Japan
KOBRAK, (Mrs.) Marie, 983 E. Howard, Pasadena, Calif 91104
KOBROCK, John P., 43 Tow Ponds Rd., Green Gate, Falmouth, Mass. 02540
KOCH, Albert R., Bd. of Gov. of Fed. Res. System, Washington, D.C. 20551
KOCH, Carl G., Wisconsin Tel. Co., Room 1306, 722 N. Broadway, Milwaukee, Wis. 53202
KOCH, James V., 610 E. Harrison St., Morton, Ill. 61550
KOCH, Jean-Ellen D., Johnson Hall, Room 821, 411 W. 116th St., New York, N.Y.
KOCH, John Brandhorst, Illinois Col., Jacksonville, Ill. 62650
KOCHEVAR, (Mrs.) Catherine H., 6109-A Averill Way, Dallas, Tex. 75225
KODERA, Takeshir, 4, Sowa-Cho 3-Chome, Nada-Ku, Kobe, Japan
KOEFOD, Paul E., Univ. of Florida, Dept. of Econ., Matherly Hall, Gainesville, Fla.
KOEHLER, John E., 98 Edwards St., Apt. 2-B, New Haven, Conn. 06511
KOEN, Suryatmodjo, 2741 Yale Sta., New Haven, Conn.
KOENIG, Charles H., Jr., c/o A. T. Kearney Co., 135 S. La Salle St., Chicago, Ill. 60603
KOENIG, Herman J., 6600 Jerry Pl., McLean, Va. 22101
KOENIG, Stanley, 2912 Brighton 12th St., Brooklyn 35, N.Y.
KOENIGSBERG, Lee, 14 Karen Pl., Metuchen, N.J.
KOENKER, W. E., Univ. of North Dakota, Univ. Sta., Grand Forks, N.D.
KOFFSKY, Nathan M., 5515 Greystone St., Chevy Chase, Md. 20015
KOGIKU, K. C., 2017 Delaware St., Berkeley, Calif. 94709
KOH, Clifford U., 2012 N. Daniel St., Arlington, Va. 22201
KOHEN, Andrew I., 34 Orchard Pl., New Haven, Conn. 06511
KOHLER, Heinz, R.F.D. 2, Amherst, Mass. 01002
KOHLER, Maynard R., Jr., 601 Kennard Rd., State College, Pa. 16801
KOHLMEIER, John M., Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
KOHN, Donald L., 910 S. Forest, Apt. 4, Ann Arbor, Mich. 48104
KOHN, Ernest, 253-04 61st Ave., Little Neck, Queens, N.Y. 14754
KOHN, Martin J., 262 Willow St., New Haven, Conn.
KOHN, Stephen J., 77 Fir Hill, Apt. 8B5, Akron, Ohio 44304
KOHN, Theodore F., 4956 McAnulty Rd., Pittsburgh, Pa. 15256
KOHNSTAMM, Victor R., Apdo. No. 15-070, Mexico 15, D.F., Mexico
KOKAT, Robert G., 13101 Taney Dr., Beltsville, Md. 20705
KOKLER, Bruce R., 162 Panoramic Way, Berkeley, Calif.
KOKORIS, James A., 3028 N. New England Ave., Chicago, Ill. 60634
KOLBE, Frank F., 40 Indian Hill Rd., Winnetka, Ill. 60093
KOLDE, Endel J., Univ. of Washington, Col. of Bus. Admin, Seattle, Wash. 98105
KOLIN, Marshall, New York Univ., Grad. Sch. of Bus. Admin., 100 Trinity Pl., New York 6, N.Y.
KOLINSKI, Ralph N., 2133 E. Grand Blvd., Detroit, Mich. 48211
KOLKER, Berndt L., 5222 Braesheather Dr., Houston, Tex. 77035
KOLLAI, Helmut R., 7553 Cromwell Dr., Clayton, Mo. 63105
KOLLER, E. Fred, Univ. Farm, Univ. of Minnesota, St. Paul, Minn. 55101
KOLLER, Walter L., Bv. Orono 560, Rosario, Argentina
KOLLITUS, Constantine, 30-21 30th Ave., Astoria, N.Y. 14710
KOLMIN, Frank W., State Univ. of New York at Albany, Albany, N.Y. 12203
KOLODNY, Saul, Amer. Sugar Ref. Co., 120 Wall St., New York 5, N.Y.
KOLODRUBETZ, Walter W., 9802 Cottrell Ter., Silver Spring, Md. 20903
KOMARYNSKY, Jaroslaw, 300 S. Goodwin Ave., Apt. 501, Urbana, Ill.
KOMINUS, Nicholas, 1001 Connecticut Ave., N.W., Washington 36, D.C.
KOMIVES, John L., Kalamazoo Col., Kalamazoo, Mich. 49001
KOMIYA, Ryutaro, Univ. of Tokyo, Faculty of Econ., Hongo, Tokyo, Japan
KONCZACKI, Z. A., Univ of Alberta, Dept. of Econ., Edmonton, Alta., Canada
KONDONASSIS, Alexander J., 61 Amarillidos, Psychico, Athens, Greece
KONICEK, Richard R., Iowa Dev. Comm., 250 Jewett Bldg., Des Moines, Iowa 50309
KONIG, Heinz, Institut fur Volkswirtschaft, Schloss Ostflugel, Manheim, Germany
KONIJN, Hendrik S., City College, Dept. of Econ., Convent Ave. at 139th St., New York 31, New York
KONIJN, Peter, 305 S. 41st St., Apt. 1R, Philadelphia, Pa. 19104
KONKEL, Ronald M., 938 Elton St., Houston, Tex. 77034
KONNO, Genpachiro, Faculty of Econ., Univ. of Tokyo, 3-1, Hongo 7-chome, Tokyo, Japan
KONO, Osao, Barao de Tingua, 305 Nova Iguacu, Est. do Rio, Brazil
KONOPA, Leonard J., Univ. of Iowa, Col. of Com., University Hall, Iowa City, Iowa
KONSTAS, Panos, Wisconsin State Univ., Dept. of Econ., Whitewater, Wis. 53190
KONTOS, Steve E., Ball State Univ., Dept. of Phil., Polit. Sci. and Econ., Muncie, Ind.
KOO, Anthony Y. C., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
KOO, Shou-Eng, 1908 Knowles St., Cleveland 12, Ohio
KOO, Young Whe, Otterbein Col., Dept. of Econ. and Bus. Admin., Westerville, Ohio 43081
KOOISTRA, John, Jr., 310 Spring St., Little Rock, Ark.
KOONTZ, Harold D., 4838 Gloria Ave., Encino, Calif.
KOOPMANS, Tjalling, Cowles Found. for Res. in Econ., Yale Univ., Box 2125, Yale Sta., New Haven, Conn.

KOORENNY, Ralph L., La Sierra Col., Div. of Bus. and Sec. Admin., Riverside, Calif. 92505
KOOT, Ronald S., Univ. of Oregon, Dept. of Econ., Eugene, Ore. 97401
KOOYMAN, Jan, Eerste Helmersstraat 74, Amsterdam, The Netherlands
KOPANITSAS, Demetrius E., 8 Tsakalof St., Athens 136, Greece
KOPF, David H., Research Dept., Fed. Res. Bank of N.Y., New York 45, N.Y.
KOPIT, Jerome, 11-01 162nd St., Bldg. #1, Apt. 3D, Whitestone, N.Y. 11357
KOPITZKE, Veldor A., Univ. of Wisconsin, Midway Rd., Fox Valley Center, Menasha, Wis. 54952
KOPLIN, H. Thomas, Univ. of Oregon, Dept. of Econ., Eugene, Ore.
KOPUSINSKI, Albert T., 141 Grand Ave., Englewood, N.J.
KORAITIM, Abdel H., 12 Elfos-ha St., Polkley, Ramleh, Alexandria, Egypt
KORANYI, Karl H., 630 Sheridan St., Hyattsville, Md.
KORBONSKI, Andrzej, Univ. of California, Dept. of Polit. Sci., Los Angeles 24, Calif.
KORETSKY, Henry A., 99 Ridge Hill Dr., Toronto 10, Ont., Canada
KORETZ, Sidney, 2817 Gainesville St., S.E., Washington, D.C. 20020
KORI, Gangadhar S., Southern Illinois Univ., Bus. Div., Edwardsville, Ill.
KORICAN, Otto H., Copley Int. Corp., 330 Madison Ave., New York, N.Y. 10017
KORN, M. L., Northern Oklahoma Junior Col., Dept. of Soc Sci., Tonkawa, Okla. 74653
KORNELL, Ronald F., 4441 S. 31st St., Arlington, Va.
KORNER, Emil, House of Elrig, Port William, Wigtownshire, Scotland
KORNIS, (Mrs.) Magdolna, 1816 New Hampshire Ave., N.W., Washington, D.C. 20009
KORNREICH, (Mrs.) Sonya, 8 Kay St., Jericho, L.I., N.Y. 11753
KOROPECKYJ, Iwan S., 617 Stratford Dr., Moorestown, N.J. 08057
KORP, Ralph V., Office of Industrial Nations, U.S. Treasury Dept., Washington, D.C. 20220
KORSUN, Meyer, Cherry Hill Apts., 1008 East, Cherry Hill, N.J. 08034
KORSVIK, William J., First Nat. Bank of Chicago, Chicago, Ill. 60690
KORTE, Louis, 75 Edgewood Dr., Florham Park, N.J. 07932
KORTELING, Gerrit J., Bennebroekerdreef 29, Bennebroek, The Netherlands
KOSH, David A., 3606 N. Woodstock St., Arlington 7, Va.
KOSHAL, Rajindra K., Ohio Univ., Dept. of Econ., Athens, Ohio 45701
KOSOBUD, Richard F., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
KOSS, J. J., 240 E. Bowman St., Wooster, Ohio
KOSSICK, Robert M., Box 597, Randolph AFB, Tex.
KOSSORIS, Max Davis, 325 Seale Ave., Palo Alto, Calif.
KOST, William E., Michigan State Univ., Dept. of Agric. Econ., 41 Agric. Hall, East Lansing, Mich. 48823
KOTLER, Philip, 8845 N. Lincolnwood Dr., Evanston, Ill. 60203
KOTOWITZ, Yehuda, 207 Ellis Ave., Toronto, Ont., Canada
KOTSONIS, Denis S., 80 Crane Rd., Mountain Lakes, N.J.
KOTTKE, Frank J., Washington State Univ., Todd Hall, Pullman, Wash.
KOTTKE, Frederick E., 1890 N. Denair Ave., Turlock, Calif. 95380
KOULES, Stephen, 137 E. 28th St., New York, N.Y. 10016
KOULOURIANOS, Dimitri Th., c/o Bank of Greece, Econ. Res. Dept., Athens, Greece
KOUTOUZOS, Eurysthenes, 44-15 43rd Ave., Sunnyside, N.Y. 12782
KOVACH, Robert Stephen, 1824 Opalocka Dr., McLean, Va.
KOVACS, Aranka E., Univ. of Windsor, Dept. of Econ., Windsor, Ont., Canada
KOVACS, Julius, c/o Malden Mills, Inc., 46 Stafford St., Lawrence, Mass.
KOWAL, Lubomyr M., Univ. of Toledo, Econ. Dept., Toledo, Ohio 43606
KOZELKA, Richard L., Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis, Minn.
KOZIARA, Edward C., San Jose State Col., Dept. of Econ., San Jose, Calif.
KOZIARA, (Mrs.) Karen S., San Jose State Col., Dept. of Econ., San Jose, Calif.
KOZIEBRODZKI, Leopold B., 1500 Oakview Dr., McLean, Va. 22101
KOZIK, Eugene, 24 Chetwynd Rd., Paoli, Pa. 19301
KRAATZ, Christian L., 40 Ranch Trail, Buffalo, N.Y. 14221
KRACHENBERG, Anthony R., Univ. of Michigan, Dearborn Center, Evergreen Rd., Dearborn, Mich.
KRAEMER, Charles L., 19 Bond Ave., Lavallette, N.J. 08735
KRAEMER, E. O., 236 Kaalawai Pl., Honolulu, Hawaii 96815
KRAEMER, Ronaldo C., Rua Brigadeiro Franco 1616, Curitiba, Est. do Parana, Brazil
KRAESSEL, Alfred, 689 Prospect Ave., West Orange, N.J. 07052
KRAFT, Gerald, 371 Central St., Auburndale, Mass. 01501
KRAMER, Charles M., Dean Junior Col., 99 Main St., Franklin, Mass. 02038
KRAMER, Helen M., 60 Remsen St., #7-D, Brooklyn, N.Y. 11201
KRAMER, Leo, Diebold Group, Inc., 1616 H St., N.W., Washington, D.C. 20006
KRAMER, Udo E., P.O. Box 2252, La Jolla, Calif. 92037
KRAMER, Walter, Portland State Col., Sch. of Bus. Admin., Portland, Ore. 97207
KRAUS, Irwin, 144-33 28th Ave., Flushing 54, N.Y.
KRAUS, Jerome, 141-30 Pershing Crescent, Jamaica, N.Y. 11435
KRAUS, Willy, Gevelsberg I. W., Klashammer 56, West Germany
KRAUSE, Lawrence Berle, Brookings Institution, 1775 Massachusetts Ave., N.W., Washington 6, D.C.
KRAUSE, Orville E., 630 A St., N.E., Washington, D.C. 20002
KRAUSE, Walter, State Univ. of Iowa, Dept. of Econ., Iowa City, Iowa
KRAUSKOPF, Henry K., Quinnipiac Col., 1450 Whitney Ave., Hamden, Conn. 06439
KRAUSS, Melvyn B., 690 Ocean Pkwy., Brooklyn 30, N.Y.
KRAUTHAMER, Sigmund, U.S. Naval Postgraduate Sch., Dept. of Bus. Admin., Monterey, Calif.
KRAUTHOFF, Louis C., Com. for Nat. Trade Policy, 1028 Connecticut Ave., N.W., Washington, D.C. 20036
KRAVIS, Irving Bernard, Wharton Sch., Univ. of Pennsylvania, Philadelphia, Pa.
KRAWCZYK, Richard F., 1181 Seneca Creek Rd., West Seneca, N Y. 14224
KREBS, Gerhard, 3267 Yorkshire Rd., Cleveland Heights, Ohio 44118
KREBS, Michael E., 101 E. 201st St., Euclid, Ohio 44123
KREIDER, Carl, Goshen Col., Goshen, Ind. 46526
KREIDER, Lawrence E., 293 Millwood Rd., Chappaqua, N.Y. 10514
KREIDER, Leonard E., Southern Illinois Univ., Dept. of Econ., Carbondale, Ill. 62903
KREIDLE, John R., Northern Illinois Univ., Dept. of Fin., DeKalb, Ill. 60115
KREININ, Mordechai E., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
KREITNER, Richard G., 57 Huntington St., Hartford, Conn. 06105
KRENZ, Ronald D., Iowa State Univ., FPED, ERS, USDA, East Hall, Room 310, Ames, Iowa 50010
KREPS, Clifton H., Jr., Morgan Creek Rd., Chapel Hill, N.C. 27514
KREPS, (Mrs.) Juanita M., Morgan Creek Rd., Chapel Hill, N.C. 27514
KREPS, Theodore J., 744 Mayfield Ave., Stanford, Calif. 94305
KRESGE, Wharton R., 1507 Linglestown Rd., Harrisburg, Pa.
KRESL, Peter Karl, Univ. of Texas, Dept. of Econ., Austin, Tex. 78712
KRESS, Andrew J., 4234 47th St., N.W., Washington, D.C. 20016

KRESS, Donald L., Burgstrasse 118, 5034 Gleuel, West Germany
KRETZSCHMAR, Charles P., Norwich Univ., Dept. of Econ. and Bus. Admin., Northfield, Vt. 05663
KRIEGER, Ronald A., 2512 Que St., N.W., Apt. 309, Washington, D.C. 20007
KRIER, Donald F., Newton Col., Newton, Mass.
KRISHNA, K. L., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
KRISHNAN, Kalpathi C., 4647 Walnut St., Philadelphia 39, Pa.
KRISHNAN, V. N., Bowling Green State Univ., Bowling Green, Ohio 43402
KRISTEIN, Marvin Michael, 7 Tudor Dr., Stony Brook, N.Y. 11790
KRISTOF, Frank S., 175 Adams St., Brooklyn 1, N.Y.
KRIZ, Miroslav A., 46 Bayard Lane, Princeton, N.J. 08540
KROEKER, Herbert R., 524 Wayne Dr., Fairborn, Ohio 45324
KROES, Robert J., 2881 Estrada Pl., Santa Maria, Calif.
KROFTA, Raymond N., Univ. of Maine, Dept. of Agric., Bus. and Econ., Orono, Me. 04473
KROLICK, Reuben H., 1471 Greene Dr., San Jose 29, Calif.
KROLL, Harvey N., 66 Cleary, Apt. 206, San Francisco, Calif. 94109
KRONENBERG, Philip S., 6605 Ridgeville St., Pittsburgh, Pa. 15217
KRONENBERG, Vernon J., Box 104 P.O., Manuka, A.C.T., Australia
KRONISH, Sidney J., 547 Sandra Pl., Teaneck, N.J. 07666
KROOPNICK, Ethel, 19926 Blackstone, Detroit 19, Mich.
KROOSS, H. E., New York Univ., 90 Trinity Pl., New York 6, N.Y.
KROWITZ, Edward J., USAID, APO 152, San Francisco, Calif.
KRUEGER, (Mrs.) Anne Louise, Univ. of Minneapolis, Dept. of Econ., Minneapolis 14, Minn.
KRUEGER, Robert O., Chicago Teachers Col. N., 6500 N. St. Louis Ave., Chicago, Ill. 60625
KRUGER, John H., Jr., 4884 N. Diana, Fresno, Calif. 93726
KRUIZENGA, Richard J., Humble Oil and Refining Co., Econ. and Planning, P.O. Box 2180, Houston 1, Tex.
KRUMME, Gunter, Univ. of Washington, Dept. of Geog., Seattle, Wash. 98105
KRUSE, Richard H., Investors Diversified Services, Inc., Investors Bldg., 8th and Marquette, Minneapolis, Minn. 55402
KRUTILLA, John V., 6609 Beverly Ave., McLean, Va. 22101
KRUYNE, Jacobus, 11670 N. St. James Lane 28 W., Mequon, Wis. 53092
KRYZDA, Bill F., Goodrich, Dalton, Little and Riquelme, Balderas 36, 130 piso, Mexico 1, D.F., Mexico
KRZYANIAK, Marian, Rice Univ., Dept. of Econ., Houston, Tex. 77001
KUARK, Yoon Tai, Univ. of Denver, Col. of Bus. Admin., Dept. of Statis. and Res., Denver 2, Colo.
KUBACH, John S., 1095 Balmoral Dr., Pittsburgh, Pa. 15237
KUBE, Harold D., Fauquier Farm, R.F.D. 1, Broad Run, Va. 22014
KUBOTA, Koichi, McGill Univ., Dept. of Econ., Montreal 2, P.Q., Canada
KUCH, Peter J., 1346 W. 6th St., Brooklyn 4, N.Y.
KUCZMA, Julius E., 3602 Underwood St., Chevy Chase, Md. 21620
KUCZYNSKI, Pedro-Pablo, 1601 38th St., N.W., Washington, D.C. 20007
KUDRYK, Oleg, 120 N. Bryan, Bloomington, Ind.
KUENNE, Robert E., 250 Spruce St., Princeton, N.J. 08540
KUGLER, William A., 4815 Cumberland Ave., Chevy Chase, Md. 20015
KUH, Edwin, Sch. of Ind. Mgt., M.I.T., Cambridge, Mass. 01922
KUHL, William O., 9401 Outlook Dr., Overland Park, Kans. 66524
KUHLMAN, John M., Univ. of Missouri, Dept. of Econ., Sch. of Bus. and Pub. Admin., Columbia, Mo.
KUHN, Alfred, Econ. Dept., Univ. of Cincinnati, Cincinnati 21, Ohio
KUHN, Gerald W., 5514 Blackstone Ave., Apt. 103, Chicago, Ill. 60637
KUHN, Harold W., Fine Hall, Box 708, Princeton, N.J. 08540
KUHN, Helmut, Kanalstrasse 18, Metzingen/Wurtt, Germany
KUHN, James W., Grad. Sch. of Bus., Columbia Univ., New York 27, N.Y.
KUHN, Tillo E., c/o OECD, P.O. Box 271, ULUS, Ankara, Turkey
KUHN, William E., 1531 N. Kaspar, Arlington Heights, Ill. 60004
KUHNS, E. Douglas, 7808 Takoma Ave., Takoma Park, Md. 21784
KUHNS, William R., Box 312, Flemington, N.J. 08822
KUIPER, John, 141 Cooper St., Apt. 806, Ottawa 4, Ont., Canada
KUIPERS, Louis, 901 Calvin Ave., Grand Rapids, Mich. 49506
KULLER, Robert G., 1315 W. 19th Ter., Lawrence, Kans.
KULSHRESHTHA, Surendra N., Dept. of Agric., Econ. Br., Ottawa, Ont., Canada
KUMAR, (Mrs.) Dharma, C-1/11, Pandara Rd., New Delhi, India
KUMINS, Lawrence C., 415 4th St., S.E., Washington, D.C.
KUNG, S. W., 165 West End Ave., Apt. 16K, New York 23, N.Y.
KUNICK, Sister M. C., Mount Marty Col., Yankton, S.D.
KUNIN, Leonard, 64 Sims Rd. #3, Santa Cruz, Calif. 95062
KUNKEL, John H., Dept. of Soc., Arizona State Univ., Tempe, Ariz.
KUNNY, John A., 32271 Vista de Catalina, South Laguna, Calif. 92677
KUNREUTHER, Howard, Inst. for Defense Analyses, Econ. and Polit. Sci. Div., 400 Army-Navy Dr., Arlington, Va. 22202
KUNTZ, Edward J., Indiana Univ., Grad. Sch. of Bus., Bloomington, Ind.
KUPFERBERG, David A., 1924 Riverside Dr., Trenton, N.J. 08618
KUPINSKY, Mannie, 303 Belton Rd., Silver Spring, Md. 20901
KUPTZIN, Harold, 1316 Xaveria Ct., Silver Spring, Md. 20903
KURIHARA, Kenneth K., Rutgers Univ., Dept. of Econ., New Brunswick, N.J.
KURNOW, Ernest, 27 Nirvana Ave., Great Neck, N.Y.
KURSBAN, Jerry, 12517 Chelton Ave., Bowie, Md. 20715
KURTH, E. A., Loras Col., Dubuque, Iowa
KURZ, Mordecai, Hebrew Univ., Econ. Dept., Kiriat Hauniversita, Jerusalem, Israel
KUSAR, Daniel D., 1440 N. Lake Shore Dr., Chicago, Ill. 60610
KUSHMAR, Howard S., J. L. Hudson Co., 1206 Woodward Ave., Detroit 26, Mich.
KUSIK, John Edward, 3120 Kingsley Rd., Shaker Heights, Ohio 43947
KUSKA, Edward A., London Sch. of Econ., Aldwych, London, W.C. 2, England
KUTSCHER, Ronald E., 8700 Westwood Dr., Vienna, Va. 22180
KUTY, Bert W., 12409 Griffing Ave., Cleveland, Ohio 44120
KUUS, Leo, 1707 Columbia Rd., N.W., Washington, D.C. 20009
KUWABARA, Kenichi G., 11812 Charles Rd., Silver Spring, Md. 20906
KUWAYAMA, (Mrs.) Patricia H., 142 Fulton St., New York, N.Y. 10038
KUZNETS, George M., Univ. of California, Col. of Agric., Berkeley, Calif. 94720
KUZNETS, Paul W., Indiana Univ., Dept. of Econ., Ballantine Hall, Bloomington, Ind. 47405
KUZNETS, Simon, Harvard Univ., Dept. of Econ., Cambridge, Mass. 01922
KVASNICKA, Joseph George, Fed. Res. Bank of Chicago, Research Dept., Chicago, Ill. 60690
KWACK, Sung Yeung, 1051-D Monroe Ave., Albany, Calif. 94507

KWAK, Nokyoon, Clemson Univ., Dept. of Ind. Mgt., Clemson, S.C. 29631
KWAK, Sang Soo, 38-57 Chang Choong, Dong, 1-Ga, Choong-Ku, Seoul, Korea
KWAN, Robert K., 3534 S. Sycamore, Los Angeles, Calif.
KWANG, Ching-wen, Calif. State Col. at Hayward, Hayward, Calif. 94542
KWO, T. T., 159 Chashwa Dr., Centerville, Ohio 45459
KWON, Jene K., 203 Stadium Park, Iowa City, Iowa
KYBAL, Elba A. Gomez del Rey de, 1 Buttonwood Lane, Tulip Hill, Washington 16, D.C.
KYBAL, Milic, 1 Buttonwood Lane, Tulip Hill, Washington 16, D.C.
KYLE, James T., Indiana State Col., Dept. of Soc. Sci., Terre Haute, Ind.
KYLE, Robert, 32 Hollywood Rd., Winchester, Mass. 01890
KYLER, Rudolph H., Univ. of Massachusetts, Sch. of Bus. Admin., Amherst, Mass.
KYVIG, Thomas, 148 Slocum Rd., Wauconda, Ill. 60084

LAATTO, Erkki, Sveavagen 166 A 21, Stockholm, Sweden
LaBARGE, Richard A., 1045 Merritt Dr., Tallahassee, Fla.
LABBACI, Tayeb, 31 Rue, Sid, Larbi Boulenouar, Sig, Dpt., Oran, Algeria
LABER, Gene E., 8716 Cameron St., Apt. 109, Silver Spring, Md. 20910
LABINI, P. Sylos, Via Capodistria 4, Rome, Italy
LABOVITZ, David E., 5405 Carolina Pl., N.W., Washington, D C. 20016
LABOVITZ, Israel M., 1370 Fourth St., S.W., Washington, D.C. 20024
LABSVIRS, Janis, Indiana State Univ., Dept. of Univ., Terre Haute, Ind. 47809
LABYS, Walter C., 819 Forest St., Latrobe, Pa. 15650
La CASCIA, Joseph S., Univ. of Omaha, Dept. of Econ., 60th and Dodge Sts., Omaha, Neb. 68101
LACASSE, Francois, 1319 Lancaster St., Durham, N C. 27701
LACEBAL, Anacleto, 326 Wilson St., Albany 10, Calif 94507
LACEY, Thomas W., 259 Broadmeadows Blvd., Columbus, Ohio
LACHMAN, Alexis E., USOECD, APO New York, N.Y. 09777
LACHMAN, Karl E., 115-30 Park Lane S., Kew Gardens, N.Y. 14478
LACKAS, John C., 41 Hicks Ave., Syosset, N.Y. 11791
LaCOMB, Rene J., 1904 63rd St., Brooklyn, N.Y.
LADD, George Wells, Iowa State Col., Dept. of Econ. and Soc., Ames, Iowa
LADD, Richard A., U.S. Coast Guard Academy, New London, Conn. 06320
LADEN, Ben E., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore 18, Md.
LADENSON, Mark L., 6517 N. Artesian, Chicago, Ill. 60645
LADIN, Jay J., 16½ N. Salisbury, West Lafayette, Ind. 47178
LADMAN, Jerry R., 50 Circuito de Navegantes, Ciudad Satelito, Edo. de Mexico, Mexico
LAFFER, Kingsley, Univ. of Sydney, Econ. Dept., Sydney, N.S.W., Australia
LAFFERTY, Sister Mary C., Villa Julie Col., Stevenson, Md. 21153
La FLEUR, James R., 237 School St., Pittsburg, Calif. 94565
LAFONTAINE, Maurice, 37 St.-Antoine, Pointe-Gatineau, Que., Canada
LAGE, Gerald M., Univ. of Minnesota, 922 B.A. Tower, Minneapolis, Minn.
LAGES, J. David, Southwest Missouri State Col., Dept. of Soc. and Sci., Springfield, Mo.
LAGO, Armando, 3375 Alma St., Apt. 271, Palo Alto, Calif. 94306
LAIDLER, Harry W., 292 Garfield Pl., Brooklyn, N.Y.
LAIRD, William E., Jr., Florida State Univ., Dept. of Econ., Tallahassee, Fla.
LAITILA, Edward E., Univ. of Illinois, Dept. of Fin., David Kinely Hall, Urbana, Ill.
LAITIN, Howard, 4916 White Ct., Torrance, Calif. 90503
LAITMAN, Leon, Box 1074, Curundu, C.Z.
LAJOUS-VARGAS, Adrian, Fuego 824, Mexico 20, D.F., Mexico
LAKE, James, Ind. Col. of Armed Forces, Washington 25, D.C.
LAKOS, Ivan A., Antioch Col., Dept. of Econ., Yellow Spring, Ohio 45387
LAL, K., Univ. of Saskatchewan, Dept. of Econ. and Polit. Sci., Saskatoon, Sask., Canada
LALLIER, Adalbert G., International House, Room 771, 500 Riverside Dr., New York, N.Y. 10027
LAMB, George A., 746 Ninth St., Oakmont, Pa. 15139
LAMB, Harry L., 2855 S. Abingdon St., Arlington, Va. 22206
LAMB, W. Ralph, Hastings Col., Hastings, Neb. 68901
LAMBERT, Eugene Wasdon, Jr., Univ. of Texas, Room 115, B.E.O.B. Bldg., Austin, Tex. 78712
LAMDEN, Charles W., c/o Peat, Marwick, Mitchell and Co., 20, Rue Louis-le-Grand, Paris 2, France
LAMPHEAR, F. Charles, Kansas State Univ., Dept. of Econ., Waters Hall, Manhattan, Kans. 66504
LAMPMAN, Robert J., 729 Oneida Pl., Madison 11, Wis.
LAMSON, Robert D., 5512 15th, N.E., #204, Seattle, Wash. 98105
LANAHAN, Thomas J., Jr., 4917 7th Pl., N.E., Washington, D.C. 20017
LANCASTER, Kelvin J., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore 18, Md.
LAND, James W., Yale Univ., 52 Hillhouse Ave., New Haven, Conn.
LAND, Philip, Piazza della Pilotta 4, Rome, Italy
LANDAU, Alfred, 370 First Ave., Apt. 5A, New York 10, N.Y.
LANDAU, Emanuel, 5906 Green Tree Rd., Bethesda, Md. 20034
LANDAUER, Carl, 119 South Hall, Campus, Berkeley 4, Calif.
LANDAY, Donald M., 6304 Owen Pl., Bethesda, Md. 21811
LANDE, Henry F., IBM Corp., Armonk, N.Y. 10501
LANDEKICH, Stephen, 430 E. 56 St., New York, N.Y. 10022
LANDER, Neil E., 420 Cortez, Denver, Colo. 80221
LANDES, David S., Harvard Univ., Dept. of History, Cambridge, Mass. 02138
LANDES, William M., Stanford Univ., Dept. of Econ., Stanford, Calif.
LANDO, Mordechai E., 217 Haven Ave., New York, N Y. 10033
LANDOLT, Robert G., 2934 Phillips, South Charleston, W Va.
LANDON, Charles E., Campbell Col., Box 291, Buies Creek, N C. 27506
LANDOW, Herbert T., 11212 W. 74th St., La Grange, Ill. 60525
LANDRETH, Edward H., Crowder Col., Dept. of Bus and Econ., Box 2, Neosho, Mo. 64850
LANDRETH, Harry H., Jr., Miami Univ., Dept. of Econ., Oxford, Ohio 45056
LANDRY, Richard S., 5417 N. 19th St., Arlington 5, Va.
LANDRY, Robert John, 19 W. 31st St., New York, N.Y. 10001
LANDSBERG, Hans H., 2314 Ashboro Dr., Chevy Chase, Md. 21620
LANDUYT, Bernard F., Univ. of Detroit, Col. of Com. and Fin., Detroit, Mich.
LANE, Sylvia, 2399 Redlands Dr., Newport Beach, Calif.
LANFRANCO, Samuel L., 1341 Milvia, Berkeley, Calif.
LANG, Aldon S., 2906 Washington Ave., Waco, Tex.
LANG, Richard O., S.C. Johnson and Son, Inc., 1525 How St., Racine, Wis. 53403
LANGBEHN, William A., 301 W. Pasadena, Apt. 1-B, Phoenix, Ariz. 85013
LANGBEIN, Leland H., 501 Windermere Ave., Interlaken, N.J. 07712
LANGDON, Arthur H., Calle Zugazarte 16, La Florida, Madrid, Spain

LANGENBERT, H. F., Reinholdt and Gardner, 400 Locust St., St. Louis, Mo. 63102
LANGER, Henry C., Alfred Univ., Box 664, Alfred, N.Y. 14802
LANGER, Leonard C. R., Morgan Guaranty Trust Co., 23 Wall St., New York, N.Y. 10015
LANGHAM, Max R., Univ. of Florida, Col. of Agric., Dept. of Agric. Econ., Gainesville, Fla.
LANGHOFF, Severin P., Jr., Young and Rubicam, Inc., 285 Madison Ave., New York 17, N.Y.
LANGLEY, (Mrs.) Kathleen M., 987 Memorial Dr., Cambridge, Mass. 02138
LANGLEY, Stanley James, 987 Memorial Dr., Cambridge, Mass. 02138
LANGMAID, Bradshaw, Jr., 701 Ferry Landing Rd., Alexandria, Va.
LANGSNER, Adolph, P.O. Box 862, Hemet, Calif. 92343
LANGSTON, Loyd H., Riverlands, Rumson, N.J. 07760
LANGSTON, Mildred J., Riverlands, Rumson, N.J. 07760
LANGUM, John Kenneth, 209 S. LaSalle St., Chicago, Ill. 60604
LANHAM, Benjamin T., Jr., 536 Gay St., Auburn, Ala. 36830
LANHAM, William J., Clemson Univ., Dept. of Agric. Econ., Clemson, S.C.
LANIER, Charles N., Jr., Univ. of Delaware, Newark, Del. 19711
LANYI, Anthony M. J., 1625 Bonita Ave., Berkeley, Calif. 94709
LANKA, George, 1400 S. Joyce St., Apt. C1209, Arlington, Va. 22202
LANKENAU, Armin H., 3808 Kelsey St., Silver Spring (Wheaton), Md. 20906
LANOU, Frank R., Jr., 241 6th Ave., Apt. 7D, New York, N.Y. 10014
La NOUE, Bertrand, St. Cecilia's Rectory, 5418 Louisiana, St. Louis, Mo.
LANSFORD, Robert R., New Mexico State Univ., Dept. of Agric. Econ., University Park, N.M. 88070
LANSING, John B., 4063 E. Huron River Dr., Ann Arbor, Mich.
LANTOS, Thomas P., California State Col., Office of Int. Prog., 1600 Holloway Ave., San Francisco 32, Calif.
LANZILLA, Eugene E., 31 Columbine Rd., Norwood, Mass. 02062
LANZILLOTTI, Robert F., 1682 Woodside Dr., East Lansing, Mich. 48823
LAPA, Joao Faria, Rua de S. Bernardo, 132-1 Esq., Lisbon, Portugal
LAPINSKY, Martin, 2330 E. 71st St., Brooklyn 34, N.Y.
LAPKIN, David T., Univ. of North Carolina, Dept. of Econ., Chapel Hill, N.C.
LaPITTUS, Jerome R., Cornell Univ., Dept. of Econ., Ithaca, N.Y. 14850
LARGAY, James Anthony, III, 3750 E. Jewel #418, Denver, Colo. 80210
LARIMORE, Theodore R., 4102 Woodberry St., University Park, Hyattsville, Md. 20782
LARKIN, John T., 125 Trapelo Rd., Apt. #30, Belmont, Mass. 02178
LARNER, Robert J., 151 E. Gilman St., Madison, Wis. 53703
LaROCQUE, Jules N., Lawrence Univ., Dept. of Econ., Appleton, Wis. 54911
LARRAIN, Jorge, VIECUR Ltd., (Viviendas Econ. Urbanas Ltda.), Ahumada 254, Santiago, Chile
LARSEN, Allen F., 528 Sixth Ave., S., St. Cloud, Minn. 56301
LARSEN, Arne, Landokonomisk Driftsbureau, Valby Langgade 19, Copenhagen Valby, Denmark
LARSEN, Larry J., Univ. of Nevada, Dept. of Econ., Reno, Nev. 89507
LARSEN, Richard F., Univ. of North Dakota, Econ. Dept., Grand Forks, N.D.
LARSON, Arlyn J., 414 E. LaJolla Dr., Tempe, Ariz. 85281
LARSON, Arnold B., Univ. of Hawaii, Dept. of Agric. Econ., Honolulu, Hawaii
LARSON, Charles A., 170 Park Row, New York, N.Y. 10038
LARSON, Gerhard P., State Univ. of Iowa, Dept. of Econ., Iowa City, Iowa 4152240
LARSON, John Albert, Northwestern Univ., Sch. of Bus., Evanston, Ill. 60201
LARSON, Joseph W., 23500 Northwest Hwy., Rm. E-127, Southfield, Mich.
LARSON, Melvin P., Quinnipiac Col., 1450 Whitney Ave., Hamden, Conn. 06439
LARY, Hal B., Nat. Bur. Econ. Res., 261 Madison Ave., New York 16, N.Y.
LASHINSKY, Bernard, Inland Steel Co., 30 W. Monroe St., Chicago, Ill. 60603
LASKO, Warren A., 2128 Key Blvd., Arlington, Va. 22201
LASSEN, Andrew P., 18570 Sobey Rd., Saratoga, Calif. 95070
LASSITER, Roy L., Jr., Rt. 4, Box 176 X, Gainesville, Fla.
LASSWELL, Harold D., Yale School of Law, New Haven, Conn.
LAST, Jerry, 62 Glenwood Rd., Upper Montclair, N.J. 07043
LATANE, Henry Allen, 212 Vance St., Chapel Hill, N.C.
LATHAM, William C., P.O. Box 4363, Tech. Sta., Lubbock, Tex. 79409
LATIMER, Murray Webb, 1625 K St., N.W., Washington, D.C. 20006
LATONA, Joseph C., 564 Cypress Ave., Akron, Ohio 44301
LATORTUE, Christian, Jr., C.P. 102 Station Sillery, Quebec 6, P.Q., Canada
LATORTUE, Gerard R., Inter American Univ. 451, San German, P.R. 00753
La TOURETTE, John, Harpur Col., Dept. of Econ., Binghamton, N.Y.
LATULIPPE, Jean-Guy, Valley Junction, Beauce, P.Q., Canada
LAUBE, Richard H., Valparaiso Univ., Col. of Bus. Admin., Valparaiso, Ind. 46383
LAUDADIO, Leonard, Univ. of Nevada, Col. of Bus. Admin., Reno, Nev.
LAUDOR, Charles R., 240 Clinton Ave., New Rochelle, N.Y.
LAUER, Raymond R., Mt. St. Marys College, Emmitsburg, Md. 21727
LAUFER, Arthur C., 450 Linares Ave., Long Beach 14, Calif.
LAUGHLIN, Eugene, Kansas State Univ. Col. of Com., Calvin Hall, Manhattan, Kans. 66504
LAUGHLIN, Louis G., Security First Nat. Bank, Res. Div., P.O. Box 2097, Terminal Annex, Los Angeles, Calif. 90054
LAUMAS, Gurcharan S., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
LAUMAS, Prem Singh, Northern Illinois Univ., Dept. of Econ., De Kalb, Ill. 60115
LAUNIE, Joseph J., 5902 Canyonside Rd., La Crescenta, Calif. 91014
LAURANT, Henry W., Stanford Res. Inst., Menlo Park, Calif.
LAURENT, Jerome K., Wisconsin State Univ., Dept. of Econ., Whitewater, Wis., 53190
LAUTER, Geza Peter, US/AID, Public Services, APO 254, New York, N.Y. 09254
LAUTERBACH, Albert, Sarah Lawrence Col., Bronxville 8, N.Y.
LAUTERSTEIN, Marion Moise, 2 Washington Square Village, 1-S, New York, N.Y. 10012
LaVALLE, Irving H., Tulane Univ., Sch. of Bus. Admin., New Orleans, La. 70118
LAVE, Charles A., Stanford Univ., Dept. of Econ., Stanford, Calif. 96128
LAVE, (Mrs.) Judith R., G.S.I.A., Carnegie Inst. of Tech., Pittsburgh, Pa. 15213
LAVE, Lester B., G.S.I.A., Carnegie Institute of Tech., Pittsburgh, Pa. 15213
LAVELLE, Michael J., Colleg Sankt Georgen, Offenbacher Landstrasse 224, Frankfurt/M-Sud 10, Germany
LaVERDA, Michael J., Newark Col. of Eng., Dept. of Ind. and Mktg. Eng., Newark 2, N.J.
LAVIN, Paul M., 103-B Staff Row, Rantoul, Ill. 61866
LAVINE, Jeffrey A., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh, Pa.
LAW, Alton Dennis, 55 Pennsylvania Ave., Westminster, Md. 21157
LAW, Hugh E., Ferris State Col., Sch. of Com., Big Rapids, Mich. 49307
LAW, Marlin V., Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta 13, Ga.
LAW, Warren A., Harvard Univ., Grad. Sch. of Bus. Adm., Soldier Field, Boston 63, Mass.
LAWRENCE, Charles B., 6505 Wescasset Rd., Washington 16, D.C.

LAWRENCE, James S., Old Chatham, N.Y. 12136
LAWRENCE, Mary E., 3940 Suitland Rd., Suitland, Md. 20023
LAWRENCE, Robert J., Bd. of Gov., Fed. Res. Sys., Banking Markets Unit, Washington, D.C. 20551
LAWRENCE, Roger C., 170 W. 73rd St., Apt. 3-C, New York, N.Y. 10023
LAWS, Ralph, Univ. of California at L.A., Dept. of Econ., Los Angeles, Calif. 90024
LAWS, Robert H., 530 E. 72nd St. Apt. 6-E, New York, N.Y. 10021
LAWSON, Eric W., R.F.D. 3, Canastota, N.Y. 13032
LAWSON, Robert L., Ball State Univ., Muncie, Ind. 47306
LAWSON, William H., 3164 Celeste Dr., Riverside, Calif.
LAYCOCK, Kenneth, Queen's Univ., Graduate Residence, Kingston, Ont., Canada
LAYER, Robert G., Southern Illinois Univ., Dept. of Econ., Carbondale, Ill.
LAYNG, John W., 6418 Charnwood St., Springfield, Va.
LAYTON, Lee E., Cornell Col., Dept. of Econ. and Bus., Mount Vernon, Iowa 52314
LAYTON, Walter T., The Economist, 22 Ryder Street, St. James, London S.W. 1, England
LAZARCIK, Gregor, Columbia Univ., 635 W. 115th St., New York 25, N.Y.
LAZER, William, Michigan State Univ., Dept. of Mktg., East Lansing, Mich.
LE, Mai Van, Univ. of Hue, Faculty of Law, 4, Le Loi, Hue, Vietnam
LEABO, Dick A., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich. 48104
LEACH, Geoffrey C., 1002 McGregor Ave., Apt. 207, Montreal 2, Que., Canada
LEADER, Alan Howard, 3816 Wedgwood Dr., Kalamazoo, Mich. 49001
LEAHY, Daniel J., St. Josephs Col., 54th and City Line Ave., Philadelphia 31, Pa.
LEAHY, William H., Univ. of Notre Dame, D-1 Univ. Village, Notre Dame, Ind. 46556
LEAKEY, Bernard Price, c/o Treasurers Dept., Creole Petroleum Corp., Apartado 889, Caracas, Venezuela
LEAL-BERNAL, Jorge A., Consejo Nac. de Plan. y Coordinacion, Casa Presidencial, San Salvador, El Salvador
LEAMER, Laurence E., Harpur Col., Binghampton, N.Y.
LEAMING, George F., 1016 W. Simmons Rd., Tucson, Ariz.
LEAN, David Francis, 2000 Penncraft Ct., Ann Arbor, Mich. 48103
LEAN, (Mrs.) Jane A., 2000 Penncraft Ct., Ann Arbor, Mich. 48103
LEANO, Pedro C., Bradley Univ., Col. of Bus. Admin., Peoria, Ill.
LEARN, Elmer W., Univ. of Minnesota, 202 Morrill Hall, Minneapolis, Minn. 55455
LEARNED, Edmund, Harvard Bus. Sch., Soldiers Field, Boston, Mass.
LEARY, John C., American Embassy, APO 503, San Francisco, Calif. 96503
LEASURE, J. William, San Diego State Col., Dept. of Econ., San Diego 15, Calif.
LEAVITT, Charles T., State Col. of Iowa, Dept. of Soc. Sci., Cedar Falls, Iowa 50613
LEBEDA, Agnes, State Col. of Iowa, Cedar Falls, Iowa 50613
LeBEL, Allen E., 59 Sandridge Ct., R.D. 1, Pennellville, N.Y. 13132
LECCISOTTI, Mario, P. le Medaglie d'oro 7, Rome, Italy
LEBERGOTT, Stanley, Univ. of California, 212 California Hall, Berkeley, Calif. 94720
LEBOWITZ, Michael A., Simon Fraser Univ., Econ. Dept., Barnaby, Vancouver, B.C., Canada
Le BRETON, Betty Lou, 5524 N.E. 180th., Seattle, Wash.
Le BRETON, Preston, 5524 N.E. 180th, Seattle, Wash.
LECHNER, Hans H., Neue Kantstrasse 27/111, 1 Berlin 19, Germany
LECHT, Leonard A., 4828 Edgemoor Lane, Bethesda, Md. 20014
LEDEBUR, Larry C., 1343 Charlotte, Tallahassee, Fla.
LEDERER, Walther, 1155 Westmorland Rd., Alexandria, Va. 22308
LEDERMAN, Leonard L., 11424 Monterey Dr., Silver Spring, Md.
LEE, Abraham Chei-Min, Yonsei Univ., Bus. Sch., Seoul, Korea
LEE, Chan Hyu, 1419 Idylwild Dr., Lincoln, Neb. 68503
LEE, Chang Soo, No. 11-14 Ka-Hoe-Dong, Ching-No-Ku, Seoul, Korea
LEE, Chi-Ling, Wisconsin State Col., Whitewater, Wis. 53190
LEE, Chung Hoon, 2704 Benvenue, Berkeley, Calif. 94705
LEE, Dae Sung, 10 Pleasant Ct., Amherst, Mass.
LEE, Feng-Yao, Univ. of Tennessee, Col. of Bus. Admin., Dept. of Econ., Knoxville, Tenn. 37916
LEE, (Mrs.) Guo-I, 400 S. Union St., Burlington, Vt.
LEE, Harry H., 5750 Cumberland St., Paradise Hills, San Diego 14, Calif.
LEE, Homer S., 221-41 Murdock Ave., Jamaica, N.Y. 11429
LEE, Hong Yong, Texas Tech. Col., Dept. of Agric. Econ., Lubbock, Tex.
LEE, Ivan M., Univ. of California, Giannini Found., Berkeley, Calif. 94720
LEE, John E., Jr., 7134 Alger Rd., Falls Church, Va. 22042
LEE, K. H. Daniel, Republic of Korea Gov., Econ. Planning Bd., Seoul, Korea
LEE, Kie Bok, Skidmore Col., Dept. of Econ., Saratoga Springs, N.Y. 12866
LEE, Lyle H., 1826 Frobisher Way, San Jose, Calif. 95124
LEE, Maurice W., 409 Estes Dr., Chapel Hill, N.C.
LEE, Maw-Lin, Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
LEE, Peter S., The Merry Villa Motel, Maryville, Mo. 64468
LEE, Philip H., 72 Morton St., Andover, Mass. 01810
LEE, Ralph M., Mars Hill Col., Mars Hill, N.C. 28754
LEE, Soo Ann, 22 College Green, Singapore 11, Malaya
LEE, Stewart M., Geneva Col., Beaver Falls, Pa. 15010
LEE, Tack S., 5529 S. Dorchester Ave., Chicago, Ill. 60637
LEE, Tong H., Univ. of Tennessee, Dept. of Econ., Knoxville, Tenn. 37916
LEE, Wellington C. H., 3106 S. Wallace Ave., Chicago, Ill. 60616
LEE, William J., St. Marys Seminary, North Paca St., Baltimore 1, Md.
LEEMAN, Wayne A., Univ. of Missouri, 220 New B. and P.A. Bldg., Columbia, Mo.
LEES, Francis A., 192 Coolidge Dr., East Meadow, N.Y. 11939
LEESE, Charles, 514 Lowes St., Dayton, Ohio 45409
LEESEBERG, Donald G., Univ. of Wisconsin, 3203 N. Downer Ave., Milwaukee, Wis. 53211
LEFEBER, Louis, Massachusetts Inst. of Tech. Dept. of Econ., 50 Memorial Dr., Cambridge, Mass. 01922
LEFF, Nathaniel H., Grad. Sch. of Educa., Harvard Univ., 38 Kirkland St., Cambridge, Mass. 01922
LEFTWICH, Howard M., Univ. of Cincinnati, Col. of Bus. Admin., Cincinnati, Ohio
LEFTWICH, Richard H., Oklahoma State Univ., Col. of Bus., Stillwater, Okla.
LEGAN, John, 829 Hamilton St., Rahway, N.J.
LEGAN, William E., 1010 Pine St., St. Louis 1, Mo.
LEGARDA, Benito F., 1 Calabash Rd., Manila, Philippines
LEGARDA, Benito, Jr., Central Bank of the Philippines, Dept. of Econ. Res., Aduana St., Manila, Philippines
LEGER, Pammela R., Cornell Univ., Econ. Dept., Goldwin-Smith, Ithaca, N.Y. 14850
LEGLER, John B., Purdue Univ., Dept. of Econ., West Lafayette, Ind.
LEHMAN, Edward R., 2424 Pennsylvania Ave., N.W., Washington, D.C. 20037

LEHMAN, John D., Richmond House, Wells Rd., Aurora, N.Y. 13026
LEHMAN, John W., 18820 Edgewater Dr., Lakewood, Ohio 44638
LEHMAN, Leland C., Macalester Col., Dept. of Econ., St. Paul, Minn.
LEHMANN, Alexander R., 136-30 Sanford Ave., Flushing, N.Y. 11355
LEHMANN, Charles, 653 Paulson Ave., Clifton, N.J.
LEHMANN, Glenn A., USAID-Tunis, Dept. of State, Washington, D.C. 20521
LEHOCZKY, Sigmund, 4714 Havana Dr., Pittsburgh, Pa.
LEHR, Edward M., 90-59 56th Ave., Elmhurst, N.Y. 14059
LEIB, Karl E., 1204 Kent Dr., Carbondale, Ill. 62901
LEIBACHER, Charles E., 316 Hawthorne St., Glen Ellyn, Ill.
LEIBBRANDT, Gottfried J., Rooseveltlaan 37, Amsterdam Zuid II, Netherlands
LEIBENSTEIN, Harvey, Univ. of California, Dept. of Econ., Berkeley 4, Calif.
LEIBOWITZ, (Mrs.) Arleen, 54 Dorothy Rd., Arlington, Mass. 02174
LEIGH, Arthur, Reed Col., Portland 2, Ore.
LEIGH, Duane E., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
LEIGHTON, Leocade, 4478 Reservoir Dr., N.W., Washington 7, D.C.
LEIGHTON, Richard I., Harpur Col., Dept. of Econ., Binghamton, N.Y. 13901
LEIJONHUFVUD, Axel Stig Bengt, Univ. of California, Dept. of Econ., Los Angeles 24, Calif.
LEIKIND, Hyman, U.S. Tariff Comm., Washington 25, D.C.
LEIKIND, (Mrs.) Lillian P., U.S. Tariff Comm., Washington 25, D.C.
LEININGER, William J., 1709 Forestdale Ct., Royal Oak, Mich. 48067
LEISERSON, Etty, Av. Salvador Maria del Carril 3706, Buenos Aires, Argentina
LEISERSON, Mark W., Yale Univ., Dept. of Econ., 52 Hillhouse Ave., New Haven, Conn. 06520
LEISERSON, Samson, Av. Salvador Maria del Carril 3706, Buenos Aires, Argentina
LEITCH, J. Elizabeth, Canadian Imperial Bank, 25 King Street West, Toronto 1, Ont., Canada
LEITER, Robert D., City College of New York, Dept. of Econ., Convent Ave. and W. 138th St., New York, N.Y. 10031
LEITH, James C., 1030 Emerald St., Madison, Wis. 53715
LEIVICK, Daniel, 6508 Kenhowe Dr., Bethesda, Md. 21811
LEKACHMAN, Robert, State Univ. of New York, Col. of Arts and Sci., Stony Brook, L.I., N.Y. 11790
LELAND, Simeon E., Northwestern Univ., Col. of Lib. Arts, Evanston, Ill.
LELONG, Donald C., Bowling Green State Univ., Bowling Green, Ohio 43402
LEMELIN, Charles, 2116 Chemin Ste-Foy #201, Quebec 10, P.Q., Canada
LEMKE, B. C., Michigan State Univ., Dept. of Acct. and Fin., East Lansing, Mich.
LEMLY, James H., Georgia State Col., 33 Gilmer St., S.E., Atlanta 3, Ga.
LEMON, Isaac E., 6000 44th Ave., Hyattsville, Md. 20781
LEMPERT, Leonard H., Statis. Indicator Associates, North Egremont, Mass. 01252
LENCHES, George, 2400 41st St., N.W., Apt. 200, Washington, D.C. 20007
LENHART, John N., 2710 Chesterton Rd., Shaker Heights, Ohio 44122
LENIHAN, Patrick M., Marquette Univ., Dept. of Econ., Milwaukee 33, Wis.
LENT, George E., 9522 Linden Ave., Bethesda, Md. 21811
LENT, Harold H., Jr., 1103 Roberta Ct., McLean, Va. 22101
LENTI, Libero, Via F. Filzi 15, Milan, Italy
LEON, Alberto, Project MAC, 545 Technology Sq., Cambridge, Mass.
LEON, Charles P., E 14 B Mayaguez Terr. Street 3, Mayaguez, P.R.
LEON, Enrique, 420 E. 72nd St., Apt. 10-K, New York, N.Y. 10021
LEON, Paolo, 5312 Sangamore Rd., Washington, D.C. 20016
LEONARD, J. L., 3341 Ocean Dr., Oxnard, Calif.
LEONARD, James R., Trust Territory Gov. Headquarters, Saipan, Manana Islands 96950
LEONARD, Lawrence A., 4201 Massachusetts Ave., N.W., Apt. 8091-W, Washington, D.C. 20016
LEONARD, Norman H., Jr., 328 N. Liberty St., Delaware, Ohio 43015
LEONARD, Peter I., 1022 Spruce St., Clarksdale, Miss. 38614
LEONARD, Robert L., Brookside Lane, Mansfield Center, Conn. 06268
LEONARD, William G., Queens Univ., Kingston, Ont., Canada
LEONARD, William N., Hofstra Univ., Hempstead, N.Y.
LEONARD, William R., United Nations, Box 20, Grand Central Sta., New York 17, N.Y.
LEONHARD, Dietrich L., 716 Lamon, Wilmette, Ill. 60091
LEONTIADES, Milton, 435 E. 77th St., Apt. 2-G, New York, N.Y.
LEONTIEF, Wassily W., Ctr. for Adv. Stud. in the Behavioral Sci., 202 Junipero Serra Blvd., Stanford, Calif. 94305
LEPPELMEIER, John W., Univ. of Florida, Dept. of Econ., Gainesville, Fla. 32603
LERAGER, A. J., Appleton-Century-Crofts, 440 Park Ave. S., New York, N.Y. 10016
LERBINGER, Otto, Boston Univ., Sch. of Pub. Rela., Boston, Mass. 02138
LERDAU, Enrique, 9716 Byeforde Rd., Kensington, Md. 20795
LERMAN, Allen H., 2685 Yale Sta., New Haven, Conn. 06520
LERMER, Arthur, Sir George Williams Col., 1441 Drummond St., Montreal 25, P.Q., Canada
LERNER, Abba P., Univ. of Tel Aviv, Ramat Aviv, Israel
LERNER, Arthur, Univ. of Guelph, Guelph, Ont., Canada
LERNER, Emanuel R., 4904 Essex Ave., Chevy Chase, Md. 21620
LERNER, Eugene, 143 Brookside Dr., Valley Stream, N.Y.
LERNER, (Mrs.) Evelyn B., 2728 Henry Hudson Pkwy., New York 63, N.Y.
LERNER, Joseph, 1413 Floral St., N.W., Washington 12, D.C.
LERNER, Lionell J., 666 N St., Sacramento, Calif. 95814
LERNER, (Mrs.) Pauline A., 4904 Essex Ave., Chevy Chase, Md.
LEROY, Vely M., Ecole des Hautes-Etudes Commerc., 535 Viger, Montreal, P.Q., Canada
LERRO, A. Joseph, East Carolina Col., Sch. of Bus., Greenville, N.C. 27835
LERRY, David P., Cordial Rd., Yorktown Heights, N.Y. 10598
LESHER, Carl Eugene, 18 Wilson Dr., Ben Aven Heights, Pittsburgh, Pa.
LESIUK, Walter J., P.O. Box 835, Cocoa Beach, Fla. 32931
LESKO, William E., 670 Penn Lane, West Hazleton, Pa. 19566
LESNOY, Selig D., San Fernando Valley Col., 18111 Nordhoff St., Northridge, Calif.
LeSOURD, David A., Univ. of Vermont, Dept. of Econ., Burlington, Vt. 05401
L'ESPERANCE, Wilford L., Ohio State Univ., Dept. of Econ., 1775 S. College Rd., Columbus, Ohio 43210
LESSARD, John R., 484 Fraser St., Quebec 6, P.Q., Canada
LESSER, Arthur, Jr., Stevens Inst. of Tech., Econ. of Eng. Dept., Hoboken, N.J. 07030
LESSLEY, Billy V., 4013 Beechwood Rd., Hyattsville, Md.
LESTER, Richard A., 32 MacLean Circle, Princeton, N.J. 08540
LETENDRE, Emile J., Jr., St. John's Univ., Grand Central and Utopia Pkways., Jamaica, N.Y. 11432
LETHANDER, Richard, Univ. of Massachusetts, Dept. of Econ., Amherst, Mass.
LETICHE, J. M., Univ. of California, Dept. of Econ., Berkeley 4, Calif.

LEUNG, Ping-Kei, 2325 42nd St., N.W., Apt. 309, Washington, D.C. 20007
LEUNG, Cho Kin, 209 Clinton Ave., Apt. 1-H, Brooklyn, N.Y. 11205
LEURQUIN, Philippe P., 32 Sq. Cogtten, Brussels 18, Belgium
LEUTHOLD, Raymond M., 1313 Tomkin Ave., Madison, Wis. 53705
LEVEN, Charles L., Washington Univ., Inst. for Urban and Reg. Studies, St. Louis, Mo. 63130
LEVEN, Merwin, 530 Stanley Ave., Mamaroneck, N.Y. 10543
LEVENSON, Albert M., 338 Church Ave., Woodmere, L.I., N.Y.
LEVESON, Irving F., 2943 Lacenia Ave., New York 69, N.Y.
LEVESON, Sidney M., 633 Nereid Ave., Bronx 70, N.Y.
LEVIN, Bertram F., Univ. of Delaware, Dept. of Econ., Newark, Del. 19711
LEVIN, Gerson, 5529 Ethel Ave., Van Nuys, Calif.
LEVIN, Harvey J., 110 Kilburn Rd., Garden City, N.Y. 11530
LEVIN, Henry M., 944 River Rd., Piscataway, N.J. 08854
LEVIN, Howard, 445 Park Ave., New York 22, New York
LEVIN, Jay H., 315 S. Division St., Apt. 19, Ann Arbor, Mich. 48108
LEVIN, Jonathan, 8620 Rayburn Rd., Bethesda, Md. 20034
LEVIN, Samuel M., 10729 Santa Maria, Detroit, Mich.
LEVIN, Theodore H., 405 Torrington Pl., Silver Spring, Md.
LEVIN, William, 5423 33rd St., N.W., Washington 15, D.C.
LEVINE, Alan H., 984 Aurora St., Highland Park, N.J. 08904
LEVINE, A. Lawrence, Univ. of New Brunswick, Dept. of Econ. and Polit. Sci., Fredericton, N.B., Canada
LEVINE, Dan, Center for Naval Anal., Franklin Inst., 1401 Wilson Blvd., Arlington, Va. 22209
LEVINE, Daniel S., 6924 S. Oglesby, Chicago, Ill. 60649
LEVINE, Herbert S., Univ. of Pennsylvania, Dept. of Econ., Philadelphia, Pa. 19104
LEVINE, Jules M., 34 N. McKinley Ave., Apt. 405, Athens, Ohio
LEVINE, Marvin, 222 Seton Drive, New Rochelle, N.Y.
LEVINE, Marvin B., 1424 Dobson St., Evanston, Ill.
LEVINE, Norman, 1949 64th. St., Brooklyn 4, N.Y.
LEVINE, Robert A., Off. of Econ. Opportunity, 1200 19th St., N.W., Washington, D.C. 20506
LEVINE, (Mrs.) Rosalie, 157 Hemlock Road, Manhasset, N.Y. 11030
LEVINE, Solomon Bernard, Inst. of Labor and Ind. Rela., Univ. of Illinois, Champaign, Ill.
LEVINSON, David, Dept. of Economics, Sch. of Com., Ohio Univ., Athens, Ohio 45701
LEVINSON, Harold M., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
LEVINSON, Marshall M., Oakland Univ., Rochester, Mich. 48063
LEVITAN, Leon B., 43 Whitman Rd., Needham 92, Mass.
LEVITAN, Sar A., 1700 Harvard St., N.W., Washington 9, D.C.
LEVY, Alan J., 136-23 71st Rd., Flushing, N.Y. 11367
LEVY, Elliot H., 762 Riverside Dr., Manhattan, N.Y. 10031
LEVY, Ferdinand K., Rice Univ., Dept. of Econ., Houston, Tex. 77001
LEVY, Fred D., Jr., Syracuse Univ., Dept. of Econ., Syracuse 10, N.Y.
LEVY, Joseph L., 653 Britton St., New York, N.Y. 10467
LEVY, Lester S., Northern Illinois Univ., Dept. of Econ., De Kalb, Ill. 60115
LEVY, Mathew, 401 E. 89th St., New York, N.Y. 10021
LEVY, Michael E., Apt. 228, 90 Morningside, New York 27, N.Y.
LEVY, Michael V., 2 Gibson Ct., South Norwalk, Conn. 06854
LEVY, Mildred B., 988 Lake St., Winnetka, Ill.
LEVY, Richard H., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
LEVY, Robert A., 6723 14th Pl., N.W., Washington, D.C. 20012
LEVY, Samuel, Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
LEVY, Walter J., 30 Rockefeller Plaza, Room 3232, New York 20, N.Y.
LEW, James A., 2290 N. Point St., Apt. 204, San Francisco 23, Calif.
LEWELLEN, Wilbur G., Purdue Univ., Sch. of Ind. Admin., West Lafayette, Ind. 47178
LEWIN, Kurt I., 137 Riverside Dr., New York 24, N.Y.
LEWINS, Leon, 8103 Eastern Ave., Silver Spring, Md.
LEWIS, (Mrs.) Anita X., 7733 N. Fifth Ave., Phoenix, Ariz. 85021
LEWIS, Ben W., 68 S. Professor St., Oberlin, Ohio 44074
LEWIS, Darell R., Luther Col., Dept. of Economics, Decorah, Iowa 52101
LEWIS, Edward E., 1778 Hobart St., N.W., Washington 9, D.C.
LEWIS, Edwin J. B., Alban Towers, Suite 323, 3700 Massachusetts Ave., N.W., Washington, D.C. 20016
LEWIS, Frank L., 3316 Valley Dr., Alexandria, Va. 22302
LEWIS, Gordon Depew, c/o Mr. C. D. Lewis, RFD 4, Box 132, Charlottesville, Va. 22901
LEWIS, Harold G., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60627
LEWIS, Herbert A., Argus Res. Corp., 61 Broadway, New York 6, N.Y.
LEWIS, Hyman L., 1640 Portal Dr., N.W., Washington 12, D.C.
LEWIS, James A., 602 Westmanor Dr., Marianna, Fla. 32446
LEWIS, John A., Florida State Univ., Box U-2292, Tallahassee, Fla.
LEWIS, John E., Georgia State Col., Sch. of Bus. Admin., 33 Gilmer St., S.E., Atlanta, Ga. 30303
LEWIS, John P., USAID, APO, New York, N.Y. 09675
LEWIS, Kenneth A., Univ. of Rochester, Dept. of Econ., Rochester, N.Y.
LEWIS, Robert E., First Nat. City Bank, 399 Park Ave., New York, N.Y. 10022
LEWIS, Robert Miller, 20 Winthrop Dr., Riverside, Conn. 06878
LEWIS, Ronald J., 155 Green Garden Rd., Marquette, Mich. 49855
LEWIS, Stanley J., 2671 Harlem Rd., Cheektowaga, Erie City, N.Y. 14225
LEWIS, Stephen D., 711 Bolton Walk, Apt. 202, Goleta, Calif. 93017
LEWIS, Stephen R., Jr., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
LEWIS, W. A., 121 Broadmead, Princeton, N.J. 08540
LEWIS, William B., 9408 Russell Rd., Silver Spring, Md.
LEYDEN, Dennis R., West Virginia Univ., Col. of Com., Morgantown, W.Va.
LEYHAN, John C., 643 Lincoln Rd., Grosse Pointe, Mich. 48138
LI, Chen-Tuan, 3214-C S. Oaks Blvd., Dallas, Texas
LI, Choh-Ming, Chinese Univ. of Hong Kong, 677 Nathan Rd., Kowloon, Hong Kong
LI, Robert Chiu Fay, 234 Lafayette St., Newark, N.J.
LI, Tien En, Delaware State Col., Dept. of Econ., Dover, Del.
LIANG, Ching C., 3814 12th St., N.E., Washington, D.C. 20017
LIANG, (Mrs.) Florence K., 2 Washington Sq., Larchmont, N.Y. 10538
LIANG, Kuo-shu, 5, Lane 98, Chi-lin Rd., Taipei, Taiwan
LIANOS, Theodore P., 1606 Benehan St., Raleigh, N.C. 27605
LIAO, Shubert S., Col. of Com., Univ. of the East, Manila, Philippines
LIBBIN, Edwin M., 405 Strath Haven Ave., Swarthmore, Pa. 19081

LICHTBLAU, John H., 498 West End Ave., New York, N.Y.
LICHTENWALNER, Milton, Talkeetna, Alaska 99676
LIDDELL, Wingham J. H., 2726 Russell, Berkeley 5, Calif.
LIEB, John S., Allis-Chalmers Mfg. Co., Legal Div., P.O. Box 512, Milwaukee, Wis. 53201
LIEBENBERG, Maurice, 4528 Ellicott St., N.W., Washington 16, D.C.
LIEBERMAN, Alfred, 440 Brookside Ave., Roosevelt, L.I., N.Y. 11575
LIEBERMAN, Herbert B., K4 Kissam Rd., Peekskill, N.Y.
LIEBERMAN, Marvin, 99 Hillside Ave., New York, N.Y. 10040
LIEBHAFSKY, Erwin E., Univ. of Houston, Dept. of Econ. and Fin., Cullen Blvd., Houston, Tex. 77004
LIEBHAFSKY, Herbert H., Univ. of Texas, Dept. of Econ., Austin 12, Tex.
LIEBHOLZ, Constantine, 88-09 35th Ave., Flushing, N.Y. 11372
LIEBLING, Herman I., 6317 W. Halbert Rd., Bethesda, Md. 21811
LIEDHOLM, Carl E., Univ. of Nigeria, Econ. Dev. Inst., P.M.B. 1080, Enugu Campus, Enugu, Nigeria
LIEFMANN-KEIL, Elizabeth, Saarbrucken, Florastr. 2, Saarbrucken, West Germany
LIEM, Som, 900 Memorial Dr., Apt. B-22 West, Cambridge, Mass. 02138
LIEPE, Wolfram, Univ. of New Mexico, Econ. Dept., Albuquerque, N.M. 87106
LIESER, Tom K., 1825 New Hampshire Ave., N.W., Apt. 803, Washington, D.C. 20009
LIESON, William A., 1351 Main St., Springfield, Mass. 01101
LIEU, Hou-Shun, 28 Marshmallow Dr., Commack, L.I., N.Y. 11725
LIEW, Chong Kiew, 1067-D Monroe St., Albany, Calif.
LIGHTFOOTE, Major B., Alabama State Col., P.O Box 287, Montgomery, Ala. 36101
LIGHTMAN, Joseph M., 608 Lamberton Drive, Silver Spring, Md.
LILE, Stephen E., Route 1, Gracey, Ky. 42240
LILL, J. Lloyd, Jr., 18 Cullens Run, Pittsford, N.Y. 14534
LILLING, Walter, 324 Palmer Ter., Mamaroneck, N.Y.
LILLQUIST, Sven F., 2920 S. Sepulveda Blvd., Los Angeles 43, Calif.
LIM, Eun Jung, Econ. Planning Bd., ROKG, Tech. Co-op. Div., Seoul, Korea
LIM, Youngil, Univ. of Hawaii, Dept. of Econ., Honolulu, Hawaii 96822
LIMMER, Ezekiel, 812 Malcolm Dr., Silver Spring, Md.
LIN, An-Loh, Univ. of Rochester, P.O. Box 5424, Rochester, N.Y. 14627
LIN, Lin, State Univ. of New York, Buffalo, Foster Hall, Rm. 313, Buffalo, N.Y. 14214
LIN, Po-tsu, Univ. of Washington, Dept. of Econ., Seattle, Wash.
LINAMEN, Harold F., Anderson Col., Anderson, Ind.
LINARES, Julio, Dept. de Estudios Econ., Banco Central de Nicaragua, Managua, Nicaragua
LINCOLN, George A., Dept. of Soc. Sci., West Point, N.Y. 10996
LIND, Marcel, 32653 Seagate Dr., Palos Verdes Peninsula, Calif. 90274
LIND, Robert C., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
LIND, Roger W., Univ. of Maryland, Dept. of Econ., College Park, Md. 20742
LINDAHL, Martin L., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
LINDAUER, John H., Claremont Men's Col., Econ. Dept., Claremont, Calif. 91717
LINDBERG, B. A., 26 Lincoln Ave., Breezy Pt., West Amityville, N.Y. 11701
LINDBLOM, Charles E., 9 Cooper Rd., North Haven, Conn. 06473
LINDE, Carl Anders, Fregattvagen 3 NB., Lidingo 1, Sweden
LINDEMAN, Bruce, Duke Univ., Econ. Dept., Durham, N.C. 27706
LINDEMAN, John, Syracuse Univ., Center for Overseas O and R., 721 Ostrom Ave., Syracuse 10, N.Y.
LINDER, Staffan B., Bravallavagen 41, Djursholm, Sweden
LINDGREN, Jon G., 1007 E. Rollins, Columbia, Mo. 65201
LINDHOLM, Richard W., 1957 Potter St., Eugene, Oreg.
LINDMAN, Bertram H., Box 3421, San Roque Sta., Santa Barbara, Calif. 93105
LINDOW, Wesley, 1 Wall St., New York 15, N.Y.
LINDQUIST, C. Albin, 383 Warren St., Stewartsville, N.J. 08886
LINDSAY, Robert, 129 Sherwood Rd., Ridgewood, N.J.
LINDSEY, Norman L., 143 Kensington Dr., Utica, N.Y. 13501
LINDSEY, Quentin W., Ford Found., Post Box 81, Kathmandu, Nepal
LINDSLEY, Darwin Reid, 4351 Bloor St. W.#25, Etobicoke, Ont., Canada
LING, Suilin, Communications Satellite Corp., 2100 L St., N.W., Washington, D.C. 20037
LINGER, Irving O., Texas A. and M. Univ., Dept. of Econ., College Station, Tex.
LININGER, Charles A., Univ. of Michigan, Survey Res. Center, Ann Arbor, Mich.
LINK, Joseph, Jr., Xavier Univ., Cincinnati 7, Ohio
LINK, Robert G., 600 W. 116th St., New York 27, N.Y.
LINN, James J., Tulane Univ., Sch. of Bus. Admin., New Orleans, La. 70118
LINNANE, Hugh P., 145-32 159th St., Jamaica, N.Y. 11434
LINNEMANN, Hans, Pieter Langendykstraat 3, Papendrecht, Netherlands
LINNENBERG, Clem C., Jr., U.S. Public Health Service, 9000 Rockville Pike, Bethesda, Md. 20014
LINSTROMBERG, Robin, 1814 Timber Ct., Peoria, Ill.
LINTNER, John, 50 Tyler Rd., Belmont 78, Mass. 02019
LINTON, Douglas D., 9600 S. Bishop St., Chicago, Ill. 60643
LINTON, John Hunter, 2121 S. Columbus St., Arlington, Va. 22206
LION, Donor M., USAID/DOM, APO New York, N.Y. 09676
LIOU, James M., c/o H. G. Kailey and Co., Ltd., P.O. Box 555, Hong Kong
LIPKOWITZ, Eugene V., 135 E. 17th St., New York, N.Y. 10003
LIPKOWITZ, Irving, c/o Reynolds Metals Co., 19 E. 47th St, New York, N.Y. 10017
LIPPIT, Victor D., 309-L Mansfield St., New Haven, Conn. 06511
LIPPITT, Vernon G., 321 Hollywood Ave., Rochester 18, N.Y.
LIPSEY, R. G., Univ. of Essex, Sch. of Soc. Studies, Dept. of Econ., Wivenhoe Park, Colchester, Essex, England
LIPSEY, Robert E., Nat. Bur. of Econ. Res., 261 Madison Ave., 10th Fl., New York 16, N.Y.
LIPSKY, David B., 393 Delta Dr., Town of Amherst, N.Y. 14226
LIPSON, Frank, Box 792, Ft. Clayton, Canal Zone
LISCHER, (Mrs.) Shirley N., Creston Community Col., Creston, Iowa 50801
LISHAN, John M., 147 S. Poinsettia Place, Los Angeles 36, Calif.
LISSAK, Richard S., 124 Amity St., Brooklyn 1, N.Y.
LISSNER, Will, 3435 Giles Pl., Apt. 7-C, Bronx, N.Y. 10463
LIST, Claire, 1737 York Ave., New York, N.Y. 10028
LITHWICK, N. Harvey, Carleton Univ., Econ. Dept., Ottawa, Ont., Canada
LITMAN, Simon, 1108 S. Lincoln Ave., Urbana, Ill.
LITT, Benjamin, 2555 West St., Brooklyn, N.Y. 11223
LITTERER, Joseph A., Univ. of Illinois, Room 306 Commerce West, Urbana, Ill.
LITTLE, Wallace I., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
LITTLECHILD, Stephen C., Stanford Univ.,

Operations Res. House, 544 Alvardo Row, Stanford, Calif.
LITTLEFIELD, William, 1624 Lee Ave., Arcadia, Calif.
LITTLEJOHN, Edward, Pfizer International, Inc., 235 E. 42nd St., New York 17, N.Y.
LITTLEPAGE, Paxton K., 711 Texas Ave., Box 149, Mart, Tex. 76664
LITVACK, James M., 17 Everett St., Cambridge, Mass. 02138
LITWAK, Alfred, 350 E. 52nd St., New York 22, N.Y.
LIU, Charles Y., North Carolina State Univ., Dept. of Econ., 4-A Patterson, Raleigh, N.C. 27607
LIU, David Wei-Ping, Stout State Univ. of Wisconsin, Dept. of Econ., Menomonie, Wis.
LIU, Ernest S., 624 Cayuga Heights Rd., Ithaca, N.Y. 14850
LIU, Jung-Chao, *McGill Univ., Dept. of Econ.,* Montreal, P.Q., Canada
LIU, Ta-Chung, Cornell Univ., Dept. of Econ., Ithaca, N.Y.
LIU, Su Feng, Univ. of Illinois, Dept. of Agric. Econ., Urbana, Ill.
LIU, Yih-wu, 905 S. Oakland, Carbondale, Ill. 62901
LIVINGSTON, Don A., Southern Illinois Univ., Bus. Div., Edwardsville, Ill. 62025
LIVINGSTON, John, Ripon Col., Dept. of Econ., Ripon, Wis.
LIVINGSTON, Joseph A., Philadelphia *Bulletin, Business Outlook,* 30th and Market Sts., Philadelphia, Pa. 19101
LIVINGSTON, Richard B., 318 Highland Rd., South Orange, N.J. 07079
LIVINGSTON, S. Morris, 910 S. Michigan Ave., Room 1606, Chicago, Ill. 60680
LIVINGSTONE, Charles F., 1500 Massachusetts Ave., N.W., Apt. 855, Washington, D.C. 20005
LIZZERI, Giancarlo V., Via Venezia 28, Rivoltella, Brescia, Italy
LLOYD, Cliff L., Purdue Univ., Dept. of Econ., Lafayette, Ind.
LLOYD, Gary E., 1820 Harvard St., N.W., Washington, D.C. 20009
LLOYD, John W., 597 Garden Rd., De Kalb, Ill. 60115
LLOYD, Lewis E., Dow Chemical Co., Midland, Mich. 48640
LLOYD, Peter J., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
LLOYD, R. Grann, Tennessee Agric. and Ind. State Univ., Sch. of Arts and Sci., P.O. Box 329, Nashville, Tenn. 37203
LLOYD, Russell D., Colorado State Univ., Rocky Mtn. Ft. Range Exp. Sta., Ft. Collins, Colo. 80521
LLUCH, Constantino F., 3211 Allen St., #203, Falls Church, Va. 22042
LOBO, Orlando, c/o Int. Monetary Fund, Western Hemisphere Dept., Washington 25, D.C.
LOCKLEY, Lawrence, Univ. of Santa Clara, Col. of Bus. Admin., Santa Clara, Calif.
LOCKLIN, D. Philip, Univ. of Florida, Gainesville, Fla.
LOCKLING, William B., Univ. of Arizona, Dept. of Econ., Tucson, Ariz. 85721
LOCKNER, Allyn O., South Dakota State Univ., Dept. of Econ., Brookings, S.D. 57007
LOCKS, Mitchell O., 240 S. Canon Dr., Beverly Hills, Calif.
LOCKWOOD, Thornton C., Benton and Bowles, Ltd., 197 Knightsbridge, London, S.W. 7, England
LOCKWOOD, William W., Princeton Univ., Sch. of Pub. and Int. Aff., Princeton, N.J. 08540
LOCKYER, Charles R., P.O. Box 1037, Bangkok, Thailand
LOE, Myron L., 3310 S. 28th St., Alexandria, Va.
LOEFFLER, Richard W., 13 Mountain Ave., Bloomfield, N.J. 07004
LOESCHER, Samuel M., Indiana Univ., Econ. Dept., Bloomington, Ind.
LOEWECKE, Donald W., 3901 Longfellow St., Hyattsville, Md. 20781
LOEWENBERG, J. Joseph, 2511 Q St., N.W., Washington, D.C. 20007
LOWENSTEIN, Louis K., 3858 Jackson St., San Francisco, Calif. 94118
LOEWY, Harris, 5516 Uppingham St., Chevy Chase, Md. 21620
LOEWY, Sheldon D., 2801 South Pkwy., Apt. 1311, Chicago, Ill. 60616
LOFT, Jacob, Brooklyn Col., Dept. of Econ., Brooklyn 10, N.Y.
LOFTING, Everard, Univ. of California, 413 Hesse Hall, Berkeley 4, Calif.
LOGAN, Bernard S., Bridgewater Col., Dept. of Econ., Bridgewater, Va. 22812
LOGAN, James P., Columbia Univ., Grad. Sch. of Bus., New York, N.Y. 10027
LOGUE, Ruth, 1821 Summit Pl., N.W., Apt. 201, Washington, D.C. 20009
LOGUE, William L., 119 Dozier Ave., Titusville, Fla. 32780
LOH, Arthur T. Y., North Lane, Windmill Farm, Armonk, N.Y. 10504
LOH, (Mrs.) Monica C., North Lane, Windmill Farm, Armonk, N.Y. 10504
LOHMAN, Philipp H., 25 East End Ave., Apt. 2C, New York, N.Y. 10028
LOHRKE, Gene, Balaton, Minn. 56115
LOKKE, (Mrs.) Laura W., 1801 16th St., N.W., Washington, D.C. 20009
LOLLAR, John H., III, Shell Chemical Co., Agric. Chem. Div., Time and Life Bldg., 110 W. 51st St., New York, N.Y. 10020
LOMAN, Harry J., Wharton Sch., Univ. of Pennsylvania, Philadelphia, Pa.
LOMASNEY, W. F., 160 N. LaSalle St., Chicago, Ill.
LOMBARDO, Angelo J., New York Hanseatic Corp., 60 Broad St., 21st Fl., New York, N.Y. 10004
LOMBARDO, Heraclio A., P.O. Box 4359, San Jose, Costa Rica
LONCKI, Ronald E., Box 512, Manhattan Beach, Calif. 90268
LONG, Burl F., Pennsylvania State Univ., Dept. of Agric. Econ. and Rural Soc., 1 Weaver Bldg., University Park, Pa. 16802
LONG, C. Richard, Atlanta Fed. Res. Bank, Res. Dept., Atlanta, Ga. 30303
LONG, Dean, 1901 E. Mulberry, Evansville, Ind. 47714
LONG, H. Owen, Pensacola Junior Col., Pensacola, Fla. 32504
LONG, James B., 480 Snyder, Champaign, Ill.
LONG, John J., 5004 Euclid Dr., Kensington, Md. 20795
LONG, Louis J., Wells Col., Aurora, N.Y. 13026
LONG, Millard F., 5336 University Ave., Chicago, Ill. 60615
LONG, Pham Truong, Nat. Bank of Vietnam, 17, Ben Chuong Duong, Saigon, Vietnam
LONG, Pho Ba, Commercial Counsellor, Esso, Duong Thong Nhat, Saigon, Vietnam
LONG, Roger B., Univ. of Idaho, Dept. of Agric. Econ., Moscow, Idaho 83843
LONG, Stanley George, Knox Col., Dept. of Econ. and Bus. Admin., Galesburg, Ill. 61401
LONG, Thomas G., 1436 Chicago Blvd., Detroit, Mich.
LONG, Tran, Sch. of Polit. Econ. and Bus. Admin., Univ. of Dalat, Dalat, Vietnam
LONG, Wesley H., 323 S. Barnard St., State College, Pa. 16801
LONG, William F. E., George Washington Univ., Bldg. P, Rm. 20, Washington, D.C. 20006
LONGSTREET, James R., Univ. of Pennsylvania, Wharton Sch. of Fin., Philadelphia, Pa. 19104
LONGSTREET, Victor Mendell, 3337 Reservoir Rd., N.W., Washington, D.C. 20007
LONNSTROM, Douglas A., Orton Associates, Inc., 182 Washington Ave., Albany, N.Y. 12210
LOOMBA, Narendra P., Lehigh Univ., Col. of Bus. Admin., Bethlehem, Pa.
LOONEY, Lawrence J., Merrimack Col., Dept. of Econ., North Andover, Mass.
LOOSMORE, Robert J., 1252 Amesbrook Dr., Ottawa 5, Ont., Canada
LOOTENS, Donald M., 43-10 Kissena Blvd., Apt. 15-F, Flushing 55, Queens, N.Y.
LOPATA, Richard S., 330 Prospect, Highland Park, Ill.
LOPATA, Simon, 147-41 70th Ave., Flushing 67, N.Y.
LOPERA-PALACIOS, Jorge, Univ. of Wisconsin, Land Tenure Center, Madison, Wis. 53706

LOPES, Jose da Silva, Av. Magalhaes de Lime 4:rc:Esq., Lisbon 1, Portugal
LOPES, Lucas, Rua Raul Pompeia 228, Apt. 802, Rio de Janeiro 26, Guanabara, Brazil
LOPEZ, Raymond H., 61-28 Broadway, Woodside, N.Y. 11377
LOPEZ-HAURAT, N. Horacio, Roca 2044, Florida, Buenos Aires, Argentina
LOPEZ-PACHECO, Manual A., Apartado 234, Caracas, Venezuela
LOPEZ-SOLORZANO, Joaquin, 11ª Av. S. O. No. 212, Managua, Nicaragua
LORANT, John H., Douglass Col., New Brunswick, N.J.
LORCA, Alejandro, calle Encarnacion, Valencia 1, Spain
LORD, William B., 3411 15th St., N., Arlington, Va. 22201
LOREN, Eugene L., Univ. of California, Dept. of Econ., Los Angeles, Calif.
LORETO, Anthony G., 11 Jacque Ter., Whippany, N.J.
LORIE, James H., Univ. of Chicago, Sch. of Bus., Chicago, Ill. 60637
LORIG, Arthur N., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
LORR, George J., Jr., 140 N. Delaplaine Rd., Riverside, Ill.
LORWIN, Lewis L., 45 Christopher St., New York, N.Y. 10014
LORWIN, Val R., Univ. of Oregon, Dept. of Hist., Eugene, Oreg. 97403
LOSCHKY, David J., 367 Benefit St., Providence, R.I.
LOSER, Claudio, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
LOSMAN, Donald L., 2825 Alaska St., Baton Rouge, La. 70802
LOTFY, Sherif, Nat. Bank of Egypt, Research Dept., 35 Kass El-Nil St., Cairo, Egypt
LOTSHAW, Elmer P., Owens Illinois Glass Co., Marketing Devel. Dept., P.O. Box 1035, Toledo 1, Ohio
LOTT, William F., Ap. Q-4 Western Manor, 2300 Avent Ferry Rd., Raleigh, N.C.
LOTTNER, Sam, Xerox Corp., 1350 Jefferson Rd., Rochester, N.Y. 14623
LOUCKS, William, Univ. of Pennsylvania, Deitrich Hall, Philadelphia, Pa.
LOUD, Frederick V., 230 Farmington Ave., Apt. F-2, Hartford, Conn. 06105
LOUDON, Elizabeth M., 4508 Burlington Pl., N.W., Washington, D.C. 20016
LOUGHLIN, James C., Trinity Col., Dept. of Econ., Hartford, Conn. 06106
LOUHI, Kullervo, 888 Audubon Rd., East Lansing, Mich.
LOUIE, Richard, Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
LOUISE, Sister Genevieve, Col. of St. Rose, Albany 3, N.Y.
LOUNSBURY, John P., 153 Altamira Rd., Richmond Hill, Ont., Canada
LOUNSBURY, Raymond H., R.F.D. 1, Vergenes, Vt. 05491
LOUTFI, Mohamed A. L., 25 Avenida Dr., Berkeley, Calif. 94709
LOUX, Maynard, Rensselaer Poly. Inst., Sch. of Mgt., Troy, N.Y. 12181
LOVE, Joseph L., 4400 Red River St., Austin, Texas
LOVE, Stanley F., Rand McNally and Co., Box 7600, Chicago, Ill. 66680
LOVEDAY, Doug F., 3338 S. Sawtelle, #10, Los Angeles, Calif. 90066
LOVEJOY, Frederick A., Frederick Lovejoy Assoc., 114 Northwood, Storrs, Conn. 06268
LOVEJOY, Robert M., 3537 Old Vestal Rd., Vestal, N.Y. 13850
LOVEJOY, Wallace F., Southern Methodist Univ., Dept. of Econ., Dallas 22, Tex.
LOVELL, C. A. Knox, Duke Univ., Dept. of Econ., Durham, N.C.
LOVELL, Hugh G., General Extension Div., Portland Center, 1633 Southwest Park Ave., Portland 1, Ore.
LOVELL, Michael C., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh, Pa. 15213
LOVELESS, (Mrs.) Shirley M., 734 Azalea Dr., Rockville, Md. 20850
LOVELY, William A., Jr., 58 Constance Rd., West Roxbury, Mass. 02132
LOVENSTEIN, Meno, Ohio State Univ., Dept. of Econ., Columbus 10, Ohio
LOVERDOS, Alexander, Hotel Grande Bretagne, Athens, Greece
LOVETT, Robert E., 1286 Sierra Madre Blvd., San Marino, Calif. 92069
LOW, Richard E., 235 E. 22nd St., New York, N.Y. 10010
LOW, Robert G., 786 W. Griswold Rd., Covina, Calif.
LOWE, Adolph, 10 Park Ter. E., New York, N.Y.
LOWENTHAL, Esther, 21 Dryads Green, Northampton, Mass.
LOWENTHAL, Paul, 79, Avenue de Juillet, Brussels 15, Belgium
LOWERY, Dave, 894 N. Kensington St., Arlington, Va.
LOWETH, Robert S., 45 Argyle Rd., West Hampstead, N.Y. 11552
LOWINGER, Thomas C., 118 Woodmere Ave., East Lansing, Mich.
LOWMAN, Fern, Southwestern State Col., Dept. of Bus. Educa., Weatherford, Okla. 73096
LOWMAN, Rex Palmer, Gallaudet Col., Washington 2, D.C.
LOWRIMORE, Don D., 2 Barbara Dr., Wappingers Falls, N.Y. 12590
LOWRIMORE, William, III, 1288 Denlyn, Novato, Calif. 94947
LOWRY, Roye L., 668 S. Jefferson St., Arlington, Va. 22204
LOWRY, Stanley Todd, Washington and Lee Univ., Sch. of Com. and Admin., Lexington, Va. 24450
LOWTHIAN, Philip, 2167 S.W. Yamhill, Apt. 6, Portland, Ore. 97148
LOY, David C., 1726 W. 22nd St., Lawrence, Kans.
LOZADA, Gil C., 24 Schermerhorn St., Brooklyn, N.Y.
LU, Chuan Ting, 17, Li Hsien New Villa, National Chengchi Univ., Taipei Hsien, Taiwan
LU, Ch'ung-tai, Baldwin-Wallace Col., Div. of Com., Berea, Ohio 44017
LU, John Y., RAND Corp., 1700 Main St., Santa Monica, Calif. 90406
LUAN, David C., Southern Illinois Univ., 909 Ohio St., East St. Louis, Ill. 62201
LUBBERT, Jens I., Bastion 5, Rendsburg 237, Germany
LUBELL, Alfred, 5-19 149th St., Whitestone, N.Y. 13492
LUBELL, Harold, US AID/ANKARA, American Embassy, APO New York 09254
LUBIN, Isador, 41 E. 70th St., New York, N.Y. 10021
LUBIN, John F., Univ. of Pennsylvania, Computer Center, Philadelphia 4, Pa.
LUBITZ, Raymond, 804 Hamilton Hall, Columbia Univ., New York, N.Y. 10027
LUCAS, John W., Univ. of Omaha, Omaha, Nebr.
LUCHINI, Lawrence, 609 Woodworth, Alma, Mich. 48801
LUCK, Thomas J., 212 Fleetwood Dr., Bloomington, Ill. 61702
LUCKETT, Dudley G., Iowa State Univ., Dept. of Econ., Ames, Iowa
LUDEWIG, Gunter A., Damaschkestr. F III, 1 Berlin 31, Germany
LUDLOW, Howard T., 48 Ash Ter., Parlin, N.J. 08859
LUDLOW, William L., Muskingum Col., Dept. of Polit. Sci., New Concord, Ohio 43762
LUDMER, Henry, 134 Peach St., Park Forest, Ill. 60466
LUDT, R. E., c/o Celanese Corp. of Amer., 522 Fifth Ave., New York 36, N.Y.
LUDTKE, James B., Univ. of Massachusetts, Sch. of Bus. Admin., Amherst, Mass.
LUEY, Paul, Univ. of Hong Kong, Dept. of Econ. and Polit. Sci., Hong Kong
LUHMANN, Thomas C., 66 Plymouth Rd., East Rockaway, N.Y.
LUKACZER, Moses, 3607 Stewart Driveway, North Chevy Chase, Md. 20015
LUKE, Raymond H., USOM/Guatemala, c/o American Embassy, State Dept. Mail Room, **Washington 25, D.C.**

LUKER, William A., 1402 Laura Lane, College Station, Tex.
LUMPKIN, R. Pierce, 8414 Yolanda Rd., Richmond, Va. 23229
LUMSDEN, Keith G., Stanford Univ., Econ. Dept., Stanford, Calif. 94305
LUND, David C., 214 Bevier Rd., Piscataway, N.J. 08854
LUND, Peter B., 1844 Blake St., Apt. D, Berkeley, Calif. 94703
LUNDBERG, Erik, Borjesonsvagen 42, Bromma, Sweden
LUNDBERG, Kenneth Victor, Rhode Island Col., Providence 8, R.I.
LUNDE, Harold I., 16 Lewis Lane, Port Washington, N.Y. 11050
LUNDGREN, Allen L., 765 Redwood Lane, New Brighton, Minn. 55112
LUNDSTEDT, Paul C., 19210 Alcoy, Detroit, Mich. 48205
LUNDY, Robert F., Iowa Wesleyan Col., Mt. Pleasant, Iowa
LUNESKI, Chris, Univ. of Ore., Sch. of Bus. Admin., Dept. of Acctg., Eugene, Ore.
LUONG, Hoang Dinh, Ministry of Nat. Econ., 59, Gia Long, Saigon, Vietnam
LUPUL, Max E., San Fernando Valley State Col., Sch. of Bus. and Econ., Northridge, Calif. 91326
LUQUETTE, Norman A., Box 238, King's College, Wilkes-Barre, Pa. 18702
LURENSKY, Robert L., 3022 Wellington Rd., Alexandria, Va. 22302
LURIE, Melvin, Univ. of Rhode Island, Col. of Business Admin., Kingston, R.I. 02836
LURY, Dennis A., Univ. Col. of Nairobi, Univ. of East Africa, P.O. Box 30197, Nairobi, Kenya
LUSHER, David W., 4970 Battery Lane, Apt. 507, Bethesda, Md. 20014
LUSTIG, Owen J., USAID, Bolivia, Casilla 673, La Paz, Dept. of State Mail Room, Washington, D.C.
LUTFALLA, Michel, 21 Place des Vosges, Paris 3, France
LUTHER, Ernest W., 6941 Morgan Ave., S., Minneapolis 23, Minn.
LUTIN, David L., 74 Sonn Dr., Rye, N.Y. 10880
LUTOLF, Franz, Int. Bank for Recon. and Dev., 1818 H Street, N.W., Washington 25, D.C.
LUTZ, Friedrich, Keltenstrasse 6, Zurich, Switzerland
LUTZ, John Piper, Univ. of Pennsylvania, Wharton Sch., Philadelphia 4, Pa.
LUTZ, Rene A., c/o U.S. Consulate General, Zurich, Switzerland
LUTZ, Warren H., 1 Dartmouth Circle, Swarthmore, Pa. 19081
LUWE, Jack F., 1508-A Ocean Ave., Seal Beach, Calif. 90740
LUYTJES, Jan Bram, Drexel Inst. of Tech., Philadelphia 4, Pa.
LWIN, Sai Myint, Box 12169, Univ. Sta., Gainesville, Fla. 32601
LYALL, Katharine C., Brook Lane Apts., 1A5, 220 Triphammer Rd., Ithaca, N.Y.
LYDIA, Sister Mary, 5531 S. Karlov Ave., Chicago 29, Ill.
LYMBEROPOULOS, P. John, Univ. of Colorado, Sch. of Bus., Boulder, Colo. 80304
LYNCH, Boyd H., 5044 28th Ave., S., Seattle 8, Wash.
LYNCH, Daniel W., King's College, Wilkes-Barre, Pa.
LYNCH, David, 3679 Highwood Dr., S.E., Washington 20, D.C.
LYNCH, Edward A., Jr., 3937 McKinley St., N.W., Washington, D.C. 20015
LYNCH, Edward S., Garden Cottage, Wildwood Rd., Hartsdale, N.Y. 10530
LYNCH, Gary A., Univ. of Idaho, Col. of Bus. Admin., Moscow, Idaho 83843
LYNCH, James T., 856 Altura Way, Upland, Calif. 91786
LYNCH, Lawrence K., Univ. of Kentucky, Bur. of Bus. Res., Lexington, Ky. 40506
LYNCH, Michael P., 314 S. Henderson, Bloomington, Ind.
LYNCH, Ralph, St. Meinrad Col., St. Meinrad, Ind. 47577
LYNCH, Robert M., 38 Church Lane, North Kingstown, R.I.
LYNCH, Vernon E., Jr., 2407-H Sabine, Austin, Tex.
LYNIP, Franklin, The Philadelphian 5-C-47, 2401 Pennsylvania Ave., Philadelphia, Pa. 19130
LYNN, Arthur D., Jr., Ohio State Univ., 190 N. Oval Dr., Rm. 308, Columbus, Ohio 43210
LYNN, Gault W., Fed. Res. Bank of San Francisco, San Francisco, Calif. 94120
LYNN, James H., Fed.-Prov. Rela. Div., Dept. of Fin., Confederation Bldg., Ottawa, Ont., Canada
LYNN, Laurence E., 5543 Sanger Ave., Apt. 12, Alexandria, Va.
LYNN, Richard K., 19A Macondray Lane, San Francisco, Calif. 94133
LYNN, Robert Athan, 200 Grover, Warrensburg, Mo. 64093
LYNSKEY, Joseph V., Bansley and Kiener, 300 W. Washington St., Chicago, Ill. 60606
LYON, Richard M., Seyfarth, Shaw, Fairweather, 111 W. Jackson Blvd., Chicago, Ill. 60604
LYON, Robert, The Windsor, Apt. 1206, 17th and Parkway, Philadelphia, Pa. 19103
LYONS, Will, 86 Northampton St., Amherst, Mass. 01002
LYPECKYJ, Roman G., 18087 Gable Ave., Detroit 34, Mich.
LYTTON, Henry D., 1530 33rd St., N.W., Washington, D.C. 20007

MA, Yuen-Cheung, Tusculum Col., Greeneville, Tenn.
MAAFO, Eugene V. A., Box 8541, Forest Hills Sta., Durham, N.C. 27707
MAALI, Bagindo R., Djl. Dr. Muwardi V No. 6, Grogol, Djakarta, Indonesia
MABRY, Bevars D., Bowling Green Univ., Dept. of Econ., Bowling Green, Ohio 43402
MACAULAY, Fred R., 11 Clover Dr., Great Neck, N.Y.
MACAULAY, Hugh H., Box 72, Clemson, S.C.
MACBETH, Thomas G., Lowell Tech. Inst., Dept. of Econ. and Mgt., Lowell, Mass.
MACCALLUM, David E., "Werribon," Willow Tree, N.S.W., Australia
MACCINI, Louis J., 2006 Sherman Ave., Evanston, Ill. 60201
MacCRIMMON, Kenneth R., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh, Pa. 15213
MacDONALD, Gordon D., Real Estate Board of New York, 12 E. 41st St., New York 17, N.Y.
MACDONALD, Hugh I., 950 Yonge St., Toronto 5, Ont., Canada
MacDONALD, Lois, Brookville Hollow Rd., Stockton, N.J. 08559
MACDONALD, Mado, Dartmouth Col., Amos Tuck Sch., Hanover, N.H. 03755
MacDONALD, Richard B., Box 86, Palmer, P.R. 00721
MACDONALD, Robert M., Dartmouth Col., Amos Tuck Sch., Hanover, N.H. 03755
MACDONALD, Ronan G., 5634 Delafield Ave., New York 71, N.Y.
MACDONALD, Wendell D., 18 Oliver St., Boston 10, Mass.
MacDOWELL, George F., Brandon Col., Dept. of Econ., Brandon, Man., Canada
MacDOWELL, H. W., New York Univ. Grad. Sch. Bus. Adm., 90 Trinity Pl., New York 6, N.Y.
MACE, Almon Turley, 909 N. Montana St., Arlington, Va.
MACE, Arthur E., University Arms Apts., 1040-1050 Ferry St., Eugene, Ore. 97401
MacEACHERN, Gordon A., Carleton Univ., Agric. Econ. Res. Coun. of Canada, Ottawa, Ont., Canada
MacEACHRON, David W., Council on For. Rela., 58 E. 68th St., New York 21, N.Y.
MACESICH, George, Florida State Univ., Dept. of Econ., Tallahassee, Fla.
MacEWAN, Arthur, Harvard Univ., Dept. of Econ., Rm. 404, 1737 Cambridge St., Cambridge, Mass. 02140
MacFARLANE, Dougald A., Seattle Univ., Sch. of Com. and Fin., Seattle, Wash. 98122
MacGOWAN, Thomas G., 295 Wyant Rd., Akron 13, Ohio
MacGREGOR, Donald C., Univ. of Toronto, Dept. of Polit. Econ., Toronto 5, Ont., Canada

ACH, Anthony, 151 Pennsylvania Ave., Westminster, Md. 21157
ACHADO, Alfredo, Piso 13 Edificio Polar, Plaza Venezuela, Caracas, Venezuela
ACHANDER, Louis N., 1402 E. Santa Clara, Santa Ana, Calif. 92701
ACHLUP, Fritz, Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
acINNIS, John S., 1828 Delaware St., Berkeley 3, Calif.
acIVER, Roderick E., 412 Read Dr., Lafayette, Calif. 94549
ACK, Edgar M., R.D. 2, Waynesburg, Pa. 5370
ACK, Ruth P., 430 W. 116th St., New York 27, N.Y.
ACKALL, Emily P., R.D. 1, Rogers, Ohio 14455
acKELVIE, V. S., Dept. of Labour Lib., Ottawa 4, Ont., Canada
acKENZIE, T. Findlay, 33 Franklin Ave., Croton-on-Hudson, N.Y.
ACKEY, (Mrs.) Judith R., Townsend Letter, 84 Strawtown Rd., West Nyack, N.Y.
ACKINNON, Allan W., Univ. of Maine, South Stevens, Rm. F, Orono, Me. 04473
ACKINTOSH, William A., Queen's Univ., Kingston, Ont., Canada
acLAURY, Bruce K., 381 Rock Rd., Glen Rock, N.J. 07028
acLEISH, John E., 134 S. LaSalle St., Chicago, Ill.
ACLEOD, Betty B., 78 Cassandra Blvd., Suite 5, Don Mills, Ont., Canada
acLEOD, William M., 557 Melbourne Ave., Ottawa, Ont., Canada
ACLIN, Luttrell, Paine, Webber, Jackson and Curti, 25 Broad St., New York 4, N.Y.
acMULLEN, Jorge E., Feo Acuna de Figueroa 590, Buenos Aires, Argentina
acNAB, John E., 4 Whitehill Ave., Ottawa 5, Ont., Canada
acNABB, Richard R., 4000 Massachusetts, Apt. 1308, Washington, D.C.
ACON, Hershal L., 3425 Timberlake Rd., S.W., Knoxville, Tenn. 37920
acPHERSON, Ronald B., DuPont Co. of Canada, Ltd., Box 660, Montreal, P.Q., Canada
acRAE, Donald A., Mankato State Col., Sch. of Bus., Mankato, Minn. 56001
acRAE, Duncan, Massachusetts Inst. of Tech., E52-274b, Cambridge, Mass. 02139
acRAE, Duncan, Jr., Univ. of Chicago, Dept. of Pol. Sci., Chicago, Ill. 60637
ACY, Bruce W., 4500 W. 77th St., Prairie Village 15, Kans.
ADAIAH, Madappa, Southern Illinois Univ., Dept of Econ., Carbondale, Ill.
ADDEN, Carl H., U.S. Chamber of Commerce, 1615 H St., N.W., Washington, D.C.
ADDEN, John J., Univ. of Guelph, Dept. of Soc. Sci., Guelph, Ont., Canada
ADDOX, Grace B., 4000 Massachusetts Ave., N.W., Apt. 1020, Washington 16, D.C.
ADDOX, James G., North Carolina State Univ., Dept. of Econ., Raleigh, N.C. 27607
ADDOX, James R., 6463 S. Ulster St., Englewood, Colo. 80110
ADEJ, John E., 7109 N. Mankato Ave., Chicago, Ill. 60646
ADIGAN, Robert M., 18 Marianne Rd., Darien, Conn.
ADJID, Abdul H., 75 Chestnut St., Boston, Mass. 02108
ADDO, Reynold B., 220 Montgomery St., Apt. 24-A, Brooklyn, N.Y.
ADRID, Carlos, Jr., 230 Tammy Dr., San Antonio, Tex. 78216
ADSEN, Albert G., Colorado State Univ., Dept. of Econ., Ft. Collins, Colo. 80521
ADSON, Johan A., 2010 Wisconsin St., Apt. 107, Oshkosh, Wis
AGALHAES, Luiz, V. N., 4822 S. 29th St., Apt. B-2, Arlington, Va. 22206
AGANA, Alvaro A., 45 Avenida Sur No. 405, San Salvador, El Salvador
AGDOFF, Harry Samuel, 555 Madison Ave., New York, N.Y. 10022
AGEE, Stephen P., 1407 Westgate, Cambridge, Mass 02139
AGIED, Awad A., Ministry of Defence, Khartoum, Rep. of Sudan

MAGLALANG, (Mrs.) Florencia D., Program Implementation Agency, 1440 Arlegui St., Manila, Philippines
MAGLOIRE, Raymond, 175-25 Devonshire Rd., Jamaica Estates, L.I., N.Y. 11432
MAGNUS, Erna F. R., Charleston Hall, 4534 N. Charles St., Apt. F, Baltimore 10, Md.
MAGRABI, (Mrs.) Frances, 656 Abbott Rd., East Lansing, Mich.
MAGRATH, C. B., 199 Birch St., Winnetka, Ill.
MAGRUDER, Bailey, 13638 S. Parnell, Riverdale, Ill. 60627
MAH, Feng-Hwa, Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
MAH, Theodore Mun Q., 2834 Loyola Ave., Richmond 10, Calif.
MAHAN, Howard F., Univ. of Southern Alabama, 307 Gaillard Dr., Mobile, Ala.
MAHAR, James F., 185 Atherwood Ave., Redwood City, Calif. 94061
MAHDI, Syed I., Univ. of Massachusetts, Dept. of Econ., Machmer Hall, Amherst, Mass.
MAHER, Edward D., Univ. of New Brunswick, Dept. of Bus. Admin., Fredricton, N. B., Canada
MAHER, John E., Joint Coun. on Econ. Educa., 1212 Ave. of the Americas, New York, N.Y. 10036
MAHER, William E., 248-07 Van Zandt Ave., Douglaston, N.Y. 11362
MAHHOUK, Adnan, Kuwait Fund for Arab Econ. Dev., Kuwait, Kuwait
MAHLER, Walter R., Jr., 392 Pathway Manor, Wyckoff, N.J.
MAHMOUD, Ibrahim K., Bus. Admin. and Acctg. Firm, P.O. Box 809, Monrovia, Liberia
MAHMOUD, Mabid A.M., 308 Westwood Plaza, Los Angeles, Calif. 90024
MAHON, Francis H., 300 E. 33rd St., Apt. 7L W., New York, N.Y. 10016
MAHONEY, (Mrs.) Bette S., 137 Tennessee Ave., Washington, D.C. 20002
MAHONEY, Edward A., 1200 Holmes Ave., Charlottesville, Va. 22901
MAHONEY, Thomas A., Univ. of Minnesota, Ind. Rela. Center, Minneapolis 14, Minn.
MAI, Ludwig H., St. Mary's Univ., San Antonio, Tex.
MAIER, Frank H., 5647 S. Maryland Ave., Chicago, Ill. 60637
MAIGUASHCA, Franklin E., P.O. Box 7507, Stanford, Calif. 94305
MAIN, Oscar W., 33 St. George St., Toronto 5, Ont., Canada
MAIN, Robert K., 450 Iropuois Dr., Bowling Green, Ky.
MAIR, George F., Smith Col., Wright Hall, Northampton, Mass.
MAISEL, Sherman J., Bd. of Governors, Fed. Res. System, Washington, D.C.
MAIZY, George Y., 11979-7 Rochester Ave., Los Angeles, Calif. 90025
MAJOR, David C., Harvard Univ., Leverett House G-107, Cambridge, Mass. 02138
MAJSTOROVICH, Branko, 20 Palisade Dr., Oak Brook, Ill. 60523
MAK, William K., 10707 Meadowhill Rd., Silver Spring, Md.
MAKI, Wilbur R., Timberlane Heights, R. R. 3, Ames, Iowa 50012
MALACH, Vernon W., 13 Boswell St., Belleville, Ont., Canada
MALAKOFF, A. L., WHO, Ave. Appia, Geneva, Switzerland
MALAMUD, Bernard, 470 W. 24th St., New York, N.Y. 10011
MALATESTA, Mary M., 5032 N. Park, Indianapolis 5, Ind.
MALCUIT, Stanley V., Aluminum Co. of America, 1501 Alcoa Bldg., Pittsburgh 19, Pa.
MALDONADO, Rita M., 500 Riverside Dr., New York 27, N.Y.
MALDUTIS, Julius, Jr., 795 Portion Rd., Lake Ronkonkoma, L.I., N.Y. 12108
MALENBAUM, Wilfred, Univ. of Pennsylvania, Wharton Sch., Dietrich Hall, Philadelphia, Pa.
MALET, Diego, P.O. Box 4391, San Juan 22, P.R.
MALI, Kuldip Singh, 862 S. East St., Jacksonville, Ill.
MALIET, Leonard, Univ. of Detroit, Dept. of Econ., Col. of Com. and Fin., Detroit 21, Mich.

MALIK, Mukhtar A., Canadian Welfare Council, 55 Parkdale Ave., Ottawa, Ont., Canada
MALISOFF, Harry, Brooklyn Col., Brooklyn, N.Y. 11210
MALKIEL, Burton Gordon, 84 Dempsey Ave., Princeton, N.J. 08540
MALKO, John R., 1012 S. Bouldin St., Baltimore, Md. 21224
MALLAN, (Mrs.) Lucy B., 6005 Ramsgate Rd., Washington, D.C. 20016
MALLEN, Bruce, Sir George Williams Univ., Commerce Faculty, Montreal 25, P.Q., Canada
MALLEY, Raymond Charles, U.S. AID Mission, c/o U.S. Embassy, APO New York 09271
MALM, F. Theodore, Univ. of California, Grad. Sch. of Bus. Admin., 310 Barrows Hall, Berkeley, Calif. 94720
MALMGREN, Harald B., Office of the Spec. Rep. for Trade Negotiations, Exec. Office of the Pres., Washington 25, D.C.
MALONE, Carl C., 32 Ferozshah Rd., New Delhi 1, India
MALONE, John R., Univ. of Notre Dame, Col. of Bus. Admin., Notre Dame, Ind. 46556
MALONE, Michael, Washington and Jefferson Col., Dept. of Econ., Washington, Pa. 15301
MALONEY, Harry D., 705 Highwood, Greencastle, Ind. 46135
MALOON, James H., 1931 Arlington Ave., Columbus, Ohio 43212
MALOSKEY, Paul, St. Francis Col., Loretto, Pa. 15940
MALOVANY, Gerald M., 1845 Harvard St., N.W., Apt. 821, Washington, D.C. 20009
MAMALAKIS, Markos J., Yale Univ., Dept. of Econ., Box 1987, Yale Sta., New Haven, Conn.
MAMER, John W., Univ. of California, 319 Giannini Hall, Berkeley 4, Calif.
MANCHESTER, Alden C., Dairy Sec. Med., Econ. Research Serv., U.S. Dept. of Agric., Washington 25, D.C.
MANDEL, Allan S., 110 D St., S.E., Apt. 209, Washington, D.C.
MANDEL, Bernard, 318 Thomas St., N., Arlington, Va. 22203
MANDELL, Daniel D., 28-14 29th St., Astoria, N.Y. 2, N.Y.
MANDELL, Maurice I., Bowling Green State Univ., Dept. of Bus. Admin., Bowling Green, Ohio 43402
MANDELL, Stuart L., 52 Columbus Ave., Haverhill, Mass. 01830
MANDELSTAMM, Allan Beryle, Michigan State Univ., Econ. Dept., East Lansing, Mich.
MANDES, Stephen, St. Francis Col., Dept. of Bus. Admin., Loretto, Pa.
MANDLE, Jay R., 5457 Morse St., Philadelphia, Pa. 19131
MANES, Charles V., 5907 Grayson St., Springfield, Va.
MANESCHI, Andrea, Yale Univ., Dept. of Econ., Econ. Growth Center, Box 1987, Yale Sta., New Haven, Conn.
MANESE, Carlos R., 1366 Monterey Ct., Chula Vista, Calif. 92011
MANFRA, Modestino R., Via San Felice 16, Bologna, Italy
MANGAS, Cloyce L., Opns. Div., Hq. USAREUR, APO 09403, New York, N.Y.
MANGUM, Fred A., Jr., Ist. di Econ. e Pol. Agraria, della Univ. di Perugia, Perugia, Italy
MANGUM, Garth L., 3310 Weller Rd., Silver Spring, Md.
MANGUNSONG, Iskander M., Kramat Kwitang I-H/10, Djakarta IV/11, Indonesia
MANHART, Lewis F., #3 Valley View Dr., Bowling Green, Ohio 43402
MANHERTZ, Huntley G., 417-D Oakmont Ave., Greensboro, N.C.
MANIYAR, Vinod Premchand, 91 Chestnut St., Brookline, Mass. 02146
MANKIN, Wyatt B., 5611 S. Blackstone Ave., Chicago, Ill. 60637
MANKOUSKI, Peter, 17 Guilden St., New Brunswick, N.J. 08901
MANLEY, Robert E., 3801 Carew Tower, Cincinnati, Ohio
MANLY, Lewis Frederick, Tufts Col., Medford, Mass. 02155
MANN, Arthur J., Darlington #1008, Mayaguez, P.R. 00708
MANN, Everett J., USAID, c/o American Embassy, APO New York 09676
MANN, Fritz Karl, 3713 Williams Lane, Washington 15, D.C.
MANN, H. Michael, Boston Col., Econ. Dept., Chestnut Hill, Mass. 01012
MANN, J. S., Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
MANN, Karl O., Univ. of Toledo, Dept. of Mgt., Toledo 6, Ohio
MANN, Martin, 2005 E. 54th St., Brooklyn 34, N.Y.
MANN, Maurice, Fed. Res. Bank of Cleveland, Research Dept., Cleveland, Ohio 44101
MANN, Patrick C., R. #2, Ossian, Ind.
MANNE, Henry G., George Washington Univ., Law Sch., 720 20th St., N.W., Washington 6, D.C.
MANNING, Bayless, Yale Univ., Law Sch., New Haven, Conn.
MANNING, Sarah L., 132 Waldron St., West Lafayette, Ind. 47906
MANNING, Travis W., Univ. of Alberta, Dept. of Agric. Econ., Edmonton, Alta., Canada
MANNINO, Ronald C., 5940 N. Kenmore Ave., Chicago, Ill. 60626
MANSFIELD, Edwin, Univ. of Pennsylvania, Wharton Sch., Dept. of Econ., Philadelphia, Pa 19104
MANSFIELD, Lawrence F., USAID, American Embassy, Conakry, Guinea
MANSFIELD, Michael, 225 E. 45th St., New York 17, N.Y.
MANSFIELD, Norman D., 905 Buckingham Dr., Silver Spring, Md. 20901
MANSON, Julius J., New York Board of Mediation, 270 Broadway, New York 7, N.Y.
MANTEL, Samuel J., Jr., Case Inst. of Tech., Div. of Org. Sci., University Circle, Cleveland 6, Ohio
MANTELL, Edmund H., 565 W. 169th St., New York 32, N.Y.
MANTELL, Leroy H., P.O. Box 736, Kennebunkport, Me. 04046
MANTHY, Robert S., 392 Westbourne Dr., Broomall, Pa. 19008
MANUS, Peter C., 330 W. 28th St., 2-F, New York 1, N.Y.
MANZOLI, (Mrs.) Mary K., 730 24th St., N.W., Apt. 818, Washington 7, D.C.
MAO, James C. T., Stanford Univ., Dept. of Statis., Stanford, Calif.
MAPA, Placido, Jr., Program Implementation Agency, Gov. of the Philippines, Manila, Philippines
MAPES, Clarel B., 300 Tulsa Bldg., Tulsa 3, Okla.
MARANKA, Gerold H., 137 N. Waverly Rd., Lansing, Mich. 48917
MARAR, Elias Y., Jordan Development I Planning Div., Amman, Jordan
MARBERRY, Charles E., State Univ. of Iowa, Col. of Com., Dept. of Gen. Bus., Iowa City, Iowa
MARBLE, George Downs, Ashland Col., Ashland, Ohio 44805
MARBURG, Theodore F., 5835 W. Trenton Pl., Milwaukee 13, Wis.
MARCH, Eli P., 11215 Bybee St., Silver Spring, Md.
MARCH, James G., Univ. of California, Irvine, Calif. 92650
MARCH, Michael S., 3233 Park View Rd., Chevy Chase, Md. 20015
MARCHAK, Thomas G., 11 Tompkins Ave., Babylon, N.Y.
MARCIS, Richard G., Univ. of Kentucky, Col. of Com., Box 8, Lexington, Ky. 40506
MARCOS-MARCOS, Fernando S., Pirineos 690, Colonia Roma, Monterrey, N.L., Mexico
MARCOUX, Dale, Washburn Univ., Topeka, Kans.
MARCUS, Edward, 870 United Nations Plaza, New York, N.Y. 10017
MARCUS, Lucille, 220 E. 73rd St., New York 21, N.Y.
MARCUS, Matityahu, Rutgers Univ., Dept. of Econ., New Brunswick, N.J. 08903
MARCUS, Mildred R., 870 United Nations Plaza, New York, N.Y. 10017
MARCUS, Philip, Univ. of North Dakota, Dept. of Economics, Grand Forks, N.D.

MARCUS, Sumner, Univ. of Washington, Seattle, Wash. 98105
MARCUSE, William, 17 Douglas Rd., Lexington, Mass. 01054
MARECEK, Karel V., P.O. Box 712, Shepherdstown, W.Va. 25443
MARENGO, Louis, R. 2, Box 376, McLean, Va. 22101
MARES, V. E., Pennsylvania State Univ., Dept. of Econ., 231 Boucke Bldg., University Park, Pa. 17774
MARGLIN, Stephen A., Massachusetts Inst. of Tech., Center for Int. Studies, 50 Memorial Dr., Cambridge 39, Mass.
MARGOLIN, Edward, 8500 Freyman Dr., Chevy Chase, Md. 21620
MARGOLIN, Raphael, 1124 5th St., Apt. 301, Santa Monica, Calif. 90903
MARGOLIS, Herman A., Purdue Univ., Dept. of Econ., West Lafayette, Ind. 47178
MARGOLIS, Julius, Stanford Univ., Dept. of Econ., Stanford, Calif. 94305
MARGOLIS, Philip, 7117 Boxford Rd., Baltimore 15, Md.
MARGOSHES, Sanford L., Box 1065, Tehran, Iran
MARHEFKA, Roger P., 968 New York Ave., Brooklyn, N.Y. 11203
MARIE, Sister Charles, Col. of St. Mary of the Springs, Columbus, Ohio 43219
MARIMONT, Martin L., 11512 Yates St., Wheaton, Md. 20902
MARIN, Enrique, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
MARION, Gerald, Univ. of Montreal, Dept. des Relations Industrielles, C.P. 6128, Montreal 3, P.Q., Canada
MARITANO, Nino, Col. of St. Thomas, Dept. of Econ., St. Paul 1, Minn.
MARK, Adolph E., 1216 Harvard Ter., Evanston, Ill. 60202
MARK, Jerome A., 11207 Lombardy Rd., Silver Spring, Md. 20901
MARK, Louis, Jr., American Embassy, Montevideo, Uruguay
MARK, Shelley M., 2036 Keeaumoku St., Honolulu, Hawaii
MARKER, Gordon A., Joint Center, CVG: Edifio Shell, Apartado 7000, Caracas, Venezuela
MARKHAM, Jesse W., Princeton Univ., Dept. of Econ., Dickenson 223, Princeton, N.J. 08540
MARKIEWICZ, Stanislaus J., 99 Freeman St., Brooklyn, N.Y. 11222
MARKLE, Herbert John, 1280 W. Crestwood Dr., Memphis 17, Tenn.
MARKO, Nicholas D., c/o Industrial Bank of Sudan, P.O. Box 1722, Khartoum, Sudan
MARKOVIC, Mihailo, 139-15 83rd Ave., Apt. 721, Jamaica, N.Y. 11435
MARKOVITZ, Jerome L., 1032 Bankers Securities Bldg., Philadelphia 7, Pa.
MARKWALDER, Don, 1400 South D, Fairfield, Iowa 52556
MARLIN, John A., 2137 F St., N.W., Washington, D.C. 20037
MARONI, Yves R., 2928 Pine Spring Rd., Falls Church, Va. 22042
MAROULIS, Angelos C., 5 Lycavittou St., Athens 135, Greece
MARPLE, Wesley W., Jr., Doctoral Program House, Harvard Bus. Sch., Boston, Mass. 02163
MARQUARD, Elva, 2400 Pennsylvania Ave., N.W., Washington 7, D.C.
MARQUARDT, Mary, 630 Clark St., Big Rapids, Mich. 49307
MARQUINEZ-L, Horacio O., Ave. Peru #72, Apt. 20, Panama, Panama
MARSCHAK, Jacob, Univ. of California, Western Mgt. Sci. Inst., Los Angeles 24, Calif.
MARSCHAK, Thomas, Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
MARSH, Charles Franklin, 148 Wofford Campus, Spartanburg, S.C. 29303
MARSH, Donald Bailey, Royal Bank of Canada, Head Office, Place Ville Marie, Montreal, P.Q., Canada
MARSH, Joseph F., P.O. Box 545, Athens, W.Va. 24712
MARSHALL, A. R., McGill Univ., Sch. of Com., Montreal 2, P.Q., Canada
MARSHALL, F. Ray, Univ. of Texas, Dept. of Econ., Austin, Tex.
MARSHALL, George, 800 Bel-Air Rd., Los Angeles, Calif. 90024
MARSHALL, Harold E., 9423 Georgetown Pike, Great Falls, Va. 22066
MARSHALL, Howard D., Vassar Col., Poughkeepsie, N.Y.
MARSHALL, Leon Carroll, 3111 Rolling Rd., Chevy Chase, Md. 20015
MARSHALL, Robert H., Univ. of Arizona, Dept of Econ., Tucson, Ariz.
MARSTON, Alfred J., 170 Park Row, Apt. 25-B, New York, N.Y. 10038
MARTEL, Arthur Hervey, 2277 Union Ave., Apt. 605, Mayfair Apts., Memphis, Tenn. 38104
MARTELLARO, Joseph A., 1020 N. Twyckenham Dr., South Bend, Ind. 46617
MARTIN, Bernard L., Eastern Michigan Univ., Col. of Bus., Dept. of Mktg., Ypsilanti, Mich. 48197
MARTIN, David Dale, Indiana Univ., Grad. Sch. of Bus., Bloomington, Ind. 47405
MARTIN, Donald L., Univ. of California at Los Angeles, Inst. of Gov. and Public Affairs, Los Angeles, Calif. 90024
MARTIN, Edward E., c/o U.S. Mission, 6th Round of GATT, 80 Rue de Lausanne, Geneva, Switzerland
MARTIN, Gilles M., 2659 Brome, Ste. Foy, P.Q., Canada
MARTIN, Harold O., 8 Whittier Pl., Apt. 6C, Boston, Mass 02114
MARTIN, Howard N., Ch. of Com., P.O. Box 53600, Houston, Tex. 77052
MARTIN, James W., 241 Tahoma Rd., Lexington, Ky.
MARTIN, John Allen, P.O. Box 966, Sherbrooke, P.Q., Canada
MARTIN, John W., Washington Col., Chestertown, Md. 21620
MARTIN, Kenneth F., 413 S. Gilbert St., Ada, Ohio 45810
MARTIN, Lee Roy, 1041 Arlington, Ann Arbor, Mich. 48104
MARTIN, Leonard W., 1136 Wincanton Dr., Deerfield, Ill. 60015
MARTIN, Margaret E., 1510 N. Herndon St., Arlington 1, Va.
MARTIN, Murray J., 4021 Overbrook Dr., Nashville, Tenn. 37204
MARTIN, Richard S., 148 Red Gate Lane, Amherst, Mass.
MARTIN, Robert L., 3640 E. County Rd., Odessa, Texas
MARTIN, Robert L., Middle Tennessee State Col., Murfreesboro, Tenn.
MARTIN, Robert W., First Old State Bank, Elkhart, Ind. 46515
MARTIN, Thomas Robert, Marquette Univ., Col. of Bus. Admin., Milwaukee 3, Wis.
MARTINELLI, Gerald J., 86-20 101st Ave., Ozone Park, N.Y. 11416
MARTIRENA-MANTEL, (Mrs.) Ana Maria, Balcarce 284, Acassuso, Prov. de Buenos Aires, Argentina
MARTINEZ, Miguel, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
MARTINEZ, Ricardo C., 1513 N. Rhodes St., Apt. 3, Arlington, Va. 22209
MARTINEZ-YBOR, Ignacio, Jr., Univ. of Wisconsin, Econ. Dept., Madison, Wis.
MARTINSEK, (Mrs.) Catherine W., 515 N. Oakland, Carbondale, Ill. 62901
MARTINSEK, Thomas A., 515 N. Oakland, Carbondale, Ill. 62901
MARTWICK, Richard I., 3804 Greystone Ave., New York 63, N.Y.
MARTY, Alvin L., 545 West End Ave., Apt. P.H.W., New York 24, N.Y.
MARTY, Robert, 6346 King Louis Dr., Alexandria, Va. 22312
MARUYAMA, Yoshihiro, Hokkaido Univ., Dept. of Agric. Econ., Sapporo, Japan
MARWAH, Kanta, Boston Col., Dept. of Econ., Chestnut Hill, Mass. 02167
MARX, Alan L., 609 W. 114th St., Apt. 96, New York, N.Y. 10025
MARX, Daniel, Jr., 14 North Balch St., Hanover, N.H. 13755
MARX, Frederic, 1444 Rock Creek Ford Rd., Washington, D.C.

MARX, Karl B., Western Illinois Univ., Dept. of Soc. Sci., Macomb, Ill. 61455
MARX, Roy L., 5319 31st Rd. N., Arlington, Va.
MARZ, Edward, 45 Obkirchergasse, Vienna 19, Austria
MARY, Sister Margaret, 5515 Franklin Ave., Los Angeles 28, Calif.
MASCO, Francis H., 6229 31st N.E. Seattle, Wash. 98115
MASCOTTI, Paul J., Elmwood Apt. 0-1, Kalamazoo, Mich. 49001
MASIH, Nolin, St. Cloud State Col., St. Cloud, Minn. 56301
MASO, Sebastian, "Kelston", 159 Nightingale Lane, London, S.W. 12, England
MASON, Edward S., 9 Channing St., Cambridge, Mass. 01922
MASON, Jarvis W., 58 Dover Rd., West Hartford 7, Conn.
MASON, John D., 403 Clinton St., Stockbridge, Mich. 49285
MASON, John L., Univ. of Toledo, Dept. of Mktg., Toledo, Ohio 43606
MASON, Joseph R., 72 Cynthia Dr., Raynham, Mass. 02767
MASON, R. Hal, 3280 South Ct., Palo Alto, Calif.
MASON, Will E., 251 E. Irvin Ave., State College, Pa. 16801
MASON, William F., 515 8th St., Brooklyn 15, N.Y.
MASSARIK, Fred, Univ. of California, Los Angeles, Calif.
MASSE, Isidore J., Loyola Col., Montreal, P.Q., Canada
MASSEL, Mark S., Brookings Institution, 1775 Massachusetts Ave. N.W., Washington 6, D.C.
MASSELL, Benton F., Univ. Col., Centre for Econ. Res., P.O. Box 30197, Nairobi, Kenya
MASSENGALE, George P., 7456 Whitehaven, St. Louis 23, Mo.
MASSEY, Mildred, Los Angeles State Col., Dept. of Bus. and Econ., Los Angeles 32, Calif.
MASSIMO, William G., 21-41 34th Ave., Long Island City, N.Y. 11106
MASSON, Claude, Laval Univ., Dept. of Econ. and Soc. Sci., Ste.-Foy, Quebec 10, P.Q., Canada
MASSON, Francis G., 5527 Nevada, N.W., Washington 15, D.C.
MASSOUH, Farid B., 2316 E. Santa Fe Ave., Apt. 2, Fullerton, Calif. 92631
MASTEN, John, Univ. of Kentucky, Col. of Com., Lexington, Ky.
MASTERS, Stanley H., Office of Econ. Opportunity, Exec. Off. of the Pres., Washington, D.C. 20506
MASTRANDONAS, Vasilius, Jefferson Dr., East Lyme, Conn. 06333
MASTRANTONIS, Constantine, 4899 Old Dominion Dr., Arlington, Va. 22207
MASTRAPASQUA, Frank M., 1755 64th St., Brooklyn 19, N.Y.
MASTRI, Thomas F., 765 Bronx River Rd., Bronxville 8, N.Y.
MATAMOROS, Albert G., Armstrong Cork Co., Lancaster, Pa. 17604
MATCHETT, Gerald J., Illinois Inst. of Tech., Dept. of Econ., Chicago, Ill. 60616
MATHER, Loys L., Univ. of Wisconsin, Dept. of Agric. Econ., Madison, Wis. 53706
MATHERLY, Walter J., 2315 Prince St., Durham, N.C. 27707
MATHESON, K. G., 1719 Balsam Lane, Villanova, Pa. 19085
MATHEWSON, Gilbert F., 380 Curtner Ave., Apt. 21, Palo Alto, Calif. 94306
MATHIS, Edward J., 146 Princeton Ave., Buffalo, N.Y.
MATHIS, F. John, Box 70, Forestview Trailer Park, Iowa City, Iowa
MATHIS, Paul C., Central Michigan Univ., Mt. Pleasant, Mich. 48858
MATHUR, Vijay K., Wayne State Univ., Dept. of Econ., Detroit 2, Mich.
MATSON, Roger A., Tennessee Valley Authority, Regional Studies Staff, Knoxville, Tenn.
MATSUI, Shichiro, Doshisha Univ., Kyoto, Japan
MATSUMOTO, Masao, Rt. 3, Box 31-A, Chico, Calif. 95926
MATSUURA, Harry, 4521 31st St. S., Apt. 203, Arlington, Va. 22206
MATTAY, Arnold K., 3317 Stanford Ave., Dallas, Tex. 75225
MATTERSDORFF, Guenter H., Lewis and Clark Col., Dept. of Econ., Portland, Ore.
MATTESSICH, R., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif.
MATTFIELD, Frederic R., S.M.T.I., Tech. Center, New Bedford, Mass.
MATTHEISEN, Richard L., 2321 Surrey Lane, Falls Church, Va.
MATTHEWS, C. Arnold, Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla.
MATTHEWS, John R., Jr., P.O. Box B-J, Williamsburg, Va. 23185
MATTHEWS, Joseph C., Jr., 4706 Western Blvd., Raleigh, N.C. 27606
MATTHEWS, R. C. O., All Souls Col., Oxford, England
MATTILA, John M., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
MATTIOLI, Raffaele, Banca Commerciale Italiana, Milan, Italy
MATULIS, Margaret K., 300 W. 12th St., New York 14, N.Y.
MAUER, William Alan, Defense Mgt. Program, U.S.N.P.S., Monterey, Calif.
MAULDON, R. G., Univ. of Western Australia, Inst. of Agric., Nedlands, Western Australia
MAUNG, Mya, 616 8th St., Brookings, S.D.
MAURER, Alan O., 1009 Chillum Rd., Hyattsville, Md 29782
MAURER, Stephen A., Rider Col., E. State St., Trenton 9, N.J.
MAURIZI, Alexander R., 791 Matadero, Palo Alto, Calif. 94306
MAVERICK, Lewis A., 626 Charter Oak St., South Pasadena, Calif.
MAX, Peter, NERA, Inc., 1120 Connnecticut Ave., N.W., Washington, D.C. 20036
MAXIM, Leslie D., National Starch and Chem. Corp., 1700 W. Front St., Plainfield, N.J. 07063
MAXWELL, Elmer W., 3675 College Ave., Apt. F-4, San Diego, Calif. 92115
MAXWELL, James A., Clark Univ., Worcester, Mass.
MAXWELL, William David, Indiana Univ., Sch. of Bus., Bloomington, Ind. 47405
MAY, Alonzo B., Univ. of Denver, Dept. of Econ., Denver, Colo.
MAY, Donald J., 1609 S. West St., Falls Church, Va.
MAY, Elizabeth S., Export-Import Bank of Wash., 811 Vermont Ave., N.W., Washington, D.C. 20571
MAY, Frank B., North Texas State Univ., Bus. Services, Denton, Tex 76203
MAY, John Wesley, R.D. 2, Box 378, Washington, Pa. 15301
MAY, Phillip A., Northern Michigan Univ., Dept. of Econ., Marquette, Mich. 49855
MAY, Todd, Jr., 15 W. 75th St., New York 24, N.Y.
MAYBERRY, Lester J., 18 W. Main St., Shiremanstown, Pa. 17091
MAYER, Erwin S., Western Washington Col. of Educa., Bellingham, Wash. 98225
MAYER, Frank B., c/o Mr. and Mrs. Sklar. 141-02 Jewel Ave., Flushing 67, N.Y.
MAYER, Helmut, Bank of International Settlements, Basel, Switzerland
MAYER, Joseph, 929 Chestnut Lane, Oxford, Ohio 45056
MAYER, Joseph H., 376 N St., S.W., Washington, D.C. 20024
MAYER, Karl M., Kremenholl 33, 563 Remscheid, Germany
MAYER, Lawrence A., 65 E. 96th St., New York 28, N.Y.
MAYER, Lona, 2192 Valentine Ave., Bronx 57, N.Y.
MAYER, Peter C., 2201 Glen Ave., Berkeley 9, Calif.
MAYER, Raymond R., 9820 S. Leavitt St., Chicago, Ill. 60643
MAYER, Robert W., Univ. of Illinois, Dept. of Fin., Urbana, Ill.
MAYER, Thomas, 1305 Campus Dr., Berkeley 8, Calif.
MAYES, Jane C., 2000 S. Eads St., Arlington, Va. 22202

MAYHEW, Timothy P., 55 E. 93rd St., New York, N.Y. 10028
MAYNARD, David, Univ. of Alberta, Dept. of Econ., Edmonton, Alberta, Canada
MAYNARD, Eugene L., 1414 Gregory, Wilmette, Ill. 60091
MAYNARD, Ruth S., 7759 Lake Shore Blvd., Madison, Ohio
MAYNE, Alvin, 61 Kings Ct., Santurce, P.R. 00911
MAYNES, E. Scott, Univ. of Minnesota, Center for Econ. Res., Minneapolis, Minn. 55455
MAYO, Robert P., 33 Little Marryat Rd., Cary, Ill.
MAYOR, Thomas H, Univ. of Maryland, Dept. of Econ., College Park, Md.
MAYRER, George F., 8 Stewart Ave., Nutley, N.J. 07110
MAZEK, Warren F., Col. of Wooster, Dept. of Econ., Wooster, Ohio
MAZZITELLI, Dominick J., 159 E. 107th St., New York 29, N.Y.
McADAM, Kevin T., 725 Scotland Rd., Orange, N.J. 07050
McADAMS, Alan K., Cornell Univ., Grad. Sch. of Bus. and Pub. Admin., Ithaca, N.Y. 14850
McADOO, Richard B., 49 E. 33rd St., New York 16, N.Y.
McALEER, James A., 81 W. Brookside Dr., Larchmont, N.Y. 10538
McALLESTER, Allen E., 4511 S. 6th St., Arlington, Va. 22204
McALLISTER, Harry E., Washington State Univ., Sch. of Econ. and Bus., Pullman, Wash.
McALLISTER, Lester B., Beloit Col., Beloit, Wis.
McALLISTER, Phyllis, 5345 S. France Ave., Minneapolis 10, Minn.
McANINCH, Vernon D., American Consulate General, Box 18, FPO, New York, N.Y. 09521
McARTHUR, E. Ralston W., Box 165, Huntington, P.Q., Canada
McBRIDE, Howard J., 371 Amazon, Cincinnati, Ohio
McCABE, David A., 317 Franklin Ave., Princeton, N.J. 08540
McCABE, James R., Apt. D, Bldg. 3, University Heights, Columbia, Mo. 65202
McCABE, Martin, Box 105, Ponce, P.R.
McCABE, Milo F., 7124 27th Ave., Kenosha, Wis. 53140
McCABE, Norbert G., Hq APSC Box 253, Andrews AFB, Wash 20331
McCAFFREE, Kenneth M., 5014 18th Ave., Seattle 5, Wash.
McCALL, John J., Jr., Univ. of Chicago, Grad. Sch of Bus., Chicago, Ill. 60637
McCALLA, Alex F., Univ. of Minnesota, Dept. [illegible]gric. Econ., St. Paul, Minn. 55101
McC[illegible]Y, John W., 13 Vestal St., Nantucket, Mass. 02554
McCALLUM, Bennett T., Rice Univ., Dept. of Econ., Houston, Tex. 77001
McCALMONT, David B., 490 Alden Ave., Columbus 1, Ohio
McCAMLEY, Francis, 3165 Grad. Hall, 2184 Lincolnway, Ames, Iowa 50010
McCAMMON, Bert, Indiana Univ., Sch. of Bus. Dept. of Mktg., Bloomington, Ind.
McCANN, Thomas R., N.E. Louisiana State Col., Monroe, La.
McCARL, Henry N., Pennsylvania State Univ., Dept. of Mineral Econ., 301 Engineering-Unit D, University Park, Pa. 16802
McCARLEY, James F., 1005 E. Cass St., Albion, Mich. 49224
McCARNEY, Bernard J., Illinois State Normal Univ., Normal, Ill. 61761
McCARTHY, Carol T., Off. of Res., Plans, Programs and Eval., Exec. Off. of the President, Washington, D.C. 20506
McCARTHY, Edmund J., 623 Kensington Rd., East Lansing, Mich. 48823
McCARTHY, James E., 368 97th St., Brooklyn 9, N.Y.
McCARTHY, James L., Canisius Col., Buffalo 8, N.Y.
McCARTHY, John J., 7208 Old Gate Rd., Rockville, Md. 20852
McCARTHY, (Mrs.) Mary F., 716 Oxford Rd., Ypsilanti, Mich. 48197
McCARTHY, Stanley J., Government Dev. Bank for P.R., Box 4591, San Juan, P.R.
McCARTHY, Terence, 110-45 Queens Blvd., Forest Hills, N.Y. 12068
McCARTHY, William J., 36 Shawnee Ave., Yonkers, N.Y.
McCARTY, John M., 357 Valley St., El Segundo, Calif. 90245
McCAY, Clayton C., Diablo Valley Col., Golf Club Rd., Concord, Calif.
McCLEARY, William A., III, Williams Col., Dept. of Econ., Williamstown, Mass.
McCLELLAN, Joel, 404-D Eagle Heights Apts., Madison, Wis. 53705
McCLELLAND, (Mrs.) Anne R., ROCAP/Guatemala, Dept. of State, Washington, D.C. 20521
McCLELLAND, Donald H., ROCAP/Guatemala, Dept. of State, Washington, D.C. 20521
McCLELLAND, Harold F., Pitzer Hall 114, Claremont Men's Col., Claremont, Calif. 91717
McCLENEGHAN, Thomas J., 3013 E. Linden St., Tucson, Ariz. 85716
McCLINTIC, Joseph, San Diego State Col., San Diego 15, Calif.
McCLOSKEY, Donald N., 1604 W. Peabody Ter., Memorial Dr., Cambridge 38, Mass.
McCLUNG, Nelson D., 1150 12th St., N.W., Washington, D.C. 20005
McCLURE, John H., 6586 Montrose, Detroit 28, Mich.
McCLURG, Donald J., Univ. of Colorado, Dept. of Econ., Boulder, Colo.
McCOLLUM, James B., Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta, Ga. 30332
McCONAHAY, J. D., 2400 Mathews St., Fort Collins, Colo.
McCONKEY, Glenn A., 9 Cedar Dr., Charleston, Ill. 61920
McCONNELL, Campbell R., Univ. of Nebraska, Dept. of Econ., Soc. Sci. Hall, Lincoln, Neb.
McCONNEN, Richard J., c/o Pacific Southwest, Forest Exp. Sta., 1960 Addison St., Berkeley 1, Calif.
McCORMICK, Colleen A., Monsanto Chemical Co., 800 N. Lindbergh Blvd., St. Louis 66, Mo.
McCORMICK, William, Jr., 717 Fair St., Berea, Ohio 44017
McCOWEN, Thomas, 1423 Rose Dr., Champaign, Ill 61820
McCOY, Leahmae, Univ. of Arizona, Col. of Bus. and Pub. Admin., Tucson, Ariz.
McCOY, Patricia, 7108 Chestnut, Kansas City, Mo 64132
McCRACKEN, Michael C., 1667 Yorktown Dr., Houston 27, Tex.
McCRACKEN, Paul, Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
McCREA, (Mrs.) Joan M., Arlington State Col., Sch. of Bus., Arlington, Texas
McCREARY, James W., 295 Grizzly Peak, Berkeley 8, Calif.
McCROSKEY, Jack L., 28 Union St., Red Bank, N.J.
McCUISTION, Clayton C., 103 N. Ripley St., # 201, Alexandria, Va.
McCULLOCH, James H., California Inst. of Tech., Blacker House, Pasadena, Calif. 91109
McCULLOCH, Roy, 10 Huntley St., Apt. 1606, Toronto 5, Ont., Canada
McCUTCHAN, Cuyler W., 8357 Schaefer, Detroit 28, Mich.
McDANIEL, William, 129 N. Park Ave., Springfield, Ill. 62702
McDERMOTT, F. J., Box 1614, Williamsburg, Va. 23185
McDERMOTT, Raymond M., 3869 California St., Omaha, Neb. 68104
McDERMOTT, Thomas J., Duquesne Univ., Sch. of Bus. Admin., Pittsburgh 19, Pa.
McDIARMID, Orville J., 4312 N. 40th St., Arlington, Va. 22207
McDONAGH, Thomas, Univ. of Notre Dame, Notre Dame, Ind. 46556
McDONALD, J. Ian, Univ. of Toronto, Dept. of Polit. Sci., 273 Bloor St., W., Toronto, Ont., Canada
McDONALD, Joe R., 830 W. Kings Hgwy., San Antonio, Tex. 78212
McDONALD, John G., 3374 St. Michael Dr., Palo Alto, Calif. 94306
McDONALD, Stephen L., Univ. of Texas, Dept. of Econ., Austin 12, Tex.

McDONALD, William A., 408 Hyde Park Ave., Roslindale, Mass. 02131
McDONNELL, Mark T., Gonzaga Univ., Spokane, Wash. 99202
McDONOUGH, Edward F., Whisper Hill, Suffield, Conn. 06078
McDOWELL, Albert C., 16654 Tribune St., Granada Hills, Calif. 91344
McDOWELL, Gary L., Dartmouth Col., Dept. of Econ., Hanover, N.H.
McDOWELL, J. Hunter, c/o Sun Oil Co., 1608 Walnut St., Philadelphia, Pa. 19103
McDOWELL, W. Boyd, Jacksonville Univ., Jacksonville 11, Fla.
McELHONE, (Mrs.) Josephine, 2018 Oldham, Apt. 2, Austin, Tex. 78705
McELROY, Frederick W., Georgetown Univ., Room 249A Novils Bldg., Washington, D.C. 20007
McELROY, James Edward, Clear View Trailer Park, Athens Star Route, Box 228, Princeton, W.Va. 24740
McELROY, Jerome X., St. Mary's Col., St. Mary's, Kans. 66536
McELROY, Kathleen M., 2847 E. 122nd, Cleveland, Ohio 44120
McENROE, Jack A., Sunset Trail, Staatsburg, N.Y. 12580
McENTAFFER, John Leroy, 60 Merrymeeting Rd., Brunswick, Me. 04011
McEVILLA, Joseph D., Univ. of Pittsburgh, Sch. of Health Professions, 3550 Terrace St., Pittsburgh 13, Pa.
McEVOY, Raymond H., 2590 41 Ave., San Francisco, Calif. 94116
McEWAN, H. Douglas, 9056 E. Shorewood Dr., Apt 241, Mercer Island, Wash. 98040
McEWEN, Robert Joseph, Boston Col., Dept. of Econ., Chestnut Hill, Mass. 02167
McFADDEN, Daniel, Univ. of California, Dept. of Econ., Berkeley, Calif.
McFARLAND, David, 64 Dogwood Acres, Chapel Hill, N.C.
McFARLAND, Floyd B., Oregon State Univ., Dept of Econ., Corvallis, Ore. 97331
McFARLAND, Kathleen A., 2315 University Ave., Bronx, N.Y. 10468
McFARLAND, William E., 413 S. Division St., Apt. 4, Ann Arbor, Mich. 48104
McFAYDEN, Stuart, 133 Wilson St., Albany, Calif. 94710
McFEATERS, Marvin C., USAID/Brazil, American Embassy, APO New York 09676
McFERRIN, John Berry, Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla. 32601
McGANN, Paul W., 3200 Glenwood Pl., Falls Church, Va. 22041
McGARRY, Edmund D., 50 N. Ledge Dr., Snyder, N.Y. 14226
McGAUGHEY, Stephen E., Iowa State Univ., Dept. of Econ., Ames, Iowa
McGEE, John S., Duke Univ., Dept. of Econ., Durham, N.C. 27706
McGEE, LeRay R., Univ. of Kentucky, Col. of Com., Lexington, Ky.
McGILLIVRAY, John, 5873 Orlando Ave., Los Angeles, Calif. 90056
McGOLDRICK, Cornelius E., 111 S. Langley, Tucson, Ariz. 85710
McGOLDRICK, Michael E., Univ. of Oregon, Dept. of Econ., Eugene, Ore. 97403
McGOUGH, B. C., Arkansas State Col., Sch. of Bus., State College, Ark. 72467
McGOVERN, Francis G., Univ. of Dayton, Dayton, Ohio 45409
McGOVERN, John J., St. Peter's Rectory, 94 Somerset St., New Brunswick, N.J. 08901
McGOVERN, Thomas A., Jr., 287 Ave. C, New York, N.Y. 10009
McGOWAN, John J., Yale Univ., Dept. of Econ., Box 1972, Yale Sta., New Haven, Conn. 06520
McGRATH, Francis Joseph, 2460 Davidson Ave., Bronx, N.Y. 10468
McGRATH, John Edward, Western R.R. Traffic Asso., 516 W. Jackson Blvd., Room 312 A, Chicago, Ill. 60606
McGRAW, Booker T., 3036 Park Pl. N.W., Washington, D.C.
McGREEVEY, John D., Jr., 618 E. 21st St., Brooklyn, N.Y. 11226
McGREGOR, Harlan E., Avondale Farm, Newburg, Md. 20664
McGREGOR, Russell Carhart, 7831 Northdown Rd., Alexandria, Va. 22308
McGREW, John Gilbert, Fenn Col., Cleveland, Ohio
McGUCKIN, Robert H., III, 259 Norwood St., Buffalo, N.Y.
McGUFFOG, Thomas B., c/o Manweb, Commercial Dept., Love Lane, Pall Mall, Liverpool 3, England
McGUIRE, Carl Wilburn, Univ. of Colorado, Dept. of Econ., Boulder, Colo.
McGUIRE, Charles B., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
McGUIRE, John A., Univ. of Hartford, Hartford 1, Conn.
McGUIRE, Joseph William, Univ. of Kansas, Sch. of Bus., Lawrence, Kans.
McGUIRE, Sister Mary Elissa, Col. of Notre Dame of Maryland, 4701 N. Charles St., Baltimore 10, Md.
McGUIRE, Timothy W., Carnegie Inst. of Tech., 651-D, Schenley Park, Pittsburgh, Pa. 15213
McGURL, Vincent, Spindletop Research Corp., Spindletop Research Center, Lexington, Ky.
McHALE, Thomas R., Victoria Milling Co., Ind., P.O. Box 171, Bacolod, Occ. Negros, Philippines
McHUGH, Helen F., 1901 Hopi Trail, Austin 3, Tex.
McHUGH, L. F., 406 Lexington Dr., Silver Spring, Md. 20901
McHUGH, Richard W., 26 Ord St., Salem, Mass. 01970
McHUGH, Thomas F., 3703 Kayson St., Silver Spring, Md.
McILVAINE, William D., 1451 Iroquois Ave., Long Beach, Calif. 90815
McINNIS, R. Marvin, 1254 Princess St., Apt. 37, Kingston, Ont., Canada
McINTYRE, Francis, Univ. of Hawaii, Col. of Bus. Admin., Honolulu, Hawaii 96822
McINTYRE, M. Charles, 2520 Niagara Way, Sacramento, Calif.
McIVOR, R. Craig, McMaster Univ., Hamilton, Ont., Canada
McJUNKIN, Frederick E., Virginia Poly. Inst., Dept. of Civil Engineering, Blacksburg, Va.
McKAY, Roberta V., 4208 E. Capital St., N.E., Apt. 104, Washington, D.C. 20019
McKEAN, John R., Univ. of Alberta, Dept. of Econ., Edmonton, Alta., Canada
McKEAN, Roland N., 2121 Malcolm Ave., Los Angeles 25, Calif.
McKECHNIE, Graeme H., 2937 Monroe St., Madison, Wis. 53711
McKEE, A. F., Laurentian Univ., Econ. Dept., Sudbury, Ont., Canada
McKEE, James R., 225 Lorraine Ave., Syracuse, N.Y. 13210
McKELLIPS, Robert W., 314½ Main Ave., Apt. 3, Brookings, S.D. 57006
McKENNA, Joseph P., Virginia Poly. Inst., Dept. of Econ., Blacksburg, Va.
McKENNA, Philip M., Kennametal, Inc., Latrobe, Pa. 15650
McKENNA, Vincent T., 63 Figler Ave., Fairfield, Conn. 06432
McKENZIE, George W., 132 Wilson St., Albany, Calif.
McKENZIE, Hillis S., Univ. of Alaska, Col. of Bus., Econ. and Gov., College, Alaska 99735
McKENZIE, Lionel W., Univ. of Rochester, Econ. Dept., Rochester, N.Y.
McKENZIE, Robert R., New York Univ., 100 Trinity Pl., New York, N.Y. 10016
McKEON, James C., 4222 Spruce St., Philadelphia, Pa. 19104
McKEOUGH, Brendan, St. Norbert Col., West De Pere, Wis. 54178
McKIE, James W., Vanderbilt Univ., Dept. of Econ., Nashville, Tenn.
McKILLOP, William, Univ. of California, Sch. of Forestry, Berkeley, Calif. 94720
McKIM, Bruce T., Univ. of Calif., Dept. of Econ., Riverside, Calif.
McKINLEY, David H., Pennsylvania State Univ., 129 Boucke Bldg., University Park, Pa. 17774
McKINLEY, Erskine W., Tulane Univ., Dept. of Econ., New Orleans 18, La.
McKINLEY, Gordon W., 14 Norman Circle, Madison, N.J. 07940

McKINLEY, Gregory E., P.O. Box No. 10, R.R.1, Manotick, Ont., Canada
McKINLEY, S. J., 250 Beacon St., Boston 16, Mass.
McKINNELL, Robert T., Univ. of New Brunswick, Dept. of Econ., Fredericton, N.B., Canada
McKINNEY, David H., Box 163, University, Miss 38677
McKINNEY, George W., Jr., 424 Upper Mountain Ave., Upper Montclair, N.J. 07087
McKINNEY, John N., Simon Fraser Univ., Dept. of Econ. and Com., Burnaby 2, B.C., Canada
McKINNEY, Joseph A., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
McKINNEY, Thomas H., 6166 Skyline Dr., East Lansing, Mich.
McKINNON, Ronald, Stanford Univ., Dept. of Econ., Stanford, Calif. 96128
McKINSTRY, Wm. J., 5128 Cole Rd., Oxford, Ohio 45056
McKNIGHT, John, 16 Beechwood Lane, Scarsdale, N.Y.
McLAIN, James, 2192 Coon Rd., Copley, Ohio 45723
McLAUGHLIN, Francis M., Boston Col., Carney 240, Chestnut Hill, Mass. 02167
McLAUGHLIN, Glenn, 3053 Ordway St., N.W., Washington 8, D.C.
McLAUGHLIN, Joseph James, 5 Strathmore Rd., Havertown, Pa. 19083
McLAUGHLIN, R. L., Scoville Mfg. Co., Waterbury, Conn.
McLAUGHLIN, Russell U., 195 Redwood Rd., King of Prussia, Pa. 19442
McLEAN, Alan A., 1318 Forest Ct., Ann Arbor, Mich.
McLEAN, Thomas, Portland State Col., Dept. of Econ., Portland 1, Ore.
McLEMORE, Harry K., 8412 Louwanda Dr., Little Rock, Ark. 72205
McLENDON, Wm. P., 4106 Woodberry St., University Park, Hyattsville, Md.
McLEOD, A. N., Toronto-Dominion Bank, King O Bay St., Toronto 1, Ont., Canada
McLEOD, Donald, Temple Univ., Box 387, Philadelphia, Pa. 19122
McLOONE, Eugene P., 2132 Wyoming Ave., N.W., Washington 8, D.C.
McLOUGHLIN, John T., 2105 Burr Ave., New York, N Y. 10461
McLOUGHLIN, Peter F. M., Univ. of Santa Clara, Dept. of Econ., Santa Clara, Calif. 95053
McLURE, Charles E., Jr., Rice Univ., Dept. of Econ., Houston, Tex. 77001
McMAHAN, Frederick J., St. Ambrose Col., Davenport, Iowa
McMAHON, James A., Jr., 177 Washington St., Geneva, N.Y. 14456
McMAHON, James E., 854 Nassau St., North Brunswick, N.J. 08902
McMAHON, Peter J., 8568 Franklin Ave., Los Angeles 69, Calif.
McMAHON, Thomas W., Jr., 484 Brookdale Rd., Union, N.J. 07087
McMAHON, Walter W., 405 E. Colorado, Urbana, Ill.
McMANUS, Charles E., III, 110 W. Lake Ave., Baltimore, Md. 21210
McMILLAN, S. Sterling, Western Reserve Univ., Dept. of Econ., Cleveland 6, Ohio
McMILLAN, Thomas E., Jr., 1121 Overlake, Richardson, Tex.
McMILLEN, Robert E., 1699 31st St., N.W., Washington, D.C. 20007
McMILLEN, W. L., Morgan Guaranty Trust Co. of New York, 23 Wall St., New York, N.Y. 10015
McMULLAN, Wilbur N., 504 Custis Rd., Glenside, Pa. 19038
McNABB, Victor O., 2000 Scroggins Rd., Alexandria, Va. 22302
McNATT, Emmett, Univ. of Illinois, 327 Commerce Bldg., Urbana, Ill.
McNEIL, John M., 1301 Delaware Ave., S.W., Apt. N-205, Washington, D.C. 20024
McNEILL, Clarence E., 3510 Woods Ave., Lincoln, Neb. 68510
McNULTY, (Mrs.) Marian S., 13 Stuart St., Waldwick, N.J. 07463
McNULTY, Paul J., 13 Stuart St., Waldwick, N.J. 07463
McNUTT, Jack, Murphy Corp., El Dorado, Ark. 71730
McPHERSON, William H., 504 E. Armory Ave., Champaign, Ill.
McPHERSON, William Kenneth, Dan McCarty Hall, Room 50, Univ. of Florida, Dept. of Agric. Econ., Gainesville, Fla. 32601
McPHERSON, Woodrow W., Univ. of Florida, Col. of Agric., Dept. of Agric. Econ., Gainesville, Fla.
McQUAID, William J., 851 California Ave., San Francisco, Calif. 94104
McQUISTON, James M., Mississippi Southern Col., Box 72, Sta. A, Hattiesburg, Miss.
McROSTIE, Clair Neil, Gustavus Adolphus Col., St. Peter, Minn. 56082
McTEER, Robert D., Jr., 460 Knottingham Dr., Athens, Ga. 30601
McVEY, Phillip, Univ. of Nebraska, Col. of Bus. Admin., Lincoln, Neb. 68508
McWHINNEY, Madeline, 33 Liberty St., New York, N.Y. 10045
MEAD, Donald C., Makerere Univ. Col., P.O. Box 262, Kampala, Uganda
MEAD, Walter J., Univ. of California, Econ. Dept., Santa Barbara, Calif.
MEADE, James E., Low Brooms, Little Shelford, Cambridge, England
MEANS, Gardiner, Route 1, Box 138, Vienna, Va. 22180
MEARS, Charles D., 18 Kentbury Way, Bethesda 14, Md. 21811
MEARS, Leon A., Univ. of Wisconsin, Philippine Project, P.O. Box 776, Manila, Philippines
MEASDAY, Walter S., Univ. of Maryland, Dept. of Econ., College Park, Md.
MECKLING, William H., 18 Van Cortland Dr., Pittsford, N.Y. 14534
MEDAWAR, George S., American Univ. of Beirut, Econ. Research Inst., Beirut, Lebanon
MEDE, Gary, 703 Jersey Ave., Apt. B-11, Bloomington, Ill.
MEDINA-MONTIEL, Lucia, 5a, Calle S.O. # 207, Managua, Nicaragua
MEDLEY, Kenneth W., 6225 31st St., N., Arlington, Va. 22207
MEDOW, P. I., Rutgers Univ., Dept. of Econ., New Brunswick, N.J.
MEEHAN, James P., 79 School St., Woburn, Mass. 01801
MEEHAN, James P., Jr., Edinboro State Col., Dept. of Soc. Sci., Edinboro, Pa. 16412
MEEHAN, James W., Jr., 34 Lake St., Brighton, Mass. 02324
MEEK, Gertrude Pennington, 707 Tuscan Ave., Hattiesburg, Miss. 39401
MEEK, Howard B., Statler Hall, Ithaca, New York
MEEK, Paul, 74 Washington Ave., Hillsdale, N.J. 07642
MEEKS, Thomas J., Duke Univ., Box 1044 MGC, Durham, N.C. 27706
MEER, Melvyn L., Macalester Col., Dept. of Econ., St. Paul, Minn. 55101
MEERMAN, Jacob, USAID-Bolivia, Dept. of State, Washington, D.C. 25
MEEROPOL, Michael, King's Col., Cambridge Univ., Cambridge, England
MEHAY, Stephen L., 303 W. Illinois, Urbana, Ill. 61802
MEHLING, Jacob G., 314 S.E. Second, College Place, Wash. 99324
MEIBURG, Charles O., Univ. of Virginia, Bur. of Population and Econ. Res., 1910 Thomson Rd., Charlottesville, Va. 22903
MEIER, Gerald M., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif.
MEIER, Robert C., Univ. of Washington, Col. of Bus. Admin., Seattle 5, Wash.
MEIGS, A. James, 16 Evergreen Circle, Princeton, N.J. 08541
MEINHART, Wayne A., Oklahoma State Univ., 302 Morrill Hall, Stillwater, Okla.
MEINKOTH, Marian R., Temple Univ., Econ. Dept., Philadelphia, Pa.
MEINSTER, David R., Brown Univ., Econ., Dept., Providence, R.I.
MEISELMAN, David, 4914 Ashby St., N.W., Washington, D.C. 20007
MEISLIK, Estelle, 85-25 215th St., Queens Village, L.I., N.Y. 11427

MEISNER, Joseph C., Washington Univ., Dept. of Econ., McMillan Hall, St. Louis, Mo. 63130
MEJIA, J. Hernan, 1255 New Hampshire Ave., N.W., Apt. 622, Washington, D.C. 20036
MEJIA, Sonia T., Regis Col., Dept. of Econ., Weston, Mass. 01985
MELA, Donald F., 1301 Kingston Ave., Alexandria, Va.
MELAMID, Alexander, 15 Ellsworth Ave., Yonkers, N.Y.
MELANCON, Albert, 262 Boul. Deguire, Cite de Saint-Laurent, Montreal 9, P.Q., Canada
MELANDER, E. R., Pennsylvania State Univ., Col. of Bus. Admin., Boucke Bldg., University Park, Pa. 17774
MELDER, Frederick, Clark Univ., Worcester 10, Mass.
MELDER, John L., 4314 Nakoma Rd., Madison, Wis.
MELICHAR, Emanuel O., Ed. of Gov. Fed. Res. System, Div. of Res. and Statis, Washington, D.C. 20551
MELITZ, Jacques, Tulane Univ., Dept. of Econ., New Orleans, La. 70118
MELKONIAN, Martin, 52 Ormonde Blvd., Valley Stream, N.Y.
MELLIN, David, 1423 Roscomare Rd., Los Angeles 24, Calif.
MELLIN, Gilbert M., Whitney Nat. Bank, P.O. Box 61260, New Orleans, La. 70160
MELLIS, G. W., 71 McGillivray Crescent, Regina, Sask., Canada
MELLISH, G. Hartley, Univ. of South Florida, Col. of Bus. Admin., Tampa, Fla.
MELLO, Edmund B., 78 Kenwood St., Kensington, Conn.
MELLON, W. Giles, Chase Manhattan Bank, Econ. Res. Dept., 1 Chase Manhattan Plaza, New York, N.Y.
MELNICOFF, David C., 3 Asbury Ave., Melrose Park, Pa. 19126
MELNYK, Myron J., 1150 Meloy Rd., Kent, Ohio
MELNYK, Zinowij L., 2163 N. Bend Rd., Cincinnati, Ohio 45224
MELO, Gabriel, Apartado aereo 13914, Bogota, D.E., Colombia
MELOAN, Taylor Wells, Univ. of Southern California, Sch. of Bus., Los Angeles 7, Calif.
MELOE, Torleif, 62 Hayes St., Blauvelt, N.Y. 10913
MELON, Maybeth, 110 Park St., Apt. 2, Sterling, Colo. 80751
MELTON, Lee J., Jr., Louisiana State Univ., Co.. of Com., Baton Rouge 3, La.
MELTON, R. B., Box 6132 N.T. Sta., Denton, Tex.
MELTZ, Noah M., 26 Carluke Cres., Apt. 405, Willowdale, Ont., Canada
MELTZER, Allan H., Carnegie Inst. of Tech., Dept. of Econ., Pittsburgh, Pa. 15213
MELTZER, Bernard C., 933 Crosswick Rd., Jenkintown, Pa 19046
MELVILLE, Robert F., 605 E. Main St., Owego, N.Y. 13827
MEMMEL, Aloysius J., 100 S. Greenwood Ave., Park Ridge, Ill. 60068
MENA-ARIZ, Jorge, Calle Ruben Dario 1112, San Salvador, El Salvador, Central America
MENASHE, Isaac, Fed. Res. Bank of New York, 33 Liberty St., New York 45, N.Y.
MENDEL, Joseph J., 407 Northridge Rd., Columbus, Ohio 43214
MENDELL, Herbert C., 1943 E. 24th St., Brooklyn 29, N.Y.
MENDELS, Ernest, Revere Copper Inc., 230 Park Ave., New York 17, N.Y.
MENDELS, Roger P., Univ. of British Columbia, Dept. of Econ., Vancouver 8, B.C., Canada
MENDELSON, Morris, 4100 Joy Rd., Detroit 4, Mich.
MENDELSSOHN, Rudolph C., 4106 Elizabeth Lane, Fairfax, Va. 22030
MENDERSHAUSEN, Horst, RAND Corp., Santa Monica, Calif.
MENDEZ-M., Jorge A., c/o Committee of None, AP, Room 413-A, 1725 Eye St., N.W., Washington, D.C. 20006
MENEFEE, Robert G., 2300 College P.O., Berea, Ky. 40403
MENGE, John Alexander, Maple Leaf Farm, Hanover Rd., Lyme, N.H. 03769
MENGEL, Stanley K., 99 A Escondido Village, Stanford, Calif. 94305
MENHART, Leonard M., 106 Center St., Clifton, N.J.
MENNIS, Edmund A., Wellington Management Co., 1630 Locust St., Philadelphia, Pa. 19103
MENZ, John A., 6441 Hogan Dr., Sacramento 22, Calif.
MENZLER, Gerhard, Gruneburgweg 85, Frankfurt, Main, Germany
MERCER, Donald G., Queen's Univ., Grad. Res., Student's Mem. Union, Kingston, Ont., Canada
MERCER, Lloyd J., 3634 N.E. 120th St., Seattle, Wash 98125
MERCER, Norman A., Union Col., Bailey Hall, Schenectady 8, N.Y.
MERCHANT, Ely O., P.O. Box 1183, Avon Park, Fla. 33825
MERCHANT, Stephen M., 304 Center Ave., Delanco, N.J. 08075
MERCILLON, Henri, 39 Rue Sambin, Dijon, Cote d'Or, France
MEREDITH, L. Douglas, Nat. Life Ins. Co., Montpelier, Vt.
MEREWITZ, Leonard A., 1211 Curtis St., Berkeley, Calif. 94706
MERINGER, Ronald A., Continental Ill. Nat. Bank, 231 S. La Salle St., Chicago, Ill. 60690
MERINO M., Rigoberto, Barrio La Hoya, Contiguo al Registro de la Prop., Tegucigalpa, D.C., Honduras
MERKT, O. E. D., 360 Jefferson Rd., Princeton, N.J. 08540
MERRIAM, (Mrs.) Ida C., 2908 Brandywine St, Washington 8, D.C.
MERRIAM, John H., Route 1, South, Pocatello, Idaho 83201
MERRIAM, Thornton W., 95 Dartmouth St., Springfield, Mass. 01109
MERRICK, Thomas W., 18th and Thompson St., Philadelphia, Pa. 19121
MERRILL, Robert S., 75 Council Rock Ave., Rochester 10, N.Y.
MERRILL, William C., Iowa State Univ., Dept. of Econ., Ames, Iowa 50010
MERWIN, Charles L., International Monetary Fund, 19th and H Sts., N.W., Washington, D.C. 20431
MERZ, Paul Edward, St. Louis Univ., Sch. of Com. and Fin., 3674 Lindell Blvd., St. Louis, Missouri 63108
MERZBACH, L. H., P.O. Box 33, Southwestern Univ. Sta., Georgetown, Tex. 78626
MESA-LAGO, Carmelo, 1070 W. 53rd Ter., Hialeah, Fla. 33012
MESA-TEJADA, Alfredo, 31-29 56th St., Woodside 77, N.Y.
MESCHI, William, Canaan, N.Y. 12029
MESELSOHN, Lawrence, 219-72 75th Ave., Bayside, N.Y. 11364
MESSENGER, Arthur F., Purdue Univ., Krannert 134, Lafayette, Ind.
MESSIANU, Phoebus M., Av. Sudermann 304-401, Mexico 5, D.F., Mexico
MESSINA, Antoinette J., 52 Wabeno Ave., Springfield, N.J. 07081
MESSINA, Milton, 6101 Durbin Rd., Bethesda, Md. 21811
MESSING, (Mrs.) Janet K., Hunter Col. in the Bronx, Bedford Park Blvd. W., New York 68, N.Y.
MESSNER, Stephen D., 302 E. 19th, Bloomington, Ind.
MESSNER, Van A., 40-23 203rd St., Bayside 61, N.Y.
MESTELMAN, Stuart, Purdue Univ., Krannert Grad. Sch. of Ind. Admin., Econ. Dept., Lafayette, Ind. 47907
MESTRE, Elroy R., Georgia Southern Col., Div. of Soc. Sci., Statesboro, Ga. 30459
METCALF, John E., 166 Locust St., Garden City, L.I., N.Y. 11535
METCALF, Richard, 3504 Raymond St., Chevy Chase, Md. 21620
METTLEN, Robert D., Indiana Univ., Sch. of Bus., Grad. Div., Bloomington, Ind.
METTLER, John Charles, The Citadel, Charleston, S.C.
METZGER, Ruth, 333 E. 79th St., Apt. 21W, New York 28, N.Y.

METZLER, Bernadette V., 78-59 80th St., Glendale 27, N.Y. 11542
METZLER, Lloyd A., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
MEURER, Emil M., Jr., 107 Monks Ave., Mankato, Minn.
MEYER, Arturo C., 628 Packard St., Ann Arbor, Mich. 48104
MEYER, Charles W., Iowa State Univ., Dept. of Econ., Ames, Iowa
MEYER, Donald G., Loyola Univ., Col. of Com., 820 N. Michigan Ave., Chicago, Ill. 60611
MEYER, George F., 732 St. Marks Ave., Brooklyn, N.Y. 11216
MEYER, Gerhard, Univ. of Chicago, Faculty Exch., Box 153, Chicago, Ill. 60637
MEYER, John Robert, 12 Clifton St., Belmont, Mass. 02019
MEYER, Michael R., 221 Willard Ave., Ottawa, Ill. 61350
MEYER, Paul A., Univ. of Maryland, Econ. Dept., College Park, Md.
MEYER, Peter B., Univ. of Wisconsin, 302 Soc. Sci. Bldg., Madison, Wis. 53706
MEYER, Richard H., 620 W. 116th St., Apt. 41, New York 27, N.Y.
MEYER, Robert A., Jr., 108-F Escondido Village, Stanford, Calif.
MEYER, Robert J., 901 Main St., Wakefield, R.I.
MEYER, Willi, Barenschanzstr. 117, 85 Nurnberg, Germany
MEYERDING, Charles E., 2827 N. Griggs St., St. Paul 13, Minn.
MEYERS, Albert L., 7201 Oakridge Ave., Chevy Chase, Md. 21620
MEYERS, Arthur C., 3674 Lindell Blvd., St. Louis 8, Mo.
MEYERS, Cecil H., Univ. of Minnesota, Dept. of Econ., Duluth, Minn. 55812
MEYROWITZ, Alvin, 1835 Loma Vista Dr., Beverly Hills, Calif.
MICHAEL, Avraham, 23 Tchlenov St., Tel-Aviv, Israel
MICHAEL, Robert T., 542 W. 112th St., Apt. 10-C, New York, N.Y. 10025
MICHAEL, Walther P., Ohio State Univ., Dept. of Econ., Columbus, Ohio 43210
MICHAELSEN, Jacob B., Univ. of California, Cowell Col., Santa Cruz, Calif. 95060
MICHAELY, Michael, Nat. Bur. of Econ. Res., 261 Madison Ave., New York, N.Y. 10016
MICHAL, Jan M., State Univ. of N.Y., Harpur Col., Dept. of Econ., Binghamton, N.Y.
MICHALIK, Benjamin A., 923 Sterner Rd., Hillside, N.J. 07205
MICHALOPOULOS, Constantine, Trinity Col., Dept. of Econ., Hartford, Conn. 06106
MICHAS, Nicocles, 333 E. 79th St., Apt. 17S, New York 21, N.Y.
MICHELL, Beverly R., 531 Buena Vista Ave., Redwood City, Calif. 94061
MICHELS, Rudolf K., 153 Dorchester Rd., Scarsdale, N.Y.
MICHELSON, Stephen, Reed Col., Dept. of Econ., Portland, Ore. 97202
MICKELSON, Lawrence R., 489 Amherst Ave., Des Plaines, Ill. 60016
MICKEY, (Mrs.) Jean L., 340 16th St., Santa Monica, Calif.
MICOLEAU, Henri LeBrec, General Motors Corp., 1775 Broadway, New York, N.Y.
MIECZKOWSKI, Bogdan, Ithaca Col., Dept. of Econ., Ithaca, N.Y.
MIELKE, Alton C., 2349 Macon St., Denver 8, Colo.
MIERNYK, William H., 824 Price St., Morgantown, W.Va. 26500
MIGHELL, Ronald L., 1210 Noyes Dr., Silver Spring, Md. 20910
MIGUE, Jean Luc, Laval Univ., Dept. of Econ., Quebec City, P.Q., Canada
MIHALASKY, John, 323 High St., Newark 2, N.J.
MIKESELL, John L., R.R. 2, Box 211, Bloomington, Ind.
MIKESELL, Raymond, Univ. of Oregon, Dept. of Econ., Eugene, Ore.
MIKI, Robert T., 5826 Conway Rd., Bethesda, Md. 20034
MIKITANI, Ryoichi, c/o Kobe Univ. of Com., Kobe Shoka Kaigaku, Tarumi-Ku, Kobe, Japan
MIKLIUS, Walter, U.S. Dept. of Agric., Market Structure and Costs Br.-MED-ERS, Transp. Econ. Group, Washington, D.C. 20250
MIKOLAJCZYK, Marian, 3219 Pauline Dr., Chevy Chase, Md. 20015
MIKOLJON, Stanley J., 91-57 79th St., Woodhaven, N.Y. 11421
MILBURN, Harold R., 240 N. Meridian St., Indianapolis, Ind. 46209
MILENER, Eugene D., 39 Cedar St., Oneonta, N.Y. 13820
MILENKOVITCH, (Mrs.) Deborah D., 425 Riverside Dr., Apt. 14B, New York, N.Y. 10025
MILGRIM, Herbert, 135-20 82nd Ave., Kew Gardens, N.Y. 11435
MILLAR, Abram A., 3509 Duke St., College Park, Md.
MILLARD, Guy E., 706 Country Club Rd., Somerville, N.J. 08876
MILLER, (Mrs.) Agnes Roman, 5 Gracie Sq., New York 28, N.Y.
MILLER, Arjay R., Ford Motor Co., The American Rd., Dearborn, Mich.
MILLER, Arthur S., George Washington Univ., Law Sch., Washington 6, D.C.
MILLER, Bruce G., 85 Valley View Dr., Morristown, N.J. 07960
MILLER, David R., 1023 Florence Ave., Evanston, Ill. 60202
MILLER, Donald C., Continental Ill. Nat. Bank, 231 S. La Salle St., Chicago, Ill. 60690
MILLER, Ervin, Univ. of Pennsylvania, Fin. Dept., Wharton Sch., Philadelphia 4, Pa.
MILLER, George L., Univ. of Santa Clara, Dept. of Fin., Sch. of Bus., Santa Clara, Calif. 95053
MILLER, Glenn H., Jr., Fed. Res. Bank of Kansas City, Res. Dept., Kansas City, Mo. 64106
MILLER, Glenn W., Ohio State Univ., Dept. of Econ., Columbus, Ohio
MILLER, (Mrs.) Gloria F., No. 2 Meadowbrook Village, Plainfield, N.J.
MILLER, H. Laurence, Carleton Col., Dept. of Econ., Northfield, Minn. 55057
MILLER, H. Lynn, 1111 N.E. 16th Ave., Ocala, Fla. 32670
MILLER, Harlan B., Inst. of Life Ins., 277 Park Ave., New York, N.Y. 10017
MILLER, Harold W., State Univ. Col. at Brockport, Brockport, N.Y. 14420
MILLER, Henry S., 14 Scott Dr., Huntington Station, L.I., N.Y. 11749
MILLER, Herman P., 105 Bluff Ter., Kemp Mill Estates, Silver Spring, Md.
MILLER, Howard L., Northwest Nazarene Col., Nampa, Idaho 83651
MILLER, James C., III, Univ. of Virginia, Dept. of Econ., Rouss Hall, Charlottesville, Va. 22901
MILLER, James D., 18959 S.W. Kristi Way, Lake Grove, Ore.
MILLER, James P., P.O. Box 5382, Raleigh, N.C. 27607
MILLER, Janet W., 2031 Lincoln St., Berkeley, Calif. 94709
MILLER, John E., 816 Cherry St., Wheaton, Ill.
MILLER, John F., Nat. Plan. Ass., 1606 New Hampshire, N.W., Washington 9, D.C.
MILLER, John Perry, 90 York Sq., New Haven 11, Conn.
MILLER, Marcus H., 2757 Yale Sta., New Haven, Conn. 06520
MILLER, Merton H., Grad. Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
MILLER, Mietzl, Lamar State Col., Sch. of Bus., Beaumont, Tex. 77704
MILLER, Morris, 13 Via Astronomia, Rome, Italy
MILLER, Norman C., R.D. #2, Altman Rd., Jeannette, Pa. 15644
MILLER, Norman C., Jr., New York Stock Exch., Dept. of Res. and Statis., 11 Wall St., New York, N.Y. 10005
MILLER, Oscar, 4638 W. Dempster, Skokie, Ill.
MILLER, Paul B., Texas A. and M. Univ., Dept. of Econ., College Station, Tex. 77843
MILLER, Paul L., 105 W. Mt. Ida Ave., Alexandria, Va. 22305
MILLER, Ralph Eli, Dunster H-21, Cambridge, Mass. 02138

MILLER, Richard A., Wesleyan Univ., Econ. Dept., Middletown, Conn. 06457
MILLER, Richard J., 30 Mary Lane, Waldwick, N.J. 07463
MILLER, Robert E., Univ. of San Diego, Alcala Park, San Diego, Calif. 92110
MILLER, Robert R., 212 Myrtle St., Iowa City, Iowa
MILLER, Roger F., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
MILLER, Roger L., Univ. of Iowa, Dept. of Bus. Admin., Iowa City, Iowa 52240
MILLER, Roger L., 5425 Dorchester Ave., Apt. 2-B, Chicago, Ill. 60615
MILLER, Ronald E., Univ. of Pennsylvania, Wharton Sch., Dept. of Regional Sci., Philadelphia, Pa. 19104
MILLER, Ronald H., 1041 University Ave., Bronx 52, N.Y.
MILLER, S. M., New York Univ., Press Annex, 23, Washington Sq., New York, N.Y. 10003
MILLER, Seymour H., 5846 N. 21st St., Arlington, Va. 22205
MILLER, Signey L., Jr., Bucknell Univ., Sch. of Bus. Admin., Lewisburg, Pa. 17837
MILLER, Stanley, 11116 Nicholas Dr., Silver Spring, Md.
MILLER, Taulman A., Indiana Univ., Dept. of Econ., Ballantine Hall, Bloomington, Ind.
MILLER, Vergil V., Oregon State Univ., Dept. of Bus. Admin., Sch. of Bus. and Tech., Corvallis, Ore. 97331
MILLER, Walter G., USDA, Econ. Res. Serv., Nat. Resource Econ. Div., Washington, D.C. 20250
MILLER, Wayne S., JRRU, Box 40, FPO, New York, N.Y. 09510
MILLER, William, Seventy Acres Rd., R.D. 3, West Reading, Conn.
MILLER, William C., 311 Everett St., Apt. D, Palo Alto, Calif. 94301
MILLER, William E., 2458 Como Ave., W., St. Paul, Minn. 55108
MILLER, William L., 4949 Sancola Ave., North Hollywood, Calif. 91602
MILLER, William L., Purdue Univ., Dept. of Agric. Econ., Lafayette, Ind.
MILLER, William L., Univ. of Georgia, Sch. of Com., Athens, Ga.
MILLER, William R., 1914 69th, Lubbock, Tex. 79412
MILLICAN, Louis A., 6803 Bender Dr., San Antonio, Tex. 78229
MILLICAN, Richard D., Univ. of Illinois, 355 Commerce West, Urbana, Ill.
MILLIKAN, Max F., 18 Scott St., Cambridge, Mass. 02138
MILLIKEN, John D., 153 W. Franklin St., Waynesburg, Pa.
MILLIMAN, Jerome W., Indiana Univ., Grad. Sch. of Bus., Bloomington, Ind.
MILLS, A. L., Jr., 3345 Reservoir Rd., N.W., Washington, D.C. 20007
MILLS, Edwin S., 2307 Crest Rd., Baltimore 9, Md.
MILLS, Gary F., 1627 E. 55th St., Long Beach, Calif. 90805
MILLS, James I., 4713 Queensbury Rd., Riverdale, Md. 20840
MILLS, Joseph C., Waterloo Lutheran Univ., Dept. of Econ., Waterloo, Ont., Canada
MILLS, Mark C., Indiana Univ., Dept. of Econ., Bloomington, Ind.
MILLS, Richard L., Tulip Tree House, Apt. 1009, Bloomington, Ind. 47401
MILLUSH, George B., Jr., 1758 Pine Rd., Homewood, Ill. 60430
MILNE, John N., Maclaren Adv. Co., 111 Richmond St. W., Toronto 1, Ont., Canada
MILNE, Matilda L., 4000 Tunlaw Rd., N.W., Washington, D.C. 20007
MILNE, Robert D., 19473 N. Sagamore Rd., Fairview Park, Ohio 43736
MILNER, Bettie A., 4 Hastings Lane, Livingston, N.J.
MILROY, Robert R., Indiana Univ., Sch. of Bus., Bloomington, Ind.
MILSTEIN, David N., Michigan State Univ., Dept. of Resource Development, East Lansing, Mich. 48823
MINCER, Jacob, Columbia Univ., Econ. Dept., New York 27, N.Y.
MINCKLER, Rex D., 3807 Ridgeview Rd., Arlington, Va. 22207
MINER, F. William, 7408 Park Vista, El Cerrito, Calif. 94530
MINER, Jerry, Syracuse Univ., Maxwell Hall, Syracuse, N.Y. 13210
MINGS, Turley R., San Jose State Col., Dept. of Econ., San Jose, Calif.
MINH, Bui Quang, Nat. Inst. of Admin., 10, Tran Quoc Toan, Saigon, Vietnam
MINHAS, Bagicha S., Indian Statis. Inst., 8 King George Ave., New Delhi 11, India
MINI, Peter V., Loyola Col., Dept. of Econ., Montreal, P.Q., Canada
MINICK, John B., USAID-Box A, APO New York 09694
MINKOFF, F. Paul, Barnes 1-F, Escondido Village, Stanford, Calif. 94305
MINKOFF, Jack, Div. of Gen. Studies, Pratt Inst., Brooklyn 5, N.Y.
MINSKY, Hyman P., Washington Univ., Econ. Dept., St. Louis, Mo. 63130
MINTON, Victor, 475 Ocean Pkwy., Brooklyn 18, N.Y.
MINTS, Lloyd W., 618 E. Myrtle St., Ft. Collins, Colo.
MINTZ, Ilse, 3135 Johnson Ave., New York 63, N.Y.
MINTZ, Norman, 214 Roe Ave., Syracuse, N.Y.
MINTZES, Joseph, American Embassy-AID, APO New York 09662
MIRACLE, Gordon E., Univ. of Michigan, Grad. Sch. of Bus. Admin., Ann Arbor, Mich.
MIRACLE, Marvin P., Univ. of Wisconsin, Dept. of Agric. Econ., Agric. Hall, Madison, Wis. 53706
MIRANDA, Gregorio S., 170 Hillside St., San Juan, Rizal, Philippines
MIRASOL, Maria Lourdes N., 41-32 50th St., Apt. 6, Woodside, N.Y. 11377
MIRE, Joseph, 2944 University Ter., N.W., Washington, D.C.
MIRIN, Linda S., Inter-American Dev. Bank, Econ. Dev. Div., Washington, D.C. 20577
MIRSKI, Michael S., 4633 Schenley Rd., Baltimore 10, Md.
MIRZA, David B., Kalamazoo Col., Kalamazoo, Mich.
MIRZA, Izhar A., Univ. of Manitoba, St. Paul's Col., Winnipeg, 19, Man., Canada
MISAWA, Takeo, 40 Suidobata 2-chome, Bunkyo-Ku, Tokyo, Japan
MISCHAIKOW, Micael K., Western Washington State, Dept. of Econ., Bellingham, Wash. 98225
MISES, Ludwig, 777 West End Ave., New York 25, N.Y.
MISHAN, Edward J., London Sch. of Econ., Houghton St., Aldwych, London, W.C.I. England
MISSIAEN, Edmond, 818 N. 60th St., Wauwatosa, Wis. 53213
MITCHELL, A. Cameron, Univ. of Houston, Gen. Bus. Dept., Houston, Tex. 77004
MITCHELL, Bridger M., Massachusetts Inst. of Tech., Dept. of Econ., Bldg. 52-243-D, Cambridge 39, Mass.
MITCHELL, Edward J., RAND Corp., 1600 Main St., Santa Monica, Calif.
MITCHELL, Francis H., 1819-1/4 Midvale Ave., Los Angeles, Calif. 90025
MITCHELL, Garlan E., 6746 Underhill, Houston, Tex. 77018
MITCHELL, George W., Bd. of Gov. of the Fed. Res. Sys., Washington, D.C. 20551
MITCHELL, Glen H., Southern Illinois Univ., Dept. of Agric. Econ., Carbondale, Ill. 62903
MITCHELL, Harry A., 2038 Pine St., New Orleans, La.
MITCHELL, Howard E., 303 21st St., Bellingham, Wash. 98225
MITCHELL, John M., 1060 Westover Rd., Stamford, Conn. 06902
MITCHELL, (Mrs.) Mary P., Bd. of Gov. of the Fed. Res. Sys., Washington, D.C. 20551
MITCHELL, Vance F., Univ. of California, Sch. of Bus. Admin., Barrows Hall, Rm. 350, Berkeley, Calif. 94720
MITCHELL, Waldo F., 1508 N. 7th St., Terre Haute, Ind.
MITCHELL, William E., 1696 C Northwinds Estates Dr., St. Louis, Mo. 63136

MITCHELL, William J., 732 Scotland St., Apt. 9, Williamsburg, Va. 23185
MITCHELL, William N., P.O. Box 706, Walworth, Wis. 53184
MITCHELL, William T., 950 Cabrillo Ave., Coronado, Calif. 92118
MITTRA, Sid, Univ. of Detroit, Dept. of Econ., Detroit, Mich. 48221
MIZE, Jan L., Georgia State Col., Atlanta, Ga. 30303
MLADEK, Jan Viktor, Int. Monetary Fund, 19th and H Sts., N.W., Washington, D.C.
MLELA, Johannes G., Wilberforce Univ., Box 332, Wilberforce, Ohio 45384
MOAK, Sah Kuhn, Box 5837 Col. Sta., Raleigh, N.C. 27607
MOAREFI, Ali, Economic Counsel, P.O. Box 755, Teheran, Iran
MOBLEY, Dorothy A., 520 E. 81st St., Apt. 8-C, New York, N.Y. 10028
MODAK, Nahar Divakar, Cons. Min. and Smelt. Co. of Canada, 630 Dorchester Blvd. W., Montreal, P.Q., Canada
MODER, Irving, 9802 Cherry Tree Lane, Silver Spring, Md. 20901
MODERN, Ernest, 1065 Walnut St., Newton Highlands, Mass. 02161
MODIGLIANI, Franco, Massachusetts Inst. of Tech., 50 Memorial Dr., Cambridge, Mass. 01922
MODLIN, Carey P., Jr., 3056 Porter St., N.W., Washington, D.C. 20008
MODLIN, George M., Univ. of Richmond, Richmond, Va.
MODROW, William G., Univ. of South Florida, Col. of Bus. Admin., Tampa, Fla. 33620
MOE, Albert F., 4729 N. Washington Blvd , Arlington, Va. 22205
MOEDE, Herbert H., 2107 N. Taft St., Apt. 18, Arlington 1, Va.
MOELLER, Charles, Jr., 288 Prospect Ave., Little Silver, N.J. 07739
MOFFAT, William R., 73 Martin St., Cambridge, Mass. 02138
MOGHADAM, Gholam R., Int. Monetary Fund, 19th and H St., N.W., Washington, D.C. 20431
MOGILNITSKY, Theodosi A., Loyola Univ., 820 N. Michigan Ave., Chicago, Ill. 60611
MOHR, Coenraad Luttig, St. Louis Univ., Sch. of Com. and Fin., 3674 Lindell Blvd., St. Louis, Mo. 63108
MOHR, Lawrence C., 7 W. 14th St., 10-G, New York, N.Y. 10011
MOHR, (Mrs.) Lillian H., 108 Single Dr., North Syracuse, N.Y. 13212
MOHRING, Herbert Dick, Univ. of Minnesota, Center for Econ. Res., Minneapolis, Minn. 55455
MOHRMAN, P. Christopher, 29 Bright St., Westbury, N.Y.
MOHTADI, Mack, 2864 Brinker, Ogden, Utah
MOISEEFF, Moise, Mills Estate, 2809 Mariposa Dr., Burlingame, Calif.
MOK, Victor, St. Mary's Col., St. Mary's College, Calif.
MOLHUYSEN, Philipp C., Univ. of New England, Dept. of Econ., Armidale, N.S.W., Australia
MOLONEY, John F., P.O. Box 12023, Memphis, Tenn. 38112
MOLTEN, Richard W., 312 Wateree Ave., Columbia, S.C. 29205
MOLZ, Ferdinand, 6609 Gude Ave., Takoma Park, Md. 20012
MONETTE, Brother Victor, Pius XII Sch., Gibson Hill Rd., Chester, N.Y. 10918
MONHOLLON, Jimmie R., 1013 Beveridge Rd., Richmond, Va. 23226
MONIZ, James, Upsala Col., Dept. of Econ., East Orange, N.J.
MONK, Paul, Route 1, Box 332, Mentone, Calif. 92359
MONROE, Arthur E., 5 Concord Ave., Cambridge, Mass. 01922
MONROE, Day, 2311 W. 17th St., Topeka, Kans. 66604
MONSEN, R. Joseph, Jr., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
MONSMA, George N., Jr., Princeton Univ., 2624 Grad. Col., Princeton, N.J. 08540
MONSON, Norman P., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
MONSON, Terry D., 714 N. State, St. Ignace, Mich. 49781
MONTAVON, Paul A., USAID/Quito, c/o American Embassy, Quito, Ecuador
MONTEALEGRE, Jorge A., Corp. Nicaraguense de Inversiones, Managua, Nicaragua
MONTEITH, Chalmers A., Kent State Univ., Col. of Bus. Admin., Kent, Ohio
MONTELEONE, Carol, 15 Mill St., Newton, N.J. 07860
MONTGOMERY, Albert A., 105 Joe St., Pullman, Wash. 99163
MONTGOMERY, George, Kansas State Univ., Dept. of Econ., Manhattan, Kans.
MONTGOMERY, Jean, Univ. of North Carolina, Dept. of City and Reg. Plan., Chapel Hill, N.C.
MONTGOMERY, Richard W., 215 S. Plum Grove Rd., Palatine, Ill. 60067
MONTGOMERY, Robert D., Univ. of North Carolina, Carroll Hall, Chapel Hill, N.C.
MONTGOMERY, Sarah S., Mt. Holyoke Col., Dept. of Econ., South Hadley, Mass. 01075
MONTI, Angel F., Uruguay 484, San Isidro (Punta Chica), Buenos Aires, Argentina
MONTIAS, John Michael, Yale Univ., 37 Hillhouse Ave., New Haven, Conn.
MONTICELLO, Ralph R., Jr., Univ. of Cincinnati, French Hall, Cincinnati, Ohio 45221
MONTOYA, (Mrs.) Dorothy H., 13900 Fiji Way, Apt. 208, Villa Venetia, Marina del Rey, Calif. 90291
MONTRIE, Charles J., 6406 Wiscasset Rd., Washington 16, D.C.
MOODY, Harold T., 9246 Geyser Ave., Northridge, Calif. 91325
MOON, (Mrs.) Jeannette B., Atlanta Bd. of Educa., Dept. of Instruction, City Hall, Atlanta 3, Ga.
MOONEY, Joseph D., Princeton Univ., Ind. Rela. Sec., P.O. Box 248, Princeton, N.J. 08540
MOONEY, Michael B., 1987 N. Star Rd., Columbus, Ohio 43212
MOONITZ, Maurice, Univ. of California, Sch. of Bus. Admin., Berkeley, Calif. 94720
MOOR, Roy E., Horseshoe Trail, Valley Forge, Pa. 19481
MOORE, A. Milton, Dalhousie Univ., Dept. of Econ., Halifax, N.S., Canada
MOORE, Amos M., Rutgers Univ., Dept. of Econ , New Brunswick, N.J.
MOORE, (Mrs.) Anne C., 2800 Woodley Rd., N.W., Apt. 312, Washington, D.C. 20008
MOORE, (Mrs.) Avis P., 2840 Sheridan Rd., Evanston, Ill.
MOORE, Basil J., Wesleyan Univ., Dept. of Econ., Middletown, Conn.
MOORE, Charles C., International Labor Office, Geneva, Switzerland
MOORE, Charles G., 810 Clark St., Evanston, Ill. 60201
MOORE, Charles T., 31 University Circle, Tuscaloosa, Ala. 35401
MOORE, Clarence A., 5021 N. 24th St., Arlington, Va.
MOORE, Donald A., Los Angeles State Col., Dept. of Econ., 5280 Gravois Ave., Los Angeles 32, Calif.
MOORE, Donald S., A. and M. Col. of Texas, Dept. of Agric. Econ., College Station, Tex.
MOORE, Elmer J., 617 Underwood St., N.W., Washington, D.C. 20012
MOORE, Floyd W., 1320 Maple St., Kalamazoo, Mich.
MOORE, Frederick T., RAND Corp., 1000 Connecticut Ave., N.W., Washington, D.C.
MOORE, Geoffrey H., 144 S. Maple Ave., Ridgewood, N.J. 07450
MOORE, Gerard E., General Motors Bldg., Room 13-124, Detroit, Mich.
MOORE, H. J., 600 Chatham Drive, Midland, Mich.
MOORE, Howard, 1321 Academy St., San Marcos, Tex. 78666
MOORE, J. Robert, 1701 Windsor Ave., Lancaster, Pa. 17601
MOORE, James J., Econ. Dev. Bd., Rm. 112, Legislative Bldg., Regina, Sask., Canada
MOORE, John Benjamin, Lindenwood Col., St. Charles, Mo.
MOORE, John H., 8-6 New Copeley Hill, Charlottesville, Va.

MOORE, John R., Univ. of Tennessee, Dept. of Econ., Knoxville, Tenn.
MOORE, John Runyan, Univ. of Maryland, Dept. of Agric. Econ., College Park, Md.
MOORE, Kenneth L., Mich. Bell Tel. Co., 1365 Cass, Detroit 26, Mich.
MOORE, Mack A., Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta, Ga.
MOORE, Michael F., 424 N. Pinckney St., Madison, Wis. 53703
MOORE, O. Ernest, Int'l. Econ. Services, 60 E. 42nd St., Suite 1644, New York, N.Y. 10017
MOORE, (Mrs.) Patricia A., 709 E. 32nd St., Austin, Tex.
MOORE, Richard L., 2085 Longmont St., Riverside, Calif.
MOORE, (Mrs.) Shirley B., Univ. of Tennessee, Dept. of Econ., Knoxville, Tenn.
MOORE, Thomas G., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
MOORE, William H., 1 Scott Circle, Washington, D.C.
MOORE, William J., Richmond, Ky.
MOORES, Barbara L., 544 Thompson St., Ann Arbor, Mich.
MOOSE, Dan M., 2130 Renrock Rd., Cleveland, Ohio 44118
MOOSE, E. Duncan W., 6 Ash St., Cambridge, Mass. 02138
MOOTS, Harold J., 7502 Eagle Creek Dr., Dayton, Ohio 45459
MORAG, Amotz, Eliezer Kaplan Sch., Hebrew Univ., Jerusalem, Israel
MORALES, Victor H., 1080 Sterling Ave., Berkeley 8, Calif.
MORAN, Carl A., 2303 Kenilworth Blvd., San Antonio, Tex. 78209
MORAN, James P., 8 Frantone Lane, Albany, N.Y. 12211
MORAN, John G., 8059 88th Ave., Woodhaven, N.Y. 11421
MORAWETZ, John H., 77-36 252d St., Bellerose 26, L.I., N.Y. 14711
MOREHEAD, Donald, 112 E. Houstonia, Royal Oak, Mich. 48073
MOREHOUSE, Edward W., 65 Allison Rd., Princeton, N.J. 08541
MOREO, Dominic W., 7317 19th, N.E., Seattle, Wash. 98115
MOREWEDGE, Hosseine, 454 Riverside Dr., New York 27, N.Y.
MOREY, Donald J., 7516 S.W. 60th Ave., Portland 19, Ore.
MORGAN, Bruce W., Univ. of Illinois, Col. of Com. and Bus. Adm., Dept. of Econ., Urbana, Ill.
MORGAN, Charles S., 5721 Chevy Chase Pkwy., Washington, D.C.
MORGAN, Chester A., Univ. of Iowa, Col. of Bus. Admin., Iowa City, Iowa
MORGAN, Daniel C., Jr., Univ. of Texas, Dept. of Econ., Austin, Tex. 78712
MORGAN, Gerald D., Jr., 52 W. 84th St., New York, N.Y. 10024
MORGAN, Howard E., Univ. of Arizona, Col. of Bus. and Pub. Admin., Tucson, Ariz.
MORGAN, J. Theodore, Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706
MORGAN, James N., 1217 Bydding Rd., Ann Arbor, Mich.
MORGAN, John D., 7000 Southridge Dr., McLean, Va.
MORGAN, Roger L., 2307 Rainbow Ave., Laramie, Wyo.
MORGAN, W. Douglas, 321-D 63rd St., Oakland 18, Calif.
MORGAN, William E., AID, APO New York 09254
MORGENSTEIN, Sam A., 108 E. Indian Spring, Silver Spring, Md.
MORGENSTERN, Otto, Univ. of San Francisco, Dept. of Econ., San Francisco 17, Calif.
MORGNER, Aurelius, Univ. of Southern Calif., Econ. Dept., Los Angeles 7, Calif.
MORGO, Joseph G., 34 Lake St., Brighton, Boston, Mass.
MORI, Thomas S., 101 Yamazato-cho, Showa-ku, Nagoya, Japan
MORIAK, Theo F., 3536 S. Stafford St., Arlington, Va. 22206
MORIARTY, Donald W., Jr., 3210 Hampton Ave. #2, St. Louis, Mo. 63139
MORIN, Alexander J., Aldine Pub. Co., 320 W. Adams, Chicago, Ill. 60606
MORIN, Bernard A., Univ. of Virginia, McIntire Sch. of Com., Rouss Hall, Charlottesville, Va.
MORKRE, Morris E., 5106 N. Kenmore, Chicago, Ill. 60640
MORLEY, Burton, Box 1994, University, Ala. 35486
MORONEY, John R., Jr., Florida State Univ., Dept. of Econ., Tallahassee, Fla. 32306
MOROZ, Osyp, 67 S. Devoe Ave., Yonkers 5, N.Y.
MORRALL, John F., III, 301-C W. Main St., Carrboro, N.C. 27510
MORREAU, Jean G., 80 Lijsterlaan, Bussum, The Netherlands
MORRILL, Chester, 3908 Ingomar St., N.W., Washington 15, D.C.
MORISSE, Kathryn A., 2625 O St., N.W., Washington, D.C. 20007
MORRIS, Bruce R., Univ. of Massachusetts, Econ. Dept., Amherst, Mass.
MORRIS, Clair Edward, Jr., 102 K Eagle Heights Apts., Madison, Wis.
MORRIS, (Mrs.) Cynthia T., 3030 Arizona Ave., N.W., Washington 16, D.C.
MORRIS, Frank E., Yorkshire Rd., Dover, Mass. 02030
MORRIS, James A., 1117 Adger Rd., Columbia, S.C.
MORRIS, James R., 3903 Lakota Rd., Alexandria, Va. 22303
MORRIS, John R., Jr., 18 E. Myrtle St., Alexandria, Va.
MORRIS, Malcolm L., Indiana Univ., Grad. Sch. of Bus., Bloomington, Ind. 47405
MORRIS, Morris David, Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
MORRIS, Ruby T., Connecticut Col., Dept. of Econ., New London, Conn.
MORRIS, S. Stanley, Grambling Col., Grambling, La. 71215
MORRIS, Thomas M., Maryknoll Seminary, Glen Ellyn, Ill. 60137
MORRIS, Victor F., 837 Grange Rd., Teaneck, N.J. 07666
MORRIS, William J., Jr., 6614 Lone Oak Dr., Bethesda, Md. 20034
MORRISON, Alexander H., Box 543, Lexington, Va.
MORRISON, Clarence C., James Wilson Dept. of Econ., Rouss Hall, Charlottesville, Va. 22903
MORRISON, Donald G., 2254 N. Clark St., Chicago, Ill.
MORRISON, George R., 309 Niagara Falls Blvd., Buffalo, N.Y. 14226
MORRISON, Jay, 1927 Shenandoah Dr., Seattle 2, Wash.
MORRISON, John M., Norfolk Ave., Norfolk, Neb. 68701
MORRISON, Paul Leslie, 2115 Central Park Ave., Evanston, Ill. 60201
MORRISON, Rodney J., 6 Weston Ter., Wellesley, Mass. 02181
MORRISON, Vernon G., R. F. D. 1, Carbondale, Ill. 62901
MORRISSETT, Irving, Purdue Univ., Dept. of Econ., West Lafayette, Ind. 47178
MORRISSEY, Frederic P., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
MORRISSEY, Frederick W., San Francisco State Col., Econ. Dept., 1600 Holloway Ave., San Francisco, Calif.
MORRISSEY, George R., 4140 E. Del Mar Ave., Long Beach, Calif. 90807
MORROW, David W., 21665 Detroit Rd., Apt. 113, Rocky River, Ohio 44116
MORROW, Glenn D., 4848 Chevy Chase Dr., Chevy Chase, Md. 21620
MORROW, Henry G., 1660 E. 21st St., Brooklyn 10, N.Y.
MORS, Wallace Peter, Babson Inst., Babson Park 57, Mass.
MORSCH, William C., c/o CVG, Apartado 7.000, Caracas, Venezuela
MORSE, Chandler, Cornell Univ., Dept. of Econ., Ithaca, N.Y.
MORSE, Emanuel A., Merrill Lynch, Pierce, Fenner and Smith, Inc., 70 Pine St., New York 5, N.Y.

MORSE, Herbert C., 333 S. Kensington, La Grange, Ill.
MORSE, Lawrence B., Univ. of Minnesota, Center for Econ. Res., Minneapolis, Minn. 55455
MORSE, Richard, Ford Found., 32 Ferozshah Rd., New Delhi 1, India
MORSE, Richard L. D., Kansas State Univ., Dept. of Family Econ., Manhattan, Kans.
MORSS, Elliott R., Hamilton House, 1255 New Hampshire Ave., N.W., Washington, D.C. 20036
MORTENSEN, Erik H., Ministry of Agriculture, Slotsholmsgade 10, Copenhagen, Denmark
MORTENSEN, Jorgen, c/o Miss E. Andersen, Univ. of Copenhagen, Studiestraede 6, Copenhagen, Denmark
MORTON, Herbert C., Brookings Institution, 1775 Massachusetts Ave., N.W., Washington 6, D.C.
MORTON, Joseph E., 142 E. 16 St., New York, N.Y. 10003
MORTON, John Elliott, 1645 Ridge Ave., Evanston, Ill. 60201
MORTON, Richard K., Jacksonville Univ. Contract Br., Jacksonville, Fla. 32211
MORTON, Robert D., 18 Norman St., Manchester, Conn. 06040
MORTON, Walter A., Univ. of Wisconsin, Dept. of Econ., Soc. Sci. Bldg., Madison 6, Wis.
MOSAK, Jacob L., United Nations, New York, N.Y.
MOSCARELLA, Joseph, c/o Comision Econ. para Amer. Latin., Apartado Postal 20718, Mexico 6, D.F., Mexico
MOSCONI, David L., 1142 S. Williams, Denver 9, Colo.
MOSCOSO, Guillermo, Rep. in Mexico, United Calif. Bank, P.O. 1425, Mexico 1, D.F., Mexico
MOSER, Collette H., 428 State St., Madison 3, Wis.
MOSES, Leon N., 2810 Harrison St., Evanston, Ill. 60201
MOSHER, Norman William, 3315 Ross Pl., N.W., Washington 8, D.C.
MOSKOFF, William, St. Norbert Col., Dept. of Econ., West DePere, Wis.
MOSKOW, Michael H., Drexel Inst. of Tech., Col. of Bus., Philadelphia, Pa. 19104
MOSKOWITZ, (Mrs.) Ester B., 55 E. 88th St., Apt. 2 W, New York, N.Y. 10028
MOSKOWITZ, Leon D., One North Dr., Great Neck, N.Y.
MOSS, Bennett R., 315 Broadleaf Dr., Vienna, Va. 21180
MOSS, Milton, 2518 Harmon Rd., Silver Spring, Md.
MOSS, Morton F., Northwest Acceptance Corp., 310 N.E. Oregon St., Portland 14, Ore.
MOSS, Richard K., 704 Powell St., Williamsburg, Va.
MOSS, Robert Warren, 519 Independence Ave., S.E., Washington, D.C. 20003
MOSS, William C., 5250 H St., Sacramento, Calif. 95819
MOSSAWIR, Albert G., 30 East End Ave., New York 28, N.Y.
MOSSBARGER, John W., 5724 Carb Dr., Ft. Worth, Tex. 76114
MOSSE, Mair, Av. Atlantica 1588, Apto. 1001, Rio de Janciro, Brazil
MOSSE, Robert, 12, Ave. Rochambeau, Grenoble, France
MOSSMAN, Frank H., Michigan State Univ., Dept. of Mktg. and Trans. Admin., East Lansing, Mich.
MOSZER, Max, 419 East Wynnewood Rd., Narberth, Pa. 19072
MOTHERAL, Joe R., USOM/K-RDD APO 301, San Francisco, Calif. 96301
MOTLEY, Brian, 2335 Russell, Apt. A, Berkeley, Calif.
MOTT, Charles F., 424 S. Jordan, Bloomington, Ind. 47403
MOTTER, David C., Office of the Comp. of Curr., Rm. 4458, Main Treasury Bldg., Washington 25, D.C.
MOTTOROS, Pierre P., 8 Cardinal Lane, Hauppauge, N.Y. 11788
MOULTON, Elma S., 5617 Lamar Rd., Washington 16, D.C.
MOUNT, Charles, 701 Willow Ave., Big Rapids, Mich. 49307
MOUNTFORD, Alexander, Int. Monetary Fund, 19 and H St., N.W., Washington, D.C. 20431
MOUTSANIDES, Demetrius, Central Washington State Col., Dept. of Econ. and Bus., Ellensburg, Wash. 98926
MOVSESIAN, Constance, 912 Cherry Lane, Riverton, N.J. 08077
MOWBRAY, Kermit, 2822 Huntoon, Topeka, Kans. 66604
MOWLL, Jack U., 4405 East-West Hwy., Bethesda, Md. 20014
MOY, Henry K. L., 13402 Lydia St., Silver Spring, Md. 20906
MOY, Joseph, 180 Park Row, Apt. 7C, New York, N.Y. 10038
MOYER, Fred W., Jr., 104 S. Meadowcroft Dr., Akron 13, Ohio
MOYER, Reed, 471 Butterfield Dr., East Lansing, Mich.
MOYER, William F., 175 Kildare Rd., Garden City, N.Y. 11530
MU, Tzung Tsann, Univ. of California, Dept. of Econ., Davis, Calif.
MUDD, Harmon L., 5 E. Ninth St., New York 3, N.Y.
MUDGE, James L., 207 Francis St., Longmont, Colo.
MUDGETT, Bruce, Hacienda Carmel, Carmel, Calif. 93921
MUDIE, John, Gov. Dev. Bank for Puerto Rico, Econ. Res. Dept., San Juan, P.R.
MUEGGLER, Roy P., 1650 Arthur Pl., Eugene, Ore. 97402
MUEHLNER, Felix, Univ. of Khartoum, Fac. of Econ. and Soc. Sci., Khartoum, Sudan
MUELLER, Dennis C., Simon Fraser Univ., Dept. of Econ., Burnaby 2, B.C., Canada
MUELLER, Eva, 606 Robin Rd., Ann Arbor, Mich.
MUELLER, Franz H., 2110 Wellesley Ave., St. Paul, Minn. 55105
MUELLER, Frederick W., De Paul Univ., Sch. of Com., 25 E. Jackson Blvd., Chicago, Ill. 60604
MUELLER, Hans G., Middle Tennessee State Univ., Dept. of Econ., P.O. Box 304, Murfreesboro, Tenn. 37130
MUELLER, (Mrs.) Marnie W., 171 W. Lincoln Ave., Libertyville, Ill. 60048
MUELLER, Max G., Univ. of California, Dept. of Econ., 405 Hilgard Ave., Los Angeles 24, Calif.
MUELLER, Willard F., 357 O St., S.W., Washington 24, D.C.
MUENCH, Carl, 322 7th St., N.W., Waverly, Iowa 50677
MUENCH, John, Jr., Nat. Forest Products Asso., 1619 Massachusetts Ave., N.W., Washington, D.C. 20036
MUENCH, Thomas J., 5343 Sanger Ave., Apt. 252, Alexandria, Va. 22311
MUKERJEE, Tapan, Univ. of New Hampshire, B-8 Forest Park, Durham, N.H. 03824
MUKHOPADHYAY, Sudhendu N., 11-13, Khyber Pass Mess, Delhi-6, India
MULDER, G. C. A., Laan van Meerdervoort 329, The Hague 8, The Netherlands
MULDER, Nick G., Wilmot Apts., F-8, Fredericton, N. B., Canada
MULHOLLAND, Edward J., 10 Garth Rd., West Roxbury, Mass. 02791
MULHOLLAND, Joseph P., Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
MULLADY, (Mrs.) Philomena M., 1415 Tuckerman St., N.W., Washington, D.C. 20011
MULLEN, James H., Temple Univ., Dept. of Mgt., Philadelphia, Pa. 19122
MULLEN, Joseph W., 6172 Delafield Ave., Bronx, N.Y.
MULLENBACH, Philip, 626 Washington Ave., Wilmette, Ill. 60091
MULLENNIX, Grady L., Fresno State Col., Fresno, Calif. 93726
MULLER, (Mrs.) Charlotte F., 346 Elm Pl., Leonia, N.J. 07605
MÜLLER-GROELING, Hubertus, Lessingstrasse 58, 66 Saarbruken, Germany
MULLIGAN, Charles F., 565 Seneca Pkwy., Rochester, N.Y. 14613
MULLIGAN, Eugene W., 1104 Trinity Dr., Alexandria, Va. 22314
MULLIGAN, Paul F., Jr., Duke Univ., Dept. of Econ., Durham, N.C.

MULLIGAN, Robert M., 9 Midway Dr., North Kingstown, R.I. 02852
MULLIN, Gail E., Kent State Univ., Col. of Bus. Admin., Kent, Ohio 44240
MULLINGS, Lewellyn M., Univ. of Bridgeport, Econ. Dept., Bridgeport 4, Conn.
MULLINS, James E., Marquette Univ., Dept. of Econ., Milwaukee 3, Wis.
MULLINS, Mary, 6643 Kingsbury, St. Louis, Mo. 63130
MULLINS, Peter L., Apt. 64-E, Escondido Village, Stanford, Calif. 94305
MULLINS, Roland T., Arkansas State Col., Dept. of Bus. Admin., State College, Ark. 72467
MUMEY, Glen A., Univ. of Saskatchewan, Col. of Com., Saskatoon, Sask., Canada
MUNCH, James C., Jr., 293 Green Ave., Lansdowne, Pa.
MUND, Vernon A., Univ. of Washington, Dept. of Econ., Seattle, Wash.
MUNK, Bernard E., 1800 19th St., N.W., Washington, D.C. 20009
MUNNICH, Frank, Albert Schweitzer Str. 6, 6908 Wiesloch, Baden, Germany
MUNOZ, Ricardo, 4305 S. 36th St., Arlington 6, Va.
MUNRO, John M., W468 Evermann Apts., Bloomington, Ind.
MUNROE, (Mrs.) Juliet, 2468 Tracy Pl., N.W., Washington 8, D.C.
MUNSON, Fred Caleb, Univ. of Michigan, Sch. of Bus., Bur. of Ind. Rela., Ann Arbor, Mich.
MUNSTER, J. H., Waldorf Towers, 2300 Overlook, Cleveland Heights 6, Ohio
MURAD, Anatol, Univ. de Puerto Rico, Colegio De Ciencias Sociales, Rio Piedras, P.R. 00745
MURANS, Francis, Loyola Univ., Dept. of Econ., 820 N. Michigan Ave., Chicago, Ill.
MURAOKA, Tom, 4530 25th Ave., Sacramento, Calif. 95820
MURCHISON, C. T., Wellfleet, Massachusetts 02667
MURCHISON, Richard L., 201 David Hall ROHR, Rodney Village, Dover, Del. 19901
MURDICK, Robert G., State Univ. of New York at Albany, Sch. of Bus., Albany 3, N.Y.
MURDOCK, John Carey, Univ. of Missouri, Sch. of Bus. Admin., Dept. of Econ., Columbia, Mo.
MURGO, John D., Wentworth Inst., 550 Huntington Ave., Boston, Mass. 02115
MURIITHI, D. Wanjohi, Makerere Univ. Col., Dept. of Econ., P.O. Box 262, Kampala, Uganda
MURPHY, Austin S., 49 Brewster Rd., Scarsdale, N.Y. 10583
MURPHY, Caroline A., 20 Boardman St., Westboro, Mass. 01581
MURPHY, Duncan Witten, Canyon Inn, Spencer, Ind. 47460
MURPHY, Edward E., Georgetown Univ., Dept. of Econ., Washington 7, D.C.
MURPHY, (Mrs.) Elenor G., 4520 Chesapeake St., N.W., Washington, D.C. 20016
MURPHY, Francis Patrick, 118 Meadbrook Rd., Garden City, N.Y. 11530
MURPHY, Henry Clifford, Berkshire Apts., 4201 Massachusetts Ave., N.W., Washington 16, D.C.
MURPHY, J. Carter, Southern Methodist Univ., Dept. of Econ., Dallas 22, Tex.
MURPHY, J. R., U.S. Military Acad., West Point, N.Y. 10996
MURPHY, James L., Univ. of North Carolina, Dept. of Econ., Chapel Hill, N.C. 27514
MURPHY, James T., Tulane Univ., Sch. of Bus. Admin., New Orleans, La.
MURPHY, John A., 9618 Woodberry St., Seabrook, Md. 20802
MURPHY, John J., Catholic Univ. of America, Dept. of Econ., Washington, D.C. 20017
MURPHY, John M., Niagara Univ., Dept. of Econ., Niagara University, N.Y.
MURPHY, Mary E., 302 Grace Dr., South Pasadena, Calif.
MURPHY, Neil B., 76 Park Ave., Natick, Mass. 01760
MURPHY, Robert J., 10 Rainbow Rd., Bridgeport, Conn. 06606
MURPHY, Robert L., 7 Juniper Lane, Medfield, Mass. 02052
MURPHY, Stephen E., 4820 N.E. 43rd St., Seattle, Wash. 98105
MURPHY, T. Henry, 16 Shermon Rd., Glen Cove, N.Y. 11542
MURPHY, William, Providence Col., Economics Dept., Providence, R.I.
MURPHY, William T., 138 Maple St., Potsdam, N.Y. 13676
MURRAY, Alan P., 8302 Bryant Dr., Bethesda, Md. 20034
MURRAY, Barbara B., 32208 Bonnet Hill, Farmington, Mich. 48024
MURRAY, Donald W., 2440 16th St., N.W., Apt. 107, Washington, D.C. 20009
MURRAY, Jack W., Trinity Univ., Dept. of Econ., San Antonio, Tex. 78212
MURRAY, James M., Moorhead State Col., Dept. of Econ., Moorhead, Minn.
MURRAY, John A., 501 W. Green, Champaign, Ill. 61822
MURRAY, Matthew, 5025 Goodridge Ave., Riverdale, N.Y. 14775
MURRAY, Roger F., TIAA-CREF, 730 Third Ave., New York, N.Y. 10017
MURRAY, Tracy W., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
MURRAY, William G., Iowa State Col., 207 Agric. Annex, Ames, Iowa
MURRY, Donald A., Univ. of Missouri at St. Louis, 8001 Natural Bridge Rd., St. Louis, Mo. 63121
MURTY, P. S. K., Nat. Accounts Div., Rm. 230, Dom. Bur. of Statis., Ottawa, Ont., Canada
MURUA, Ricardo, Centro de Investigaciones Econ., Casilla de Correo 594, Mendoza, Argentina
MUSALEM, Alberto, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
MUSCAT, Robert J., Agency for International Dev., Rio de Janeiro, Brazil
MUSGRAVE, John C., 159 Boulevard Pereire, Paris 17, France
MUSGRAVE, (Mrs.) Peggy, Harvard Univ., Dept. of Econ., Cambridge, Mass.
MUSGRAVE, Richard A., Harvard Univ., Dept. of Econ., Cambridge, Mass.
MUSTAFA, Tahseen Hamid, Min. of Labor and Soc. Affairs, Baghdad, Iraq
MUTH, John F., Michigan State Univ., Dept. of Mgt., East Lansing, Mich.
MUTH, Richard F., Inst. for Defense Analyses, 400 Army-Navy Dr., Arlington, Va. 22202
MUTSUNE, Wycliffe A., Univ. of Calif. at Los Angeles, African Studies Center, Los Angeles, Calif. 90024
MWANJISI, Frank, P.O. Box 9014, Dar-es-Salaam, Tanganyika
MYERS, A. Howard, 64 Garland Rd., Newton Center, Mass. 01355
MYERS, Buddy L., 5237 Fifth Ave., Apt. A-3, Pittsburgh, Pa. 15232
MYERS, Charles Andrew, Industrial Rela. Sec., M.I.T. 52-454A, Cambridge, Mass. 02139
MYERS, Frederic, Univ. of California, Sch. of Bus., Los Angeles 24, Calif.
MYERS, Herman L., 8224 Lilly Stone Dr., Bethesda, Md. 20034
MYERS, Kenneth H., 4159 Dover Rd., Pasadena, Calif. 91103
MYERS, Lawrence B., 238 Ruskin Rd., Buffalo, N.Y. 14226
MYERS, Leslie P., 89 Durie St., Toronto 3, Ont., Canada
MYERS, Margaret G., 24 Thelberg Rd., Poughkeepsie, N.Y. 12601
MYERS, Max, Ave. Bias Fortes 222, Belo Horizonte, M.G., Brazil
MYERS, Milton Linwood, Quassuk Rd., Woodbury, Conn. 06798
MYERS, Myron G., Brown Univ., Econ. Dept., Providence, R.I.
MYERS, Ramon H., Australian Nat. Univ., Econ. Dept., Sch. for Pac. Res., G.P.O. 4, Canberra, Australia
MYERS, Robert J., U.S. Bur. of Labor Statis., Washington 25, D.C.
MYERS, Samuel L., 2219 Windsor Ave., Baltimore 16, Md.
MYERS, Stewart C., Stanford Univ., Grad. Sch. of Bus., Doctoral Program, Stanford, Calif.
MYHR, Anders F., 3001 36th Ave. S., St. Petersburg, Fla. 33712

MYHRMAN, Johan, Box 23015, Stockholm 23, Sweden
MYLES, Jack C., 77 Walden St., West Hartford, Conn. 06090
MYRDAL, Gunnar, Inst. for Int. Econ. Studies, Sveav, 166, 19th Fl., Stockholm, Sweden
MYRICK, Donald, 312 E. Pedregosa St., Santa Barbara, Calif.
MYSLICKI, Chester, USAID/PAD, APO 205, New York, N.Y. 09205

NACK, Kenneth Richard, 225 E. 63rd St., New York, N.Y. 10021
NADAL, Francisco de P., Boston Col., Chestnut Hill, Mass. 01012
NADEL, Edward, Univ. of Maine, Dept. of Econ., Orono, Me. 04473
NADIRI, Mohammed I., Northwestern Univ., Dept. of Econ., Evanston, Ill. 60201
NADLER, Paul S., 100 Trinity Pl., New York 6, N.Y.
NADWORNY, Milton J., 33 Balsam St., Burlington, Vt. 05401
NAESS, Ragnar, 2 Broadway, New York 4, N.Y.
NAESSENS, Maurits, Ave. d'Hoogvorst 30, Meise, Belgium
NAFZIGER, E. Wayne, 1010 W. Green 76, Urbana, Ill.
NAGELL-ERICHSEN, Einar Fr., Aftenposten, Akersgaten 51, Oslo, Norway
NAGOURNEY, Donald, 84 Kirkwood St., Long Beach, L.I., N.Y.
NAGY, Alex G., 119 Audrey Alene, San Antonio, Tex. 78228
NAHL, Perham C., 1907 Lincoln St., Evanston, Ill. 60201
NAIDEL, Simon, 2000 F St., N.W., Washington 6, D.C.
NAIL, J. F., 12772 Elizabeth Way, Tustin, Calif. 92680
NAIR, Kumaran R., State College Station, P.B. 5838, Raleigh, N.C. 27607
NAITO, William S., 11025 S.W. Tryon, Portland, Ore.
NAKAGAMA, Sam I., 28 Gifford Rd., Somerset, N.J. 08873
NAKAMURA, James I., 501 W. 121st St., New York 27, N.Y.
NAKAMURA, Tsutomu, 90 Yamazato-cho, Showa-ku, Nagoya, Japan
NAKAR, Ram Swarup, 153 Kakanagar, New Delhi, India
NAKAYAMA, Ichiro, 861 Kombacho, Meguro-Ku, Tokyo, Japan
NAKAYAMA, Saburo, Japanese Fed. of Emp. Asso., Inst. of Labor Econ. Res., 2-1 Chome, Chiyoda-ku Marunouchi, Tokyo, Japan
NALESZKIEWICZ, Wladimir, Univ. of Notre Dame, Dept. of Econ., Notre Dame, Ind. 46556
NALLEN, Leslie R., 5314 Stock Rd., Columbus, Ohio 43224
NAM, Woo H., 4737 Brooklyn Ave., N.E., Seattle, Wash 98105
NAMIAS, Jean, 80 Seaman Ave., New York 34, N.Y.
NANDOLA, Kahandas N., 233 S. 41st St., Philadelphia, Pa.
NAQVI, Syed N. H., 116 Witherspoon St., Princeton, N.J. 08540
NARGUND, Narayan K., 82-C Finderne Ave., Somerville, N.J.
NARASIMHAM, G. V. L., Kansas State Univ., Dept. of Econ., Manhattan, Kans. 66504
NARAYANAN, R., Janaki Mansion No. 7, Bajana Koil First St., Kodambakkam, Madras-24, India
NARAYANAN, U. S., Univ. of Wisconsin, Soc. Sci. Bldg., Room 343, Madison, Wis. 53706
NARVER, John C., Univ. of Washington, Sch. of Bus. Admin., Seattle, Wash.
NASH, Kent D., North Carolina State Univ., Dept. of Econ., Bldg. 1911, Rm. 210, Raleigh, N.C. 27607
NASLUND, Bertil, Algrytevagen 43, Skarholmen, Sweden
NATESAN, L. A., C Inderpuri, New Delhi 12, India
NATHAN, Kurt, U.S. Dept. of State—Bogota, Washington, D.C. 20521
NATHAN, Otto, 55 E. Tenth St., New York 3, N.Y.
NATHAN, Robert R., R. Nathan Associates, 1218 Sixteenth St., N.W., Washington 6, D.C.
NATRELLA, Vito, 1718 N. Hartford St., Arlington, Va. 22201
NAUMANN, Jens, Tile-Brugge-Weg 17, 1 Berlin 27, Germany
NAVARRO, Alfredo M., Rivadavia 3026, 6° Piso, Mar del Plata-RCA., Argentina
NAVARRO-V, Agustín, Plaza 20 de Nov., #2-F 405, Mexico 1, D.F., Mexico
NAVIN, Leo J., 1325 W. Grand River, Apt. 72-B, East Lansing, Mich. 48823
NAWA, Toshio, Sumitomo Shoji New York, Inc., San Francisco Office, 25 California St., San Francisco, Calif. 94111
NAYLOR, Martyn R. P., Ryuwa Bldg., 2, 2-chome, Yuraku-chu, Chiyoda-ku, Tokyo, Japan
NAYLOR, Thomas H., Duke Univ., Dept. of Econ. and Bus. Admin., Durham, N.C.
NAZ, Mian Abdul Hameed, Plaza Towers East, 6700 Belcrest Rd., Hyattsville, Md. 20780
NEACE, Myron B., Emory Univ., Sch. of Bus. Admin., Atlanta, Ga. 30322
NEAL, Alfred C., 57 Forest Ave., Rye, N.Y. 10580
NEAL, (Mrs.) Marguerite S., 57 Forest Ave., Rye, New York 10580
NEALE, Walter C., Univ. of Texas, Dept. of Econ., Austin 12, Tex.
NEDD, Eustace, 175-07 109th Ave., Jamaica, N.Y. 11433
NEDECZKY, Frank, 80-45 Winchester Blvd., Queens Village 27, N.Y.
NEEDHAM, W. Robert, Univ. of Waterloo, Dept. of Econ., Waterloo, Ont., Canada
NEEDLEMAN, Lionel, London Sch. of Econ., Houghton St., Aldwych, London W.C. 2, England
NEEDY, Charles W., Jr., 324 S. West St., Stillwater, Okla.
NEEL, Richard E., 2821 Kimberly Lane, Tampa 18, Fla.
NEELY, Frederick Tilden, 38 Waverly Dr., Jackson, Tenn.
NEELY, Richard, 2173 Yale Sta., New Haven, Conn. 06520
NEENAN, William B., 114 N. Ingalls St., Ann Arbor, Mich.
NEFF, Louis E., Monroe Community Col., Dept. of Soc. Sci., 410 Alexander St., Rochester, N.Y. 14607
NEFF, Philip, Planning Res. Corp., 1100 Glendon Ave., Los Angeles, Calif. 90024
NEGRI, Barbara, 225 E. 70th St., New York, N.Y. 10021
NEHER, Philip A., 118 Williams St., Providence, R.I. 02906
NEHMER, Stanley, 9007 Kirkdale Rd., Bethesda, Md. 21811
NEHRING, John A., 70192A, 32 TACRECON SQDN, APO New York, N.Y. 09083
NEIDERT, Kalo E., Univ. of Nevada, Col. of Bus. Admin., Reno, Nev.
NEIFELD, Morris, 649 Prospect St., Maplewood, N.J. 07040
NEIL, Herbert E., Jr., 641 Timber Hill Rd., Deerfield, Ill. 60015
NEILL, Weldon C., 5004 Live Oak, Apt. 204, Dallas, Tex. 75206
NEILS, Allan E., 7-D Solano Park, Davis, Calif. 95616
NEIMANIS, George J., 1197 N. Broadway, North Massapequa, N.Y. 12967
NEISSER, Albert C., 7112 44th St., Chevy Chase, Md. 21620
NEISSER, Hans P., 11 Park Ave., Greenvale, N.Y. 11548
NEISWANGER, William A., Jr., Univ. of Illinois, Col. of Com., Dept. of Econ., Urbana, Ill.
NELL, Edward J., Wesleyan Univ., Dept. of Econ., Middletown, Conn.
NELLES, Duane, 451 Lake Park, Birmingham, Mich. 48009
NELSEN, Clair E., 3917 N. Cedar, Fresno, Calif. 93726
NELSON, Charles R., 21 W. Gilman St., Madison 03, Wis.
NELSON, Boyd L., 9826 Hedin Dr., Silver Spring, Md.
NELSON, Clarence W., Fed. Res. Bank of Minneapolis, Minneapolis, Minn. 55440
NELSON, Cora L., P.O. Box 792, Davidson, N.C. 28036
NELSON, David C., North Dakota State Univ., Dept. of Agric. Econ., Fargo, N.D.

NELSON, David P., 3477 Westmount Ave., Los Angeles 43, Calif.
NELSON, Emil L., 900 Burnt Crest Lane, Silver Spring, Md. 20903
NELSON, George J., 12 E. 93rd St., New York 28, N.Y.
NELSON, J. Russell, Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
NELSON, James Cecil, Washington State Univ., Dept. of Econ., Pullman, Wash.
NELSON, James R., 67 Dana St., Amherst, Mass.
NELSON, Jesse J., 1810 Clover Ct., Columbus, Ind. 47201
NELSON, Larry C., Muskingum Col., Dept. of Econ., New Concord, Ohio 43762
NELSON, (Mrs.) Myrtle Gill, 7912 Robison Rd., Bethesda, Md. 20034
NELSON, Paul E., Jr., 4436 N. Vacation Lane, Arlington, Va. 22207
NELSON, Phillip J., 595 West End Ave., New York 25, N.Y.
NELSON, Ralph L., 76 Clinch Ave., Garden City, N.Y. 11530
NELSON, Richard Robinson, RAND Corp., 1700 Main St., Santa Monica, Calif.
NELSON, Robert A., 6723 Tennyson Dr., McLean, Va. 22101
NELSON, Robert H., 254 Kenforest Dr., Pittsburgh, Pa. 15216
NELSON, Roger S., 7912 Robinson Rd., Bethesda, Md. 20034
NELSON, Saul, 9119 Sligo Creek Pkwy., Silver Spring, Md.
NELSON, Stewart, Doane Col., Crete, Neb. 68333
NELSON, Wallace Boyd, Arlington State Col., Div. of Bus. Admin., Arlington, Tex.
NELSON, Wallace L., Jr., Univ. of Wyoming, Col. of Com. and Ind., Dept. of Econ., Laramie, Wyo. 82071
NELSON, Wallace M., 724 Via Bella, Winter Park, Fla. 32789
NELSON, William G., 90 Renwick Dr., Youngstown 14, Ohio
NELSON, William G., IV, 1000 Ross Rd., Creve Coeur, Mo. 63141
NEMETHY, Anthony, Newton Col. of the Sacred Heart, Newton, Mass. 01355
NEMETZ, Sidney, Shark River Rd., Box 474, R.D.2, Neptune, N.J. 07753
NEMMERS, Erwin E., 339 E. Chicago Ave., Chicago, Illinois
NEMSHICK, Michael R., Hazle Brook R.D. #1, Hazleton, Pa. 18201
NERLOVE, Marc L., Cowles Found., Box 2125 Yale Sta., New Haven, Conn. 06520
NERLOVE, Samuel H., Univ. of California, Grad. Sch. of Bus., Los Angeles, Calif. 90024
NESS, Norman T., Anderson, Clayton and Co., P.O. Box 2538, Houston 1, Tex.
NESTEL, Gil, Ohio State Univ., Col. of Com. and Admin., 1775 S. College Rd., Columbus, Ohio 43210
NETREBA, Sydney S., 13 Prospect Ave., Montclair, N.J. 07042
NETSCHERT, Bruce C., 1913 Martha's Rd., Alexandria, Va. 22307
NETTER, Klaus, UNCTAD, Palais des Nations, Geneva, Switzerland
NETTO, Antonio Delfim, Rua Dr. Vila Nova, 268—20, Sao Paulo, Brazil
NETZER, Dick, New York Univ., Grad. Sch. of Pub. Admin., New York 3, N.Y.
NEUFELD, Charles M., 518 N.W. 15th St., Gainesville, Fla.
NEUFELD, E. P., Univ. of Toronto, Dept. of Polit. Econ., 100 St. George St., Toronto, Ont., Canada
NEUMAN, Duane F., North Carolina State of Univ. of North Carolina, 4 Patterson Hall, Raleigh, N.C. 27607
NEUMANN, Alfred K., 177 Davis Ave., Brookline, Mass. 02146
NEUMANN, Carl A., 9030 W. Meinecke Ave., Wauwatosa, Wis. 53226
NEUMARK, S. Daniel, c/o COEDPAR, Rua 15 de Novembro, 270—6 andar, Curitiba, Pr., Brazil
NEUNER, Edward, San Diego State Col., Dept. of Econ., San Diego 15, Calif.
NEUTZE, G. M., Australian Nat. Univ., Sch. of General Studies, Dept. of Econ., P.O. Box 4, Canberra, A.C.T., Australia
NEVEU, Raymond P., Indiana State Univ., Sch. of Bus., Terre Haute, Ind. 47809
NEVILE, John Warwick, Univ. of New England, Dept. of Econ., Armidale, N.S.W., Australia
NEVIN, Jerome R., 931 Covington Dr., Detroit 3, Mich.
NEWARK, Christine, Ohio State Univ., Sch. of Home Econ., 1787 Neil Ave., Columbus, Ohio 43210
NEWBURG, (Mrs.) Cecile J., 219 8th St., S.E., Washington 3, D.C.
NEWCOMB, Richard T., Pennsylvania State Univ., 301 Engineering Unit D., University Park, Pa. 16802
NEWELL, Chesley S., 7 Youngs Rd., Westwood, Mass. 02090
NEWELL, David R., 421 Conner Ave., Fort Worth, Tex. 76105
NEWELL, Howard F., 311 E. 14th St., Emporia, Kans. 66801
NEWELL, William T., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
NEWHOUSE, Joseph P., 247 Kent St., Brookline, Mass. 02146
NEWKIRK, Wayne E., Memphis State Univ., Sch. of Bus., Memphis, Tenn. 38111
NEWMAN, Barry S., 52-53 Oceania St., Bayside 64, N.Y.
NEWMAN, Charles D., 10207 Scofield Lane, Houston 35, Tex.
NEWMAN, (Mrs.) Dorothy K., 3508 Woodbine St., Chevy Chase, Md. 21620
NEWMAN, Herbert E., P.O. Box 479, Carlisle, Pa. 17013
NEWMAN, Kenneth R., Albion Col., Albion, Mich. 49224
NEWMAN, Lloyd M., 2232 Green St., San Francisco, Calif. 94123
NEWMAN, Monroe, 1629 Columbia Rd., N.W., Washington, D.C. 20009
NEWMAN, Peter K., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore, Md. 21218
NEWMAN, Philip C., 250 Woodside Ave., Ridgewood, N.J.
NEWMAN, Ted E., 402 Stadium Park, Iowa City, Iowa
NEWMAN, William H., Columbia Univ., Grad. Sch. of Bus., 725 Uris Hall, New York, N.Y. 10027
NEWNAM, Albert H., CMR, Box 1543, Brookley AFB, Ala. 36615
NEWSOM, Howard F., American Embassy, Fleet Post Office, San Francisco, Calif. 96690
NEWTON, Bernard, Long Island Univ., 385 Flatbush Ave. Ext., Brooklyn 1, N.Y.
NEWTON, John P., Jamestown Col., Jamestown, N.D.
NEWTON, Neil W., 7508 Allan Ave., Falls Church, Va. 22042
NICANDRO, Honesto G., c/o Central Bank of the Philippines, Manila, Philippines
NICHOLAS, Constantine J., 1730 Dublin Dr., Silver Spring, Md.
NICHOLAUS, H. A., 460 East Briar Lane, Lake Forest, Ill. 60045
NICHOLLS, John A. F., Indiana Univ., Grad. Sch. of Bus., Bloomington, Ind. 47405
NICHOLS, Alan T., 18 Walker St., Vermillion, S.D. 57069
NICHOLS, Dorothy M., Fed. Res. Bank of Chicago, 230 S. La Salle St., Chicago, Ill. 60690
NICHOLS, George C., 1100 Woodcliff Dr., Alexandria, Va. 22308
NICHOLS, John D., 2147 Heatherwood, Toledo, Ohio 43614
NICHOLS, John R., 719 Sunny Hills Dr., Fullerton, Calif. 92632
NICHOLS, Paul R., 153 Grant St., Needham 92, Mass.
NICHOLS, Russell T., 20431 Little Rock Way, Malibu, Calif. 90265
NICHOLS, T. Everett, Jr., 3141 Ashel St., Raleigh, N.C.
NICHOLSON, Howard White, 522 21st St., N.W., Apt. 408, Washington, D.C. 20006
NICHOLSON, Walter E., III, 308 Commonwealth Ave., Apt. K., Boston, Mass. 02115
NICKLAUS, George E., 631 S. Ridge Ave., Arlington Heights, Ill.

NICOL, (Mrs.) Helen O., 4501 Conn. Ave., N.W., Washington 8, D.C.
NICOLS, Alfred, Univ. of California, Dept. of Bus. Admin., Los Angeles 24, Calif.
NICOLSON, Miklos S., University Sta., P.O. Box 2271, Enid, Okla.
NICOSIA, Francesco M., Univ. of California, Sch. of Bus. Admin., 113 South Hall, Berkeley 4, Calif.
NIEB, Joseph E., Gannon Col., Dept. of Mgt., Erie, Pa.
NIEBUHR, Richard J., World Bank, 1818 H St., N.W., Washington, D.C.
NIEBYL, (Mrs.) Elizabeth C., 300 Hamilton Rd., Glassboro, N.J. 08028
NIEBYL, Karl, 300 Hamilton Rd., Glassboro, N.J. 08028
NIEDERCORN, John Herman, 21503 Marchena St., Woodland Hills, Calif.
NIEH, Candia K., 422 Liberty Rd., Englewood, N.J.
NIELSEN, A. Evald, Belhaven Col., Dept. of Econ., Jackson, Miss. 39302
NIELSEN, Donald A., 107 Smith Lane, Apt. 1-A, Syracuse, N.Y. 13210
NIELSEN, N. H., Massey-Ferguson Ind., Ltd., 915 King St., W., Toronto 3, Ont., Canada
NIELSEN, Norman W., 4316 Chippewa, Apt. B., St. Louis 16, Mo.
NIELSEN, Robert S., Eastern Airlines, Market Res., Rm. 1203, 10 Rockefeller Plaza, New York, N.Y.
NIEMELA, Roy W., Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla.
NIEMI, Esther P., 2798 Grethen Dr., N.E., Warren, Ohio
NIERENBERG, Harold, Long Island Univ., Zeckendorf Campus, Brooklyn 1, N.Y.
NIERENBERG, Norman, 23208 Calvert St., Woodland Hills, Calif.
NIGHTINGALE, Edmund A., Univ. of Minnesota, Grad. Sch. of Bus. Admin., Minneapolis, Minn. 55455
NIGHTINGALE, Ray W., R.F.D. #2, Houlton, Me. 04730
NIGRO, Peter D., 55 Seth Ct., Staten Island 1, N.Y.
NIJHOWNE, Tilak, 16 Cooper #5, Ottawa 4, Ont., Canada
NILAND, Powell, Washington Univ., Grad. Sch. of Bus. Admin., St. Louis 30, Mo.
NILSSON, Arthur E., Cornell Univ., Sch. of Bus. and Pub. Admin., Ithaca, N.Y. 14850
NIMER, Daniel A., 4416 Church St., Skokie, Ill. 60076
NING, Werner Y. F., 1369 E. Hyde Park Blvd., Apt. 903, Chicago, Ill. 60615
NISBET, Charles T., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
NISHIYAMA, Paul C., 22 Sagami Heights, Shimotsuruma, Tamato-shi, Kanagawa Ken, Japan
NISKANEN, William A., 7209 Gordons Rd., Falls Church, Va.
NISS, James F., 5617 S. Oak, Hinsdale, Ill.
NISSLEY, Warren W., 145 Phelps Rd., Ridgewood, N.J.
NITSCH, Thomas O., St. Mary's Univ. Dept. of Econ., San Antonio, Tex. 78228
NIVATANONDA, Charungklin, Nat. Statis. Office, Survey Div., Larluang Rd., Bangkok, Thailand
NIXON, (Mrs.) Florence Guild, 171 Hicks St., Brooklyn 1, N.Y.
NIXON, John H., 345 E. 52nd St., New York 22, N.Y.
NIXON, John W., Michigan State Univ., Dept. of Agric. Econ., Agric. Hall, East Lansing, Mich.
NIXON, Russell Arthur, 171 Hicks St., Brooklyn 1, N.Y.
NIXON, Stanley E., 11 Rosemont Ave., Montreal 6, Westmount, Canada
NOAH, Harold, Columbia Univ., Teachers Col., New York 27, N.Y.
NOBEL, (Mrs.) Marion M., 801 Sunrise Ct., Ann Arbor, Mich.
NOBLE, John H., 748 Cherokee Dr., Ft. Collins, Colo.
NOBLE, Robert B., 5313 S. Kimbark Ave., Chicago, Ill. 60615
NOE, Gordon A. Sammy, 425 S. Lee St., Alexandria, Va. 22314
NOEL, Normand R., 26 Tweed St., Pawtucket, R.I.
NOEL, Walter M., Arthur D. Little, Inc., 35 Acorn Park, Cambridge, Mass. 01922
NOETZEL, Arthur J., John Carroll Univ., Cleveland 18, Ohio
NOETZEL, Grover A. J., Univ. of Miami, Dept. of Econ., Coral Gables, Fla. 33124
NOH, Laird, R. 1, Kimberly, Idaho 83341
NOLAN, Thomas J., 1410 Mifflin St., Huntingdon, Pa. 16652
NOLEN, Russell M., Stetson Univ., De Land, Fla.
NOLLEN, Stanley D., Dow Chemical Co., P.O. Box 512, Midland, Mich. 48641
NONOMURA, Kazuo, Hitotsubashi Univ. Campus, Kunitachi 185, Kitatama-gun, Tokyo, Japan
NOONAN, George E., Jr., 1063 Plymouth St., Windsor, Conn. 06095
NOORZOY, Mohammad S., Univ. of Alberta, Fac. of Bus. Admin. and Com., Edmonton, Alta., Canada
NOPAKUN, Somchit, Nat. Econ. Dev. Bd., Bangkok, Thailand
NORBOM, Jon Ola, C/O Gatt, Villa Le Bocage, Geneva, Switzerland
NORBY, John C., Los Angeles State Col., 5151 State College Dr., Los Angeles, Calif. 90032
NORBYE, Ole David, Ford Found., P.O. Box 1081, Nairobi, Kenya
NORCROSS, Harry C., 100 E. York St., Biglerville, Pa. 17307
NORDHAUSER, Fred, Qtrs 4405-C, USAF Academy, Colo. 80840
NORDIN, J. A., Kansas State Univ., Dept. of Econ. and Soc., Waters Hall, Manhattan, Kans.
NORDQUIST, Gerald, 1301 Yewell, Iowa City, Iowa
NORDSTRAND, Raymond W., 518 W. Roscoe St., Chicago, Ill. 60657
NORDSTROM, Carl, 78 Smith Ave., South Nyack, N.Y.
NORDYKE, James, New Mexico State Univ., Econ. Dept., University Park, N.M. 88070
NOREN, James H., 8622 Cromwell Dr., E., Springfield, Va.
NORENSBERG, Lanny, 1900 Albermarle Rd., Brooklyn, N.Y. 11226
NORGAARD, Richard L., B.E.O.B. 104, Univ. of Texas, Austin 12, Tex.
NORGREN, Paul H., Interlaken Rd., Stamford, Conn.
NORMAN, Charles B., 1448 W. Euclid, Stockton, Calif. 95204
NORMAN, Roger, Ohio State Univ., Dept. of Econ., Col. of Com. and Admin., 1775 S. College Rd., Columbus, Ohio 43210
NORRIS, Alfred G., 526 N. Serrano Ave., Los Angeles, Calif. 90004
NORRIS, Charles H., Jr., c/o Mr. H. D. Thayer, Box 686, Chatham, Mass. 02633
NORRIS, David Y., Box 157, Stewartstown, Pa. 17363
NORRIS, Nilan, Hunter Col., Dept. of Econ., 695 Park Ave., New York, N.Y. 10021
NORSWORTHY, J. Randolph, 5514 S. Blackstone Ave., Chicago, Ill. 60637
NORTH, Douglass C., Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
NORTHRUP, Herbert, 517 Thornbury Rd., Haverford, Pa. 19041
NORTON, Frank E., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles, Calif. 90024
NORTON, George H., Jr., Continental Ill. Nat. Bank, 231 S. LaSalle St., Chicago, Ill. 60690
NORTON, Hugh S., Univ. of Tennessee, Dept. of Transp., Knoxville, Tenn.
NORTON, John H., 1211 Lombard St., Philadelphia, Pa. 19147
NORTON, Roger D., USOM to Korea, APO San Francisco 96301
NORTON, Virgil J., Bur. of Commercial Fisheries, Dept. of Interior, 1815 N. Fort Myer Dr., Rm. 600, Roselyn, Va. 22967
NORWOOD, Bernard, Trade Staff Com., Exec. Office of the Pres., Washington, D.C.
NOTHMANN, Rudolph, P.O. Box 32, Pacific Palisades, Calif. 90272
NOURI, Clement J., 5336 N. 91st St., Milwaukee, Wis. 53225

NOURSE, Edwin G., 3802 Jocelyn St., N.W., Washington, D.C. 20015
NOURSE, Hugh Oliver, Univ. of Illinois, Col. of Com. and Bus. Admin., Dept. of Econ., Urbana, Ill.
NOURSI, David B., Jordan Development Bd., P.O. Box 555, Amman, Jordan
NOVAK, George J., 121 Fourth St., S.E., Washington, D.C. 20003
NOVAK, Wanda, 37-27 94th St., Jackson Heights 72, N.Y.
NOVEY, Donald F., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich.
NOVICK, David, 1700 Main St., Santa Monica, Calif.
NOVICK, Harry, 1220 Blair Mill Rd., Silver Spring, Md. 20910
NOWLAN, David M., Univ. of Toronto, Dept. of Polit. Econ., 100 St. George St., Toronto 5, Ont., Canada
NOWOTNY, F. A., St. Francis Col., Biddeford, Me. 04005
NUBEL, Vincent A., 29372 Hoover Rd., Warren, Mich. 48093
NUGENT, Jeffrey B., 825 Idaho Ave., Santa Monica, Calif.
NUNAG, Nicholas, 71-01 35th Ave., Jackson Heights, N.Y.
NURNBERG, Hugo, 141-31 73rd Ter., Flushing, N.Y. 11367
NURNBERG, Max, 249 N. Dithridge St., Pittsburgh, Pa. 15213
NUSBAUMER, Jacques A. E., 166 E. 61st St., New York, N.Y. 10021
NUSSBAUM, Jacob, 254 W. 73rd St., Apt. No. 1, New York, N.Y. 10023
NUTTER, G. Warren, Univ. of Virginia, Dept. of Econ., Charlottesville, Va.
NYHAN, David J., 1592 Culver Rd., Rochester 9, N.Y.
NYMAN, Bertil C., 309 W. Ells St., Champaign, Ill. 61822
NYSTROM, Paul E., 4400 Holly Hill Rd., Hyattsville, Md.

OAKES, Eugene E., 4801 Connecticut Ave., N.W., Washington 8, D.C.
OAKES, Ralph H., 5550 S. Dorchester Ave., Chicago, Ill. 60637
OAKLAND, William H., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore, Md.
OATES, Sister M. Norbert, 420 Temple St., New Haven, Conn. 06511
OATES, Wallace E., Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
O'BANNON, N. D., Pacific Univ., Dept. of Econ., Forest Grove, Ore.
O'BEIRNE, Bert E., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
OBELSKY, Alvan J., Univ. of Michigan, Dearborn Campus, Dept. of Econ., Dearborn, Mich.
OBERENDER, Peter O., Singerstr. 16, 85 Nurnberg 9, Bavaria, Germany
OBERG, Winston, Michigan State Univ., Col. of Bus. Serv., Dept. of Mgt., East Lansing, Mich.
OBERSTEINER, Erich, 7 Mitchell Ave., New Brunswick, N.J. 08901
O'BOYLE, Edward J., 4992 Parker, St. Louis, Mo. 63139
O'BRIEN, Francis T., 21 Jupiter St., Weymouth, Mass. 02673
O'BRIEN, Frederick S., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
O'BRIEN, James M., Sherman Hall, Rm. 253, 909 S. 5th St., Champaign, Ill. 61822
O'BRIEN, John, c/o Sir George Williams Univ., 1435 Drummond St., Montreal, Que., Canada
O'BRIEN, John C., Fresno State Col., Dept. of Econ., Fresno, Calif. 93726
O'BRIEN, Peter A., Jr., 16 Richard St., Dover, N.J. 07801
OBST, Norman P., 513-1 S. Campus Cts., West Lafayette, Ind.
OCADIZ, Julio, Monte Caucaso 977, Mexico 10, D.F.
OCAMPO, Jorge R., Calle 15 No. 8-32, Bogota, Colombia
OCHS, Jack, Indiana Univ., Dept. of Econ., Bloomington, Ind.
OCHSENIUS, Victor, Boccaccio #7348, La Reina, Santiago, Chile
OCKERMAN, Everett W., Niagara Univ., Col. of Bus. Admin., Niagara University, N.Y. 14109
O'CONNELL, Donald W., 3401 Rosemary Lane, Hyattsville, Md. 20782
O'CONNELL, James J., 136-89 Roosevelt Ave., Flushing, N.Y. 11354
O'CONNER, Dennis J., 928 E. Campus Dr., Tempe, Ariz. 85281
O'CONNOR, Edward F., 6231 Broadway, Bronx, N.Y. 10471
O'CONNOR, Hernando A., 14 Viola Dr., Glen Cove, N.Y. 11542
O'CONNOR, John J., 640 Esplanada, Pelham Manor, N.Y. 10803
O'CONNOR, John T., 321 Stahl Dr., Huntingdon Valley, Pa. 19006
O'CONNOR, Leonard A., 8 Centerwood Dr., Cromwell, Conn. 06416
O'CONNOR, Michael J. L., Central Michigan Univ., Mt. Pleasant, Mich. 48858
O'CONNOR, Mildred C., 687 Washington St., Brighton 35, Mass. 02324
O'CONNOR, Paul, 3690 Townley Rd., Shaker Heights, Ohio 43947
O'CONNOR, William C., 2019 Franklin Ave., S.E., Minneapolis, Minn. 55414
ODAKA, Konosuke, 2-5 Honkomagome, 2-chome, Bunkyoku, Tokyo, Japan
ODATE, Gyoju, Akita Keizai Daigaku, Akita Junior Col., Barajima, Akita-Shi, Japan
ODAWARA, Ken-ichi V., 1-3-4 Fujigaya, Kugenuma, Fujisawa-shi, Japan
ODIORNE, George, Univ. of Michigan, Bur. of Ind. Rela., Ann Arbor, Mich.
ODLUM, (Mrs.) Harriet D., 1018-4 Beechnut St., APO Seattle, Wash. 98731
O'DONNELL, Sister Angela St. G., St. Bernard Convent, 1497 Washington St., West Newton, Mass. 02165
O'DONNELL, Charles F., 4380 Byron Ave., Bronx 66, N.Y.
O'DONNELL, John J., Mankato State Col., Dept. of Econ., Mankato, Minn.
O'DONNELL, John L., Michigan State Univ., Grad. Sch. of Bus. Admin., Room 423—Eppley Center, East Lansing, Mich.
O'DONNELL, Liguori A., 5803 Lignum St., Springfield, Va. 22150
OEHLER, Hartmut, Hermannstrasse 109, 3550 Marburg (Lahn), Germany
OEI, Hong Lan, United Nations ECAFE, Sala Santitham, Bangkok, Thailand
OEI, Rudy T. S., 9 Studio Hill Rd., Briarcliff Manor, N.Y. 10510
OETTINGER, Martin P., Univ. of Calif., Dept. of Econ., Davis, Calif. 95616
OFFEI, Comfort, Min. of Agric., P.O. Box M37, Accra, Ghana
OFFICER, Lawrence H., Harvard Univ., Dept. of Econ., 1746 Cambridge St., Cambridge, Mass. 02138
OGDEN, Delbert C., Colorado State Univ., Dept. of Econ., Fort Collins, Colo. 80521
OGG, Wallace E., Iowa State Col. of Agr., Ames, Iowa
O'GRADY, Lawrence J., P.O. Box 478, Postal Terminal A., Ottawa 2, Ont., Canada
OGRAM, Ernest W., Jr., Georgia State Col., 39 Gilmer St., S.E., Atlanta, Ga. 30303
OH, Kyong Hee, Bank of Korea, Res. Dept., Seoul, Korea
O'HARE, Thomas E., M. W. Kellogg Co., 711 Third Ave., New York, N.Y. 10017
OHLIN, Bertil, Stockholm Commercial Col., Stockholm, Sweden
OHLSON, Morton K., 8179 W. Louisiana Ave., Denver, Colo. 80226
OHM, Young Suck, 1661 Colby Ave., Apt. 3, Los Angeles 25, Calif.
OHRENSTEIN, Roman A., 28-74 208th St., Bayside, L.I., N.Y. 11361
OHRN, Sandra H., 85 Whittier Rd., Needham 92, Mass.
OI, Walter, Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
OISHI, Yasuhiko, 255 Kami-Ochiai 1-chome, Shinjuku-ku, Tokyo, Japan
OJOW, Francis I., 331 Packard St., Ann Arbor, Mich.

OKE, Donald W., Canada Dept. of Fin., Ottawa 4, Ont., Canada
OKIGBO, Pius N. C., Cabinet Off., Lagos, Nigeria
OKNER, Ben, Coun. of Econ. Advisers, Exec. Off. of the President, Washington, D.C. 20506
OKSANEN, Ernest H., McMaster Univ., Dept. of Polit. Econ., Hamilton, Ont., Canada
OKUDA, Kenji, Ford Found., P.O. Box 81, Katmandu, Nepal
OKUN, Arthur M., Coun. of Econ. Advisers, Exec. Off. Bldg., Washington, D.C.
OKUN, Bernard, Queens Col., Dept. of Econ., Flushing 67, N.Y.
OKUN, Julius L., 1703 East-West Hwy., Silver Spring, Md. 20910
OLCAY, O. John, 3 Ashburn Pl., Radburn, N.J.
OLDMAN, Oliver, Harvard Law Sch., Langdell Hall, Cambridge, Mass. 01922
O'LEARY, Edmund B., Univ. of Dayton, Dayton, Ohio
O'LEARY, James J., 277 Park Ave., New York, N.Y. 10017
O'LEARY, John F., Jr., 224 E. Cuthbert Blvd., Westmont, N.J. 07480
O'LEARY, Paul M., Cornell Univ., Goldwin Smith Hall, Ithaca, N.Y.
OLEJARCZYK, Kazimierz, 7313 Emanon, Dearborn, Mich.
OLESON, John R., 9304 Milroy Pl., Bethesda, Md. 20014
OLIKER, Louis R., Indiana Univ., Sch. of Bus., Bloomington, Ind.
OLIVEIRA, Kenneth G., Columbia Univ., 1321 John Jay, New York, N.Y. 10027
OLIVEN, (Mrs.) Carol C., 117 Quonset Park, Iowa City, Iowa
OLIVER, David R., 3303 King Arthur Rd., Annandale, Va. 22003
OLIVER, Henry M., Jr., Indiana Univ., Econ. Dept., Bloomington, Ind.
OLIVER, John H., 7-V Research Rd., Greenbelt, Md. 20770
OLIVER, Robert W., Calif. Inst. of Tech., 1201 E. California Blvd., Pasadena, Calif.
OLIVER, Robert W., 67 Four Leaf Rd., Levittown, Pa.
OLIVERIO, Mary E., Columbia Univ., Teachers Col., New York, N.Y. 10027
OLKEN, Samuel C., 217 Broadway, Rm. 412, New York, N.Y. 10007
OLLEY, Robert E., Univ. of Saskatchewan, Dept. of Econ. and Polit. Sci., Saskatoon, Sask., Canada
OLSEN, Arden B., Univ. of Denver, Col. of Bus. Admin., 1445 Cleveland Pl., Denver 2, Colo.
OLSEN, Arthur R., Western Illinois Univ., Macomb, Ill. 61455
OLSEN, Bernard M., North Carolina State Col., Dept. of Econ., Raleigh, N.C.
OLSEN, E. O., Rice Univ., Dept. of Econ. and Bus. Admin., Houston, Tex. 77001
OLSEN, Herluf V., 4 Dana Rd., Hanover, N.H. 03755
OLSEN, Paul C., Brooklyn Col. of Pharmacy, 600 Lafayette Ave., Brooklyn, N.Y. 11216
OLSEN, Richard J., Univ. of Massachusetts, Econ. Dept., Amherst, Mass. 01003
OLSEN, Robert M., 1207 Michigan Ave. Apt. 2, Evanston, Ill. 60202
OLSEN, Ronald R., Univ. of Kansas, Dept. of Econ., Lawrence, Kans.
OLSON, Alden C., Michigan State Univ., 328 Eppley Center, East Lansing, Mich.
OLSON, Bruce H., Univ. of Washington, MacKenzie Hall, Seattle, Wash. 98105
OLSON, Charles E., 210 Commerce, Univ. of Wisconsin, Madison, Wis. 53706
OLSON, David A., 262 Bridge St., Northampton, Mass 01060
OLSON, Edwin G., 4743 48th Ave. N.E., Seattle, Wash. 98105
OLSON, Ernest C., 322 Highland, San Rafael, Calif. 94901
OLSON, Eric J., Duke Univ., Dept. of Econ., Durham, N.C. 27706
OLSON, Fred L., 13009 Pacific Ave., Rockville, Md. 20853
OLSON, James R. G., Millikin Univ., Decatur, Ill. 62522
OLSON, Josephine E., 521 Munro Ave., Mamaroneck, N.Y.
OLSON, Mancur, Jr., Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
OLSON, Ordell P., Monmouth Col., Econ. Dept., Monmouth, Ill. 61462
OLSON, Oscar A., 1315 Lyndon Rd., McPherson, Kans 67460
OLSON, Paul R., Univ. of Iowa, Iowa City, Iowa
OLSON, Richard E., Wichita State Univ., Dept. of Econ., Wichita, Kans. 67208
OLVEY, Lee D., Military Educa. Found., Box 192, Highland Falls, N.Y. 12528
ONAKOYA, Benjamin, Ministry of Econ. Plan., Ibadan, Nigeria
ONANIAN, Edward D., 11502 Bucknell Dr., Apt. 204, Silver Spring, Md.
ONDRECHEN, John P., 2984 Avon Rd., Bethlehem, Pa.
O'NEAL, Arthur J., Jr., N. J. Dept. of Labor and Ind., Rm. 1211, John Fitch Plaza, Trenton, N.J. 08625
O'NEIL, John T., 2637 Orrington Ave., Evanston, Ill.
O'NEILL, Harold J., 2113 Juana Vista, Las Vegas, Nev. 89102
O'NEILL, John, York Univ., Dept. of Soc., Toronto 12, Ont., Canada
O'NEILL, John J., Jr., 1006 Upshur St., N.E., Washington, D.C. 20017
O'NEILL, Paul H., 5522 Kings Park Dr., Springfield, Va. 22151
ONIKI, Hajime, Tohoku Univ., Dept. of Econ., Sendai, Japan
ONITSUKA, Yusuke, Rm. 755 Int. House, 1414 E. 59th St., Chicago, Ill. 60637
ONORATO, Nicholas L., Worcester Poly. Inst., Worcester 2, Mass
O'NUALLAIN, Labhras, Univ. Col., Faculty of Com., Galway, Ireland
OOMS, Van Doorn, c/o Mrs. C. W. Ooms, 609 S. Kensington Ave., La Grange, Ill. 60525
OORT, C. J., Park Arenberg 99, De Bilt, The Netherlands
OPIE, Redvers, 1232 17th St. N.W., Washington 6, D.C.
OPP, Gerald L., 2111 Jackson Bluff Rd., Tallahassee, Fla. 32304
OPPEL, Edwin I., 19 Patton Pl., Upper Montclair, N.J. 07043
OPPENHEIMER, Enid B., 6735 Ridge Blvd., Brooklyn, N.Y. 11220
OPPERMAN, Edward B., Quar. 6408-A, U.S. Air Force Academy, Colo. 80840
OPPITZ, Robert J., Western Kentucky Univ., Col. of Com., Bowling Green, Ky. 42101
ORCUTT, Guy H., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
ORDEN, Alex, 5715 S. Kenwood Ave., Chicago, Ill. 60637
ORDEN, (Mrs.) Susan R., 5715 Kenwood Ave., Chicago, Ill. 60637
ORENBUCH, Don, 68-60 108th St., Forest Hills, N.Y. 11375
ORESCANIN, Danilo, c/o Ford Found., Box 98, Ramna, Dacca, Pakistan
OREWA, George C., Premiers Off., Benin City, Nigeria
ORKAND, Donald S., 3211 University Blvd. W. Apt. 22, Kensington, Md. 20795
ORMAECHEA, Enrique, Casilla 414, La Paz, Bolivia
ORMAN, Larry, 1340 Plain Ave., #2, Las Cruces, N.M. 88001
ORNATI, Oscar, 1255 Rebecca Dr., Alexandria, Va. 22307
OROSS, John J., City Hall, Orlando, Fla. 32801
O'ROURKE, Fidelis, St. Bonaventure Univ., St. Bonaventure, N.Y. 14778
O'ROURKE, George E., 1 Wilson Rd., Concord, Mass. 01742
ORR, Andrew Jr., 434 Aldine St., Chicago, Ill. 60657
ORR, Daniel, Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
ORR, Dorothy M., 4716 Butterworth Pl., N.W., Washington, D.C. 20016
ORR, John A., 1177 S.W. 4th St., Boca Raton, Fla. 33432

ORR, Lloyd D., Indiana Univ., Econ. Dept., Bloomington, Ind.
ORSAGH, Thomas J., Univ. of North Carolina, Carroll Hall, Chapel Hill, N.C. 27514
ORSHANSKY, Mollie, Div. of Res. and Stat., Social Security Admin., Washington 25, D.C.
ORTH, Andrew P., c/o H.E. Bretz, 8 Stoner Ave., Poland, Ohio 44514
ORTLIEB, David W., 2909 Washington Ave., Evansville, Ind. 47714
ORTNER, Robert C., 374 White Oak Ridge Rd., Short Hills, N.J. 07078
OSBORN, Grant M., 344 Churchill St., Northfield, Ill. 60093
OSBORN, Marjorie M., The Mead Corp., Talbott Tower, Dayton, Ohio 45402
OSBORN, Richards C., Univ. of Illinois, 198 Commerce West, Urbana, Ill. 61803
OSBORNE, Dale K., c/o Fed. Res. Bldg., 21st and Constitution Ave., Washington 25, D.C.
OSBORNE, William A., Winston-Salem State Col., Winston-Salem, N.C. 27102
OSE, Odale J., 9639 Granada, Overland Park, Kans. 66207
OSER, Jacob, Utica Col., Utica, N.Y.
OSGOOD, Frank W., Robert Gladstone and Assoc., 1750 Pennsylvania Ave., N.W., Washington, D.C. 20006
OSGOOD, Theodore K., 9019 Jones Mill Rd., Chevy Chase, Md. 20015
O'SHEA, Thomas W., 42 Ft. Charles Pl., New York, N.Y. 10463
OSHIMA, Harry T., Univ. of Hawaii, Dept. of Econ., Honolulu 14, Hawaii
OSIAS, Max S., 9202 Thoreau Dr., Bethesda, Md. 21811
OSIFO, David E., Manchester Univ., Fac. of Econ. and Soc. Studies, Dover St., Manchester, England
OSMAN, Jack W., 335-B Crowells Rd., Highland Park, N.J. 08904
OSTASIEWSKI, Donald A., 3300 Jefferson Ave., Cincinnati, Ohio 45220
OSTER, Clinton V., 960 Varsity Ave., Columbus 21, Ohio
OSTER, Robert J., 3000 Montebello Ct., Concord, Calif. 94520
OSTERGREN, Clarence N., Apt. 1A Drake, Garth Woods, Scarsdale, N.Y. 10583
OSTERHOUDT, Frank H., Univ. of Wisconsin, Land Tenure Center, Madison, Wis. 56706
OSTHEIMER, Richard H., 56 Old Hwy., Wilton, Conn. 06897
OSTMAN, Henry E., 417 Indiana Ave., Lindenhurst, N.Y. 11757
OSTRANDER, F. Taylor, Jr., 72 Leroy Rd., Chappaqua, N.Y.
OSTROM, (Mrs.) Elinor, Indiana Univ., Dept. of Gov., Bloomington, Ind. 47405
OSTROM, Vincent, Indiana Univ., Dept. of Gov., Bloomington, Ind. 47405
OSTRY, Sylvia, Five Oaks, Aylmer Rd., R.R.1, Hull, Que., Canada
O'SULLIVAN, James S., 31 Codman Hill Ave., Dorchester, Mass. 02124
OSWALD, Mary K., 1447 Jones St., #4, San Francisco, Calif.
OSWALD, Rudolph, 815 16th St., N.W., Washington, D.C.
OTA, Tetsuo, 3625 N. Pine Grove Ave., Chicago, Ill. 60613
OTERO, Rafael, 225 Campus Ave., Ames, Iowa
OTIS, Delos S., c/o Mrs. M. H. Bibbins, 126 Hancock St., Cambridge, Mass. 02139
O'TOOLE, Dennis P., 1776 Castle Hill Ave., New York, N.Y. 10462
O'TOOLE, John R., 405 E. 56th St., New York, N.Y. 19122
OTT, David J., Southern Methodist Univ., Dept. of Econ., Dallas, Tex.
OTT, Roy, Jr., Univ. of Alabama, P.O. Box 2343, University, Ala. 35486
OTTE, Herman F., Columbia Univ., Grad. Sch. of Bus., 714 Uris Hall, New York, N.Y. 10027
OTTESON, Schuyler F., Indiana Univ., Sch. of Bus., Bloomington, Ind.
OTTO, Erich A., Rutgers Univ., Grad. Sch. of Bus. Admin., Newark, N.J. 07102
OTTO, Ingolf, Box 473, Southern Sta., Hattiesburg, Miss. 39401
OTTOSON, Howard W., 5811 Margo Dr., Lincoln, Neb.
OUCHI, Albert Y., 3607 N.E. 78th St., Portland 13, Ore.
OUELLETTE, Conrad M., Dalhousie Univ., Dept. of Econ. and Soc., Halifax, N.S., Canada
OUNJIAN, Daniel, 12 Sunset Rd., Somerville, Mass. 02565
OVEDOVITZ, Albert, 58-50 251st St., Little Neck, N.Y. 11362
OVENS, E. Albert, 50 Broadway, New York 4, N.Y.
OVERLY, Don H., Wayne State Univ., C.A.S.T., Detroit, Mich. 48202
OWEIS, Jiryis S., Univ. of Utah, Col. of Bus., Dept. of Econ., Salt Lake City, Utah 84112
OWEISS, Ibrahim, 512 Delaware St. S.E., Apt. 217, Minneapolis, Minn. 55414
OWEN, Bruce M., 426 Packard St., Ann Arbor, Mich. 48107
OWEN, John M., Jr., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore 18, Md.
OWEN, John P., Univ. of Houston, Sch. of Bus. Admin., 3801 Cullen Blvd., Houston 4, Tex.
OWEN, Lyle, Univ. of Tulsa, Tulsa, Okla. 74104
OWEN, Thomas L., River East Apt., Apt. 7-C, 251 E. 32nd St., New York, N.Y. 10016
OWEN, (Mrs.) Virginia L., 609 Cullom, Normal, Ill. 61761
OWEN, William C., 5259 N. Capitol Ave., Indianapolis, Ind. 42608
OWEN, Wyn F., Univ. of Colorado, Econ. Dept., Boulder, Colo.
OWENS, Gerry, Ohio State Univ., Dept. of Agric. Econ., 2120 Fyffe Rd., Columbus, Ohio 43210
OXENFELDT, Alfred R., 187 Allen Rd., Rockville Centre, N.Y.
OXENFELDT, (Mrs.) Gertrude E., 187 Allen Rd., Rockville Centre, N.Y.
OXLEY, Frances E., 4800 Chicago Beach Dr., Apt. 512 N., Chicago, Ill. 60615
OYAL, Attila, Ticaret Bakanligi, Disticaret Dairesi, Ankara, Turkey
OZAKI, Robert S., 633 San Luis Rd., Berkeley, Calif. 94707
OZANNE, Larry, 415 Fernando Ave., Palo Alto, Calif. 94306
OZANNE, Robert W., Univ. of Wisconsin, Dept. of Econ., Madison 6, Wis.
OZAWA, Terutomo, 181 Claremont Ave., Apt. 4?, New York, N.Y. 10027
OZOL, Cengiz, Vanderbilt Univ., P.O. Box 1429, Sta. B, Nashville, Tenn. 37203
OZOL, Ertugrul I., 184 Bld. Malesherbes, Paris 17, France
OZOLINS, Eizens-Adrians, P.O. Box 537, Lock Haven, Pa. 17745
OZSOY, Numan, Maliye Mufettisi, Ankara, Turkey

PAANANEN, Orman, 1369 N. 4th, Space 13, Laramie, Wyo.
PAARLBERG, Don, 1214 Hayes St., West Lafayette, Ind. 47178
PAAUW, Douglas S., USAID, APO San Francisco 96528
PABST, William R., Jr., 3420 Quebec St. N.W., Washington 16, D.C.
PACE, Bill J., 1843 Jefferson Dr., Concord, Calif. 94521
PACK, Howard, Box 1987, Yale Sta., New Haven, Conn. 06520
PACK, (Mrs.) Janet, Box 1987, Yale Sta., New Haven, Conn. 06520
PACK, Pauline H., 4421 Stonehedge Way, Sacramento, Calif. 95823
PACKER, Stephen B., Continental Can Co., Inc., 633 3rd Ave., New York 17, N.Y.
PADEN, David L., Midwest Res. Inst., 425 Volker Blvd., Kansas City, Mo. 64110
PADEN, Donald W., Univ. of Illinois, Dept. of Econ., Urbana, Ill. 61801
PADGET, Gerald E., 6402 Hughes Dr., Huntington Beach, Calif.
PAGANI, John, Jr., Univ. of Santa Clara, Col. of Bus. Admin., Santa Clara, Calif. 95053
PAGE, Alfred N., Univ. of Washington, Sch. of Bus. Admin., Dept. of Fin. and Statis., Seattle, Wash. 98105
PAGE, Earl, Jr., 6529 W. Abbottswood Dr., Palos Verdes, Calif. 90274

PAGE, Lee R., 1529 Menown Ave., Independence, Mo.
PAGE, Walter P., Univ. of Kansas, Dept. of Econ., Lawrence, Kans.
PAGENSTECHER, Ulrich, Gleuelerstr. 265, Cologne-Lindenthal, Germany
PAGLIN, Morton, 2164 N.W. Aspen Ave., Portland, Ore. 97210
PAI, Padmanabha B., 548½ Gayley Ave., Los Angeles 24, Calif.
PAINE, Neil R., Univ. of Houston, Dept. of Gen. Bus. Admin., Cullen Blvd., Houston, Tex. 77004
PAK, Chee Hong, 35 Yongee Dong, Chong-Ro Ku, Seoul, Korea
PAK, Tong C., 241 N. Swall Dr., #C, Beverly Hills, Calif. 90210
PAKONEN, R. Rodney, D-5 Kamiak Apt., Pullman, Wash. 99163
PAL, I. D., Univ. of Victoria, Dept. of Econ., Victoria, B.C., Canada
PALDA, Kristian S., Claremont Grad. Sch., Harper 107, Claremont, Calif. 91715
PALEKAR, Shreekant A., Sir George Williams Col., 1441 Drummond St., Montreal 25, P.Q., Canada
PALINAY, Frank, Wisconsin State Col., Whitewater, Wis. 53190
PALM, Thomas, 811 E. Ann, Ann Arbor, Mich. 48104
PALMER, Boyd Z., Atlas Chemical Ind., Inc., Wilmington 99, Del.
PALMER, Donald K., 5404 Greystone St., Chevy Chase, Md. 21620
PALMER, Hans C., Pomona Col., Dept. of Econ., Claremont, Calif.
PALMER, Harry M., 6520 Chadwell Rd., S.W., Huntsville, Ala. 35802
PALMER, Ransford W., Central Connecticut State Col., Dept. of Soc. Sci., New Britain, Conn. 06050
PALMER, William B., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
PALOMBA, Neil A., 826 Delaware St., S.E., Minneapolis, Minn. 55414
PALUBINSKAS, Feliksas, California State Col. at Long Beach, Mktg. Dept., Long Beach, Calif. 90804
PALYI, Melchior, 211 W. Wacker Drive, Chicago, Ill. 60606
PAN, Yu-Pu, United Nations, P.O. Box 20, Room 3165 A, New York 17, N.Y.
PANAGIDES, Stahis S., 3425 Oakland Ave., Ames, Iowa 50012
PANAS, Eustace G., 6, Naiadon St., Athens 516, Greece
PANCOAST, Elinor, 6100 Bellinham Ct., Apt. 1022, Baltimore, Md. 21210
PANCOAST, Omar Jr., R.F.D. 2, Blackrock Rd., Germantown, Md. 20767
PANCRATIA, Sister M., Pius XI H.S., Econ. Dept., 135 N. 76th St., Milwaukee, Wis. 53213
PANDAY, Devendra R., Univ. of Pittsburgh, Grad. Sch. of Pub. and Int. Affairs, Pittsburgh, Pa. 15213
PANDIT, Alba M. Z., 7905 Park Overlook Dr., Bethesda, Md. 20034
PANDIT, Ramu, 18, Laxmi Mahal, Bomanji Petit Rd., Bombay 26, India
PANGILINAN, Benjamin A., 207 Columbus Ave., New York, N.Y. 10023
PANT, Pushker N., Maiti Devi, Dilli Bazar, Katmandu, Nepal
PANZONE, Clemente, Montes de Oca 817, Depto. B-P, B 2°Cuerpo, Buenos Aires, Argentina
PAPAGEORGIOU, Demetrius V., 26 I. Drossopoulou Str., Athens, Greece (T.T. 801)
PAPAMARGARIS, Theocharis, 13, Evritanias St., Athens 605, Greece
PAPANDREOU, Andreas G., 58 Souidias St., Athens, Greece
PAPANEK, Gustav F., c/o Dev. Advisory Service, 6 Divinity Ave., Cambridge, Mass. 01922
PAPERA, D. Robert, Univ. of Santa Clara, Sch. of Bus., Santa Clara, Calif. 95053
PAPI, G. Ugo, Il Rettore dell Univ. de Roma, Rome, Italy
PAPIER, William, 1023 S. Remington Rd., Columbus 9, Ohio
PAPKE, James A., Purdue Univ., Dept. of Econ., Lafayette, Ind.
PAPP, Frank R., c/o EUROFINANCE, 9, Avenue Hoche, Paris VIII, France
PAPPAS, Dean C., 5930 Bush Hill Dr., Alexandria, Va.
PAPPAS, Michael, 800 S. Pennsylvania Ave., Lansing, Mich. 48912
PAPPONE, Rinaldo R., 10747 67th St., Edmonton, Alta., Canada
PARAI, Louis, Univ. of Western Ontario, Univ. Col., Dept. of Econ. and Soc., London, Ont., Canada
PARAMESWARAN, Veketachalam, 47-37 45th St., Queens, N.Y. 11377
PARASKEVOPOULOS, George, Philadelphia Col. of Textiles and Sci., Schoolhouse Lane and Henry Ave., Philadelphia, Pa. 19144
PARDEE, Frederick S., 12218 Paisley Lane, Los Angeles, Calif. 90049
PARDRIDGE, William D., P.O. Box 9808, Washington, D.C. 20015
PARIGI, Sam F., 6335 Carnation Dr., Beaumont, Tex. 77706
PARIS, James D., 83 Old Pond Rd., Great Neck, N.Y.
PARIS, Quirino, Univ. of California, 207 Giannini Hall, Berkeley 4, Calif.
PARISI, Natalie, Catholic Univ., 308 Zimmerman Hall, Washington, D.C. 20017
PARK, Chong Kee, 8107 15th Ave., Apt. 104, Hyattsville, Md. 20783
PARK, Colin, 43 Dale Dr., Chatham Township, N.J. 07928
PARK, Harry R., 28 Jean Pl., Syosset, L.I., N.Y. 11791
PARK, Jae Chun, Temple Univ., Econ. Dept., Philadelphia, Pa.
PARK, Rolla E., 217-B King St., Princeton, N.J. 08540
PARK, Sang O., Clemson Univ., Dept. of Ind. Mgmt., Clemson, S.C. 29631
PARK, Sei-Young, 6030 Sherborn Lane, Springfield, Va. 22150
PARK, Seong Y., Case Inst. of Tech., Div. of Org. Sci., University Circle, Cleveland, Ohio 44106
PARK, Soo-Bin, 440 N. Winona St., Apt. 512, Indianapolis, Ind. 46202
PARK, T. Hardie, Cornell Col., Dept. of Econ., Mt. Vernon, Iowa 52314
PARK, Thae S., 1202 Northport Dr., Madison 4, Wis.
PARK, Ung Suh, Box 1356, East-West Center, Honolulu, Hawaii 96822
PARK, William R., 8101 Tomahawk Rd., Prairie Village, Kans. 66208
PARKANY, John, 4718 Slayden Rd., N.E., Tacoma, Wash. 98422
PARKE, Robert N., 1661 Jefferson, Galesburg, Ill. 61401
PARKER, Alfred, 50 Rockefeller Plaza, New York 20, N.Y.
PARKER, Alfred L., 1101 Tomasita N.E., Albuquerque, N.M. 87112
PARKER, David G., 4125 W. 214th St., Fairview Park, Ohio 43736
PARKER, Harrison, 1121 Arlington Blvd. Apt. T-1006, Arlington, Va. 22209
PARKER, (Mrs.) Ida J., 630 North St., White Plains, N.Y. 10605
PARKER, James S., 10512 Cascade Pl., Silver Spring, Md.
PARKER, Jan, Wheaton Col., Box 1014, Norton, Mass. 02766
PARKER, John E., 333 Eighth St., Ann Arbor, Mich.
PARKER, Judson M., Jr., 2425 14th St. N.E., Apt. 362-B, Washington, D.C. 20018
PARKER, Newton B., 4870 Reservoir Rd., N.W., Washington, D.C. 20007
PARKER, Russell C., Fed. Trade Comm., Bur. of Econ., Washington 25, D.C.
PARKER, Sanford S., 115 E. 52nd St., New York, N.Y. 10022
PARKER, Wallace C., 6664 Woodwell St., Pittsburgh, Pa. 15217
PARKER, William M., 3006 N. 9th St., Fresno 3, Calif.
PARKER, William N., Yale Univ., Dept. of Econ., New Haven, Conn.
PARKHURST, Frederick W., Jr., Guilford Col., Box 8207, Greensboro, N.C. 27410

PARKHURST, Kenneth L., 1503 Birch St., Marquette, Mich. 49855
PARKINSON, J. F., Confederation Bldg., Room 217, Wellington St., Ottawa, Ont., Canada
PARKOFF, Stephen B., 1227 Academy Way, Rockville, Md. 20852
PARKS, M., Socony Mobil Oil Co., Inc., 150 E. 42nd St., New York, N.Y. 10017
PARKS, Richard W., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
PARKS, Robert H., Life Ins. Assoc. of Amer., 277 Park Ave., New York, N.Y. 10017
PARMELEE, Rexford C., 693 Sheridan Rd., Wilmette, Ill. 60091
PARNES, Herbert S., Ohio State Univ., Econ. Dept., Columbus 10, Ohio
PARR, Josephus O., III, 3083 W. 139th St., Cleveland, Ohio 44111
PARRA, Alirio A., Quinta No. 4, 'Entremontes' Calle 7 A, los Palos Grandes, Caracas, Venezuela
PARRISH, Evelyn M., 2710 Macomb St., N.W., Washington, D.C. 20008
PARRISH, Gordon O., Bentley Col., 921 Boylston St., Boston, Mass.
PARRISH, John B., 330 Commerce West, Urbana, Ill.
PARRY, David L., 4607 Palm Dr., La Canada, Calif.
PARRY, Robert T., 1330 Mimosa Lane, Silver Spring, Md.
PARSON, Daniel, Northern Illinois Gas Co., P.O. Box 190, Aurora, Ill. 60507
PARSONS, Charles G., Conover Pl., Alexandria, Va. 22308
PARSONS, Edgar A., 1913 Lakeshore Dr., Lake Forest, Chapel Hill, N.C.
PARSONS, Kenneth H., Univ. of Wisconsin, 336-A Agric. Hall, Madison, Wis. 53706
PARSONS, Melvin G., 804 Edgewood, Columbia, Mo. 65201
PARSONS, Robert A., 18 Miller Stile Rd., Quincy, Mass.
PARTI, Michael, 1128 W. Morse Ave., Chicago, Ill. 60626
PARTIN, Charles A., Lamar State Col. of Tech., Sch. of Bus., Beaumont, Tex. 77704
PARVIN, Manoucher, 6141 Broadway, Apt. 4B, Bronx 71, N.Y.
PARZYCH, Kenneth M., 5 B St., Adams, Mass. 01220
PASANEN, Osmo N., 1002 S. 1st Ave., Sioux Falls, S.D. 57102
PASCAL, Anthony H., c/o RAND Corp., 1700 Main St., Santa Monica, Calif.
PASHEK, Robert D., Pennsylvania State Univ., Col. of Bus. Admin., 122 Boucke Bldg., University Park, Pa. 16802
PASHIGIAN, B. Peter, Grad. Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
PASICIEL, Tadeusz, 42 Lynd Ave., Toronto 3, Ont., Canada
PASOUR, Ernest C., Jr., North Carolina State Univ., Dept. of Agric. Econ., Raleigh, N.C.
PASSER, Harold C., 133 Imperial Circle, Rochester 17, N.Y.
PATE, James L., Monmouth Col., Dept. of Econ., Monmouth, Ill. 61462
PATEL, Navnit N., 19 Ellsworth Ave., Cambridge, Mass. 02139
PATEL, Raman C., Univ. of California, Dept. of Econ., Riverside, Calif. 92507
PATERSON, Robert W., Univ. of Missouri, Sch. of Bus. and Pub. Admin., Columbia, Mo.
PATINKIN, Don, Hebrew Univ. of Jerusalem, Jerusalem, Israel
PATINO, Alfonso, c/o Colombian Delegation to the U.N., 140 E. 57th St., 5th Fl., New York, N.Y. 10022
PATMOS, A. Edward, Wittenberg Univ., Springfield, Ohio
PATON, William A., 2203 Hill St., Ann Arbor, Mich.
PATRICK, Hugh T., Yale Econ. Growth Center, Box 1987 Yale Sta., New Haven, Conn.
PATTEE, F. V., New England Col., P.O. Box 177, Henniker, N.H. 03242
PATTEN, Joseph M., Esso Research and Engr. Co., P.O. Box 172, Linden, N.J.
PATTERSON, Braxton I., Univ. of Wisconsin, 3203 Downer Ave., Milwaukee 11, Wis.
PATTERSON, (Mrs.) D. Jeanne, 619 N. Fess Ave., Apt. J-3, Bloomington, Ind.
PATTERSON, David A., 3409 Kesterwood Rd., Knoxville 18, Tenn.
PATTERSON, Ernest F., Box 513, Davidson, N.C. 28036
PATTERSON, Ernest M., F-405 Garden Court Apt., 47th and Pine Sts., Philadelphia 43, Pa.
PATTERSON, Franklin, Lincoln Filene Center, Tufts Univ., Medford, Mass. 02052
PATTERSON, Gardner, Gen. Agreement on Tariffs and Trade, Villa le Bocage, Palais des Nations, Geneva 10, Switzerland
PATTERSON, James M., 619 N. Fess Ave., Apt. J-3, Bloomington, Ind.
PATTERSON, Kenneth D., 2010 N. 27, Corvallis, Ore. 97330
PATTERSON, Robert T., 1400 20th St., N.W., Washington, D.C. 20006
PATTISON, John D., 111 St. Clair Ave. W., Toronto, Ont., Canada
PATTON, Arthur H., 12122 Shiremont, Dallas, Tex. 75230
PAUL, Allen B., 5813 Ipswich Rd., Bethesda, Md. 21811
PAUL, Charles T. A., 313 Suwanee, Clinton-Sherman AFB, Oklahoma 73632
PAUL, Donald E., Laclede Gas Co., 1017 Olive St., St. Louis, Mo. 63101
PAUL, Monique, Boston Col., Dept. of Econ., Chestnut, Mass. 02167
PAULEY, Eugene D., 449 Jefferson Ave., Cape Girardeau, Mo. 63701
PAULIS, Robert J., Univ. of Rhode Island, Bus. Admin. Col., Kingston, R.I. 02836
PAULY, Mark V., Univ. of Virginia, Dept. of Econ., Charlottesville, Va.
PAUNIO, Jouko, Inst. of Econ. Res., Bank of Finland, Helsinki, Finland
PAUSTIAN, Paul W., 33 East Cherokee Hills, Tuscaloosa, Ala.
PAVIS, Rhona L., 9304 Piney Branch Rd., Silver Spring, Md. 20910
PAVLICK, Anthony L., Twinkling Star Rd., Whitewater, Wis. 53190
PAVORD, William C., 4125 Pomeroy Ct., Louisville, Ky. 40218
PAXSON, Dean A., Harvard Bus. Sch., Doctoral House, Boston, Mass. 02163
PAYKIN, Bentley H., 227-28 Murdock Ave., Queens Village, N.Y. 11429
PAYNE, James P., Jr., Louisiana State Univ., Dept. of Econ., Baton Rouge 3, La.
PAYNE, John Barton, 933 Golf Course Dr., St. Louis 32, Mo.
PAYNE, W. Scott, Inst. for Defense Anal., 400 Army-Navy Dr., Arlington, Va. 22202
PAYNE, Wilson F., 288 Charles River St., Needham, Mass.
PAYNTAR, (Mrs.) Sally C., Standard Oil Co. of N.J., Econ. Dept., 30 Rockefeller Plaza, New York, N.Y.
PAZEMENAS, Joseph P., 89-41 Hollis Ct. Blvd., Queens Village, N.Y. 12141
PAZOS, Felipe, 1725 Eye St. N.W., Washington 6, D.C.
PEACH, William N., Univ. of Oklahoma, Bur. of Bus. Res., Norman, Okla. 73069
PEACOCK, Alan T., Univ. of York, Heslington, York, England
PEAKE, Charles F., Univ. of Maryland, Econ. Dept., College Park, Md. 20742
PEARCE, C. A., 225 Seaman Ave., Rockville Centre, N.Y.
PEARCE, Walter H., Univ. of Kentucky, Col. of Com., Lexington, Ky.
PEARLSTEIN, Ned C., 18 The Uplands, Berkeley 5, Calif.
PEARMAN, Allen L., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
PEARMAN, Jean R., Florida State Col., Sch. of Soc. Welfare, Tallahassee, Fla.
PEARSON, (Mrs.) David B., 6030 N. Sheridan Rd., Apt. 2002, Chicago, Ill. 60626
PEARSON, David C., 2610 Las Lunas St., Pasadena, Calif. 91107
PEARSON, Donald W., Univ. of Texas, Dept. of Econ., Austin, Tex.
PEARSON, John E., Texas A. and M. Univ., Sch. of Bus. Admin., College Sta., Tex.
PEARSON, Robert W., 7510 Hopkins Ave., College Park, Md.

PEASE, Dustin S., 14 Candleberry Rd., Barrington, R.I.
PEASE, Laurel E., 431 W. College, Granville, Ohio 43023
PECHMAN, Joseph A., 7112 Wilson Lane, Bethesda, Md. 21811
PECK, George S., State Univ. of Iowa, Iowa City, Iowa
PECK, Gustav, 4501 Connecticut Ave., Washington 8, D.C.
PECK, Harvey W., 50 Howell Branch Rd., Winter Park, Fla.
PECK, Henry A., Univ. of Maine, Alumni Hall, Orono, Me. 04473
PECK, Maynard A., Sterling Col., Sterling, Kans. 67579
PECK, Merton J., Yale Univ., Dept. of Econ., 402 Strathcona Hall, New Haven, Conn.
PEDAN, John C., 218 S. Cedar, Galesburg, Ill. 61401
PEDERSON, Eldor O., 3514 N. Verde, Tacoma, Wash. 98407
PEDROTTI, Benedick J., San Fernando Valley St. Col., Dept. of Econ., Northridge, Calif.
PEEK, Ronald M., 9305 Fleetwood Dr., Lemon Valley, Nev.
PEEK, William W., Chico State Col., Div. of Soc. Sci., Chico, Calif.
PEER, Y. M., 6922 Jeffery Ave., Chicago, Ill. 60649
PEERA, Nurali, Moberly Tower, Burlington St., Manchester 15, England
PEERY, Joseph S., Univ. of Utah, Col. of Bus., Salt Lake City, Utah
PEEVEY, Michael R., 601 Buena Vista Ave., San Francisco 17, Calif.
PEGRUM, Dudley F., Univ. of California, Los Angeles, Calif.
PEIRCE, William S., Case Institute of Tech., Div. of Org. Sci., Cleveland, Ohio 44106
PEIRSOL, Elizabeth H., Box 1882, Route 1, Port Richey, Fla. 33568
PEITCHINIS, S. G., 11 Medway Crescent, London, Ont., Canada
PEIXOTO, Aluizio B., Rua Candido Mendes, 1070-202, Rio de Janeiro-GB-Brazil
PEJOVICH, Svetozar, St. Mary's Col., Winona, Minn.
PELEKOUDAS, Constantine G., Antioch Col., Dept. of Econ., Yellow Springs, Ohio 45387
PELOVSKY, Gerald M., 750½ N. 1st West, Logan, Utah 84321
PELTZMAN, Sam, Univ. of California, Dept. of Econ., Los Angeles 24, Calif.
PELZ, Edward J., 1261 Madison Ave., New York 28, N.Y.
PEMBERTON, Donald K., Hiram Col., Dept. of Econ., Box 211, Hiram, Ohio 44234
PENDERGAST, Russell A. J., Univ. of Alberta, St. Josephs Col., Edmonton Campus, Edmonton, Alta., Canada
PENDLETON, Edwin C., Univ. of Hawaii, Col. of Bus. Admin., 2500 Campus Rd., Honolulu, Hawaii 96822
PENDLETON, Thomas H., Concord Col., Div. of Bus., Athens, W.Va. 24712
PENDLETON, William, The Ford Found., 477 Madison Ave., New York, N.Y. 10022
PENN, Erwin S., 1111 Massachusetts Ave., Apt. 306, Washington, D.C. 20005
PENN, Raymond J., Univ. of Wisconsin, 316 B Agric. Hall, Madison, Wis. 53706
PENN, William M., Jr., Duke Univ., Dept. of Econ., Durham, N.C. 27706
PENN, William S., Jr., Mgt. and Econ. Res., Inc., 800 Welch Rd., Palo Alto, Calif. 94304
PENNER, Rudolph G., Univ. of Rochester, Dept. of Econ., Rochester, N.Y. 14627
PENO, Joseph D., Jr., Univ. of Texas, Dept. of Econ., Austin, Tex.
PENROSE, Edith, Shawcroft, Fawkham Ave., New Barn, Longfield, Kent, England
PERAGALLO, Edward, Col. of the Holy Cross, Worcester 3, Mass.
PERCUS, Philip M., 15 Brunell Ave., Lenox, Mass. 01240
PERER, William, 2353 Tilbury St., Pittsburgh 17, Pa.
PERET, J. Corland G., 201 Noland St., Falls Church, Va. 22046
PEREZ-CASTILLO, Juan P., Edificio COMVAR, Apto. 2, Prolong. Ave. Principal #68, Los Chorros, Caracas, Venezuela
PEREZ-OLIVARES, Manuel V., Edificio Polar-Piso 13, Plaza Venezuela, Caracas, Venezuela
PERKEL, George, Textile Workers Union of Amer., 99 University Pl., New York, N.Y. 10003
PERKINS, Charles E., Amer. Hotel and Motel Asso., 221 W. 57th St., New York, N.Y. 10019
PERKINS, Dwight H., 319 School St., Watertown, Mass. 01083
PERKINS, John E., Arlington State Col., Arlington, Tex.
PERKINS, Maurice F., Michigan State Univ., Agric. and Int. Programs, Agricultural Hall 113, East Lansing, Mich.
PERKINS, William C., U.S. Military Academy, Dept. of Soc. Sci., West Point, N.Y. 10996
PERLES, Benjamin M., 38 Everett St., Newton Centre, Mass. 01355
PERLINE, Martin M., Wichita State Univ., Dept. of Econ., Wichita, Kans. 67208
PERLMAN, Jacob, 2900 Connecticut Ave., N.W., Apt. 249, Washington, D.C. 20008
PERLMAN, Mark, Univ. of Pittsburgh, Econ. Dept., Pittsburgh 13, Pa.
PERLMAN, Richard W., Univ. of Wisconsin-Milwaukee, Econ. Dept., Milwaukee, Wis. 53211
PERLMUTTER, Edith L., 5324 6th Ave., Los Angeles, Calif. 90043
PERLO, Victor, Park Trail, Croton-on-Hudson, N.Y. 10520
PERLOFF, Harvey S., 3809 Raymond St., Chevy Chase, Md. 21620
PERNIAPEREZ, Nestor J., 1533 Arthur St., Eugene, Ore. 97402
PERRING, Katherine, 925 25th St. N.W., Apartment 805, Washington 7, D.C.
PERROUX, Francois, 6 Blvd. des Capucines, Paris 90, France
PERRY, G. Neil, 3240 Beach Dr., Victoria, B.C., Canada
PERRY, George L., 115 E. 51st St., Minneapolis, Minn. 55419
PERRY, Jesse J., 405 S. Main, Galax, Va. 24333
PERRY, Joseph M., Univ. of Florida, Col. of Bus. Admin., Dept. of Econ., Gainesville, Fla. 32603
PERRY, Louis B., 515 Boyer Ave., Walla Walla, Wash. 99362
PERRY, Richard J., 94 Manor Ct., Birmingham, Mich.
PERRY, Robert H., 34 Linden Ave., Atherton, Calif. 94025
PERSONS, Robert H., Jr., 16 Whitehouse Rd., White Plains, N.Y.
PESEK, Boris P., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
PESKE, Edgar, Illinois Bell Telephone Co., 212 W. Washington Blvd., Chicago, Ill. 60606
PESKIN, Henry M., 4024 Calvert St., N.W., Washington, D.C. 20007
PESKIN, (Mrs.) Janice K., 4024 Calvert St., N.W., Washington, D.C. 20007
PESSEMIER, Edgar A., 172 Creighton Rd., West Lafayette, Ind. 47178
PETCHENIK, Kenneth H., World Book Encyclopedia, Merchandise Mart, Chicago, Ill. 60654
PETER, Hollis W., 2649 Heather Way, Ann Arbor, Mich.
PETER, Philip, 482 University Ave., Los Altos, Calif.
PETERKE, Werner H., Clarion State Col., Soc. Sci. Dept., Clarion, Pa. 16214
PETERS, Douglas D., 4800 Walnut St., Apt. 214, Philadelphia, Pa. 19139
PETERS, Francis M., Jr., 3201 White St., Ravenswood Park, Falls Church, Va. 22044
PETERS, (Mrs.) Sarah C., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
PETERS, W., The Univ., Dept. of Econ., Manchester 13, England
PETERS, William S., Arizona State Univ., Col. of Bus., Tempe, Ariz.
PETERSEN, Harold A., Boston Col., Econ. Dept., Chestnut Hill, Mass. 02167
PETERSEN, Lawrence L., 9127 Santayana Dr., Fairfax, Va. 22030
PETERSEN, Walter T., 509 Rose Ave., Park Ridge, Ill. 60068

PETERSON, Clair A., 41 Poppy Rd., Carmel Valley, Calif. 93924
PETERSON, D. E., Univ. of Illinois, Grad. Sch. of Bus. Admin., 319 Commerce West, Urbana, Ill. 61803
PETERSON, Edward D., Mankato State Col., Box 208, Mankato, Minn.
PETERSON, G. Shorey, Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
PETERSON, Jerrold M., 1611 Valley Rd., Apt. D-3, Champaign, Ill.
PETERSON, John M., Univ. of Arkansas, Col. of Bus. Admin., Fayetteville, Ark.
PETERSON, (Mrs.) Mary P., 533 Riddle Rd., Cincinnati, Ohio 45220
PETERSON, R. D., Univ. of Idaho, Col. of Bus. Admin., Dept. of Econ., Moscow, Idaho 83843
PETERSON, Richard C., Bank of America, Dept. of Econ., Rincon Annex, P.O. Box 3415, San Francisco, Calif.
PETERSON, Richard E., P.O. Box 208, Forest Knolls, Calif. 94933
PETERSON, W. H., 475 Riverside Dr., Princeton, N.J. 08540
PETERSON, Wallace C., Univ. of Nebraska, Dept. of Econ., Lincoln, Neb. 68508
PETERSON, Weber H., Villanova Univ., Dept. of Econ., Villanova, Pa. 19406
PETERSON, William A., 1459 Ridgewood Dr., Jenison, Mich. 49428
PETERSON, Willis L., Univ. of Minnesota, Inst. of Agric., Dept. of Agric. Econ., St. Paul, Minn. 55101
PETIT, A. C. Rodriquez, 286 Broad Ave., Leonia, N.J. 07605
PETIT, Thomas A., Drury Col., Breech Sch. of Bus., Springfield 2, Mo.
PETRAS, George S., EBS Mgt. Consultants, Inc., 2 Rector St., New York, N.Y. 10006
PETRECOLLA, Alberto O., Virrey del Pino 3210, Buenos Aires 26, Argentina
PETREI, Amalio H., Av. 24 dr Septembre 1615, Cordoba, Argentina
PETRELLA, Frank, 245 Grennwood St., Worcester 7, Mass.
PETRENAS, Algimantas M., 15 Claremont Ave., New York 27, N.Y.
PETRIE, Mary, 4720 Linden Pl., P.O. Box 549, Downers Grove, Ill. 60515
PETRO, Andrew J., Notre Dame Univ. Econ. Dept., Col. of Arts and Letters, Notre Dame, Ind. 46556
PETROGIANNIS, D. S., P.O. Box 765, Athens, Greece
PETROVIC, Marko, Simina 22, Stan 10, Belgrade, Yugoslavia
PETRUZELLI, Nicholas M., 11522 Maple View Dr., Silver Springs, Md. 20902
PETSHEK, Kirk R., 2513 E. Park Pl., Milwaukee 11, Wis.
PETT, Mary I., Dept. of State, E-OA-AP-5826, 2201 C St., N.W., Washington, D.C.
PETTEE, E. W., North Dakota Agric. Col., State College Sta., Fargo, N.D.
PETTEE, James C., 2353 N. Oakland St., Arlington 7, Va.
PETTENGILL, Robert B., 1901 Wyllys St., Midland, Mich. 48642
PETTWAY, Richard H., Michigan State Univ., Dept. of Acc. and Fin. Admin., Eppley Center, East Lansing, Mich. 48823
PETZENBAUM, Hannah, 251 W. 89th St., Apt. 5C, New York, N.Y. 10024
PFANNER, (Mrs.) Margaret R., 24 River Glen, 645 N. Broadway, Hastings-on-Hudson, N.Y. 13076
PFAUTER, Hermann, 6632 Sabado Tarde Rd., Goleta, Calif. 93017
PFEFFER, Aloysius R., 730 24th St., N.W.-#305, Washington, D.C. 20037
PFEFFER, Irving, Univ. of California, Grad. Sch. of Bus., Los Angeles 24, Calif.
PFEFFER, Philip M., 418 S. Graham St., Apt. 3, Carbondale, Ill. 62903
PFISTER, Richard L., Dartmouth Col., Dept. of Econ., Hanover, N.H.
PFOUTS, Ralph W., Univ. of North Carolina, Dept. of Econ., Chapel Hill, N.C.
PFUSTERSCHMIDT, Heinrich, Rue Notre Dame 42, Luxemburg, Luxemburg
PHARES, Donald, 145 Smalley Rd., Syracuse, N.Y. 13210
PHARISS, John W., 8318 Varona Dr., Hyattsville, Md.
PHARR, John D., Jr., 2011 Kendon Dr., W., Pittsburgh 35, Pa.
PHAUP, Marvin M., Jr., Box 186, Roanoke Col., Salem, Va. 24153
PHELON, Ronald W., 32 Galveston St., S.W., # 101, Washington, D.C. 20032
PHELPS, Clyde W., 2930 Alta Laguna Blvd., Laguna Beach, Calif. 92651
PHELPS, (Mrs.) Edmund S., Cowles Found., Box 2125, Yale Sta., New Haven, Conn.
PHELPS, Edmund S., Cowles Found., Box 2125, Yale Sta., New Haven, Conn.
PHELPS, Ernest D., South House, Chandler Farms, Pomfret, Conn. 06258
PHELPS, John, 255 Raymond St., Rockville Centre, N.Y.
PHELPS, Orme W., Claremont Men's Col., Claremont, Calif.
PHILBROOK, Clarence E., Univ. of North Carolina, Carroll Hall, Chapel Hill, N.C.
PHILIP, Brother Daniel, La Salle Col., Philadelphia, Pa.
PHILIPPATOS, George, 452 Ft. Washington Ave., New York, N.Y. 10033
PHILIPPS, Denis S., 301 West 108th St., New York 25, N.Y.
PHILIPPS, Eugene A., Univ. of Maryland, APO, New York, N.Y. 09403
PHILIPS, G. Edward, Pennsylvania State Univ., Col. of Bus. Admin., University Park, Pa. 16802
PHILLIPS, Almarin, Univ. of Pennsylvania, Dept. of Econ., Philadelphia 4, Pa.
PHILLIPS, Charles F., Jr., Washington and Lee Univ., Newcomb Hall, Room 21, Lexington, Va. 24450
PHILLIPS, Charles W. E., 86-43 107th St., Richmond Hill, N.Y. 11418
PHILLIPS, Chester A., 721 N. Linn St., Iowa City, Iowa
PHILLIPS, Clinton A., Tulane Univ., Sch. of Bus. Admin., New Orleans, La. 70118
PHILLIPS, (Mrs.) Doris G., California State Col. at Fullerton, Fullerton, Calif. 92631
PHILLIPS, E. Bryant, Univ. of Southern California, Dept. of Econ., University Park, Los Angeles, Calif. 90007
PHILLIPS, Edmund J., Jr., 1135 E. Verdugo, Burbank, Calif.
PHILLIPS, Edward L., Univ. of Colorado, Dept. of Econ., 1100 14th Ave., Denver, Colo. 80202
PHILLIPS, Edward S., Univ. of Missouri at Kansas City, 204 Haag Hall, Kansas City, Mo. 64110
PHILLIPS, Harold, Andrews Univ., Dept. of Bus. Admin., Berrien Springs, Mich.
PHILLIPS, Harold M., First Nat. Bank of Oregon, P.O. Box 3457, Portland 8, Ore.
PHILLIPS, Irving P., 703 Ludlow St., Takoma Park, Md. 21784
PHILLIPS, James, 1917 Seneca Rd., Canby Park, Wilmington, Del. 19805
PHILLIPS, James A., 2142 Derek Dr., Apt. C, Fullerton, Calif.
PHILLIPS, John G., 2550 Ross Rd., Silver Spring, Md.
PHILLIPS, John M., First Nat. Bank Tube 64, P.O. Box 1136, 399 Park Ave., New York 22, N.Y.
PHILLIPS, Joseph B., 382 Wellington Rd., Delmar, N.Y. 12254
PHILLIPS, Joseph D., Univ. of Illinois, 428 David Kinley Hall, Urbana, Ill.
PHILLIPS, Keith E., Portland State Col., Dept. of Econ., Portland 1, Ore.
PHILLIPS, Philip R., 1572 Broad St., Bloomfield, N.J. 07004
PHILLIPS, Robert W., 524 Hooper Lane, Chapel Hill, N.C. 27515
PHILLIPS, Samuel H., Jr., Mary Washington Col., Box 1128, Fredricksburg, Va.
PHILLIPS, W. G., Univ. of Windsor, Dept. of Econ., Windsor, Ont., Canada
PHILLIPS, William E., 1231-½ Wexham Way, Inglewood, Calif. 90302
PHILLIPS, William J., 301 Dunreath St., Lafayette, La.
PHINNEY, Josiah T., 37 Herrick Ave., Delmar, N.Y. 12054

PHIPPS, Irving R., P.O. Box 1166, Sarasota, Fla. 33578
PHLIPS, Louis, Centre de Recherches Economiques, Universite Catholique de Louvain, 2 B, rue E. Van Even, Louvain, Belgium
PHOTIAS, Nikos G., 5327 41 St. N.W., Washington 15, D.C.
PICARD, Fred Q., Ohio Univ., Dept. of Econ., Athens, Ohio 45701
PICCINI, Raymond, 553 N. 12th St., New Hyde Park, N.Y.
PICKART, Delmore M., 1913 Regent St., Madison 5, Wis.
PICKEN, Robert F., 1228 E. 56th St., Chicago 37, Ill. 60637
PICKER, (Mrs.) Anne D., 35 Pine St., Yonkers, N.Y.
PICKERING, (Mrs.) Margaret H., Fed. Home Loan Bank Bd., Office of Res. and Home Fin. Room 770, Washington, D.C. 20552
PICKERING, Richard C., Fed. Home Loan Bank Bd., Office of Res. and Home Fin. Room 770, Washington, D.C. 20552
PICKERSGILL, Gary M., Univ. of Washington, Dept. of Econ., Seattle, Wash.
PICKERSGILL, Joyce S., Univ. of Washington, Econ. Dept., Seattle, Wash.
PICKETT, John C., Univ. of Missouri, Dept. of Econ., B and PA Bldg., Columbia, Mo. 65201
PICKETT, Ralph R., Univ. of Kentucky, Col. of Com., Lexington, Ky.
PIDOT, George, Jr., Dartmouth Col., Dept. of Econ., Hanover, N.H.
PIEDRA, Alberto M., 5707 Kirkwood Dr., Washington 16, D.C.
PIEKARSKI, Julian A., 5570 N. Bay Rudge Ave., Milwaukee, Wis. 53217
PIEKARZ, Rolf R., 10829 Georgia Ave., Silver Spring, Md.
PIERCE, Alfred E., 509 Parsons St., Easton, Pa. 18042
PIERCE, J. Eugene, Univ. of Tenn., Bus. Admin. Col., Knoxville, Tenn.
PIERCE, James L., Bd. of Gov., Fed. Res. System, Div. of Res. Statis., Washington, D.C. 20551
PIERSON, Frank, Swarthmore Col., Swarthmore, Pa. 19081
PIERSON, Gail, 630 Williams Ave., Natchitoches, La. 71457
PIERSON, Gordon K., USOM, APO, San Francisco 96346
PIERSON, John H. G., 22 Pilot Rock Lane, Riverside, Conn. 06878
PIGMAN, Nathaniel M., Jr., 1610 Martha Ter., Rockville, Md. 20852
PIGOTT, William, III, Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
PIKE, James F., 670 Oriole Ave., W. Hempstead, L.I., N.Y. 11552
PIKL, I. James, Jr., Univ. of Wyoming, Dept. of Econ., Laramie, Wyo. 82071
PILHOFER, Hans, Massey-Ferguson Ltd., Econ. Res. Dept., 200 University Ave., Toronto 1, Ont., Canada
PILLAI, N. G., Loyola Col., Dept. of Econ., Montreal, P.Q., Canada
PILLSBURY, Warren A., 2838 Westminster Rd., Bethlehem, Pa. 18017
PILVIN, Harold, 108 Hampshire Rd., Great Neck, N.Y.
PINCKNEY, Annette M., 1425 17th St. N.W., No. 603, Washington, D.C.
PINCUS, John A., c/o RAND Corp., 1000 Connecticut Ave., N.W., Washington, D.C. 20036
PINES, Jerome M., 104-40 Queens Blvd., Forest Hills, N.Y. 11375
PINNELL, William, Indiana Univ., Sch. of Bus., Bloomington, Ind.
PINOLA, Rudolph, Univ. of Alberta, Edmonton, Alta., Canada
PINSKY, Gerald A., 1505 Grand Concourse, New York, N.Y. 10452
PINTER, Matthew D., 1171 Levona, Ypsilanti, Mich. 48197
PINTO, Peter J. J., Elizabeth House, Gokhale Rd. S., Dadar, Bombay 28, India
PIORE, Nora K., 40 Worth St., Room 720, New York, N.Y. 10013
PIOTROWSKI, Marian, 1003 Main St., Arkadelphia, Ark.
PIOVIA, Frank, 406 S. Carlyn Spring, Arlington 4, Va.
PIRON, Robert, 25 College Pl., Oberlin, Ohio 44074
PISCIOTTOLI, Louis F., Duke Univ., Econ. Dept., Durham, N.C. 27706
PI-SUNYER, Pere, Standard Oil Co. (New Jersey), Gen. Econ. Dept., 30 Rockefeller Plaza, New York 20, N.Y.
PITCHER, Hugh M., 1462 Oak St., Evanston, Ill. 60201
PITIGLIANI, Fausto R., Via Antonio Allegri 13, Rome, Italy
PITKIN, William A., Southern Illinois Univ., Carbondale, Ill. 62903
PITT, Courtnay H., 429 Clement Rd., Jenkintown, Pa. 19046
PITTERMAN, Marvin, Univ. of Rhode Island, Col. of Bus. Admin., Kingston, R.I. 02836
PIZER, Samuel, 217 Normandy Dr., Silver Spring, Md.
PLAN, Edgar, 2925 Bellevue Ter. N.W., Washington 16, D.C.
PLANT, Kenneth Morton, 910 S. Michigan, Room 1727, Chicago, Ill. 60680
PLANTE, Joseph A., 1115 Laurel Ave., St. Paul, Minn. 55104
PLANTZ, Don V., Arizona State Univ., Dept. of Econ., Tempe, Ariz.
PLASSCHAERT, Sylvain R. F., 53, avenue Lebon Oudergem, Brussels 16, Belgium
PLATH, Raymond A., Eastern Illinois Univ., 216 Coleman Hall, Charleston, Ill. 61920
PLATT, Elliott N., 4048 Baltimore Ave., Philadelphia, Pa. 19104
PLATT, Robert B., 147-26 Hawthorne Ave., Flushing, N.Y. 11355
PLAUT, Walter, 701 Madison Ave., New York 21, N.Y.
PLAXTON, Arthur N., 1300 Adams, Apt. 102, Denver, Colo. 80206
PLESHER, Michael A., 2949 East Shangri-La Rd., Phoenix, Ariz.
PLESSNER, Yakir, Iowa State Univ., East Hall, Ames, Iowa 50010
PLIMSOLL, James, Dept. of External Affairs, Canberra, A.C.T., Australia
PLOTKIN, Norman, 106 E. Kirkwood Ave., Claremont, Calif. 91712
PLOTNICK, Alan R., S.M.T.I. Dept. of Econ., North Dartmouth, Mass. 02747
PLOTNIK, Mortin J., Franklin Col., Dept. of Econ., Franklin, Ind. 46131
PLOTT, Charles R., 2501 Soldiers Home Rd., Bldg. 20, Riverview Apts., West Lafayette, Ind.
PLOWMAN, E. Grosvenor, 48 Partridge Circle, Portland, Me.
PLUHAR, Ivan, 270 Park Ave., New York, N.Y. 10024
PLUMLEY, Alden J., Univ. of Nevada, Dept. of Econ., Reno, Nev. 89507
PLUMMER, Addison W., 5816 Dean Dr., McLean, Va. 22101
PLUMMER, David E., 724 International Blvd., Apt. 28, Houston, Tex. 77024
PLUMMER, James L., Hasbrouck Apts. 51-D, Ithaca, N.Y. 14850
PLUNKERT, (Mrs.) William, 601 E. 20th St., Apt. 11-H, New York 10, N.Y.
POAPST, J. V., Univ. of Toronto, Sch. of Bus., Toronto 5, Ont., Canada
PODEA, Titus, 817 Fifth Ave., New York, N.Y. 10021
PODELL, Richard N., Vanderbilt Hall, 107 Ave. Louis Pasteur, Boston, Mass. 02115
PODOFF, David, Apt. 410 Westgate, Cambridge 39, Mass.
PODOSKI, Joseph J., 3732 Windom Pl., Washington, D.C. 20016
POE, Douglas, 410 Normal Ave., Normal, Ill. 61761
POE, Jerry B., Drury Col., Springfield, Mo. 65802
POENSGEN, Otto H., Mass. Inst. of Tech., Dept. of Econ., Cambridge, Mass. 02139
POGUE, Joseph E., Mountain Lake, Lake Wales, Fla. 33853
POGUE, Thomas F., 701 Grant St., Iowa City, Iowa 52240
POINDEXTER, J. Carl, Mt. Regis, Salem, Va. 24153

POKEMPNER, Stanley J., 249 Shady Brook Lane, Princeton, N.J. 08540
POKRUPA, Z. Peter, c/o Shell Oil Co., P.O. Box 100, Calgary, Alta., Canada
POLACH, Jaroslav G., 225 S. Panorama Dr., Oxon Hill, Md., Washington, D.C. 20021
POLAK, George, West Liberty State Col., Dept. of Bus. and Econ., West Liberty, W.Va. 26074
POLAK, Jacques J., Int. Monetary Fund, 19th and H Sts. N.W., Washington 25, D.C.
POLAK, Robert B., Armour and Co., 401 N. Wabash Ave., Chicago, Ill. 60611
POLAKOFF, Murray E., 24 Crossbar Rd., Hastings-on-Hudson, N.Y. 10706
POLAND, Orville F., State Univ. of New York, Grad. Sch. of Pub. Affairs, Albany, N.Y. 12226
POLAVIEJA, Enrique G. de, Plaza de Chamberi 10, Madrid 10, Spain
POLEMAN, Thomas T., Jr., Cornell Univ., Dept. of Agric. Econ., Warren Hall, Ithaca, N.Y.
POLEMIS, (Mrs.) Bernice W., 5000 S. Cornell Ave., 7B, Chicago, Ill. 60615
POLENSKE, Karen R., 84 Prescott St., Apt. 47, Cambridge, Mass. 01922
POLINSKY, Ella J., 116 W. University Pkwy., Baltimore 10, Md.
POLK, Isaac N., Jr., 4416 Summercrest Ct., Ft. Worth, Tex. 76109
POLK, Judd, 4 Washington Sq. Village, New York, N.Y. 10012
POLK, Lon, 400 Fair Lane, Dearborn, Mich. 48124
POLLACK, Gerald A., 1100 6th St. S.W., Washington 24, D.C.
POLLACK, Irving, 121 E. 182nd St., Bronx, N.Y. 10453
POLLAK, Robert A., 123 W. Plumstead Ave., Lansdowne, Pa. 19050
POLLAK, William, 1008 Park St., Grinnell, Iowa 50112
POLLARD, Robert F., 247 S. 40th St., Philadelphia, Pa. 19104
POLLOCK, David H., 7, Chemin du Ravin, Chambesy-Geneva, Switzerland
POLLOCK, David M., 5220 S. Kenwood Ave., # 509, Chicago, Ill. 60615
POLLOCK, Frederick, 6926 Montagnola, Tessin, Switzerland
POLLOCK, Richard L., 1150 12th St. N.W., Washington, D.C. 20005
POLNER, Walter, 213 Chestnut St., Madison, Wis. 53705
POLSTER, Nathaniel, Action for Appalachian Youth, P.O. Box 3228, Charleston, W.Va. 25332
POLYZOIDES, A. Th., P.O. Box 769, North Hollywood, Calif.
POLZIN, Paul, 23190 W. Nine Mile Rd., Southfield, Mich.
POMERANTZ, Sidney I., The City Col. of New York, Dept. of History, Covent Ave. and 139th St., New York 31, N.Y.
POND, Martin T., Purdue Contract, USAID/ARDO, APO New York 09676
PONDER, Henry, Ft. Valley State Col., P.O. Box 265, Ft. Valley, Ga. 31030
PONDER, S. Gordon, Greenwood Acres, R. 1, Cary, N.C. 27511
PONTECORVO, Giulio, Columbia Univ., Grad. Sch. of Bus., New York, N.Y.
PONTNEY, Jack A., California State Col. at Fullerton, Dept. of Econ., Fullerton, Calif. 92631
POOL, John C., Univ. of Colorado, Dept. of Econ., Boulder, Colo.
POOLE, Elmer W., 913-A 25th St., San Diego, Calif. 92102
POOLE, Kenyon E., Northwestern Univ., Dept. of Econ., Evanston, Ill.
POOLE, Richard W., Oklahoma State Univ., Col. of Bus., Dept. of Econ., Stillwater, Okla.
POOLE, William, Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore 18, Md.
POORE, Daniel M., 120 Washington Rd., Forest Hills, Pittsburgh 21, Pa.
POPE, James Arthur, III, Guilford Col., Dept. of Econ., Greensboro, N.C. 27410
POPE, Norris F., Diablo Valley Col., Concord, Calif.
POPE, Ralph M., 20 Bertha St., Lowell, Mass.
POPER, Frank J., 162 E. 24th St., Paterson, N.J.
POPESCU, Oreste, Naciones Unidas, Casilla 686, La Paz, Bolivia
POPKIN, (Mrs.) Helen F., 123 Duddington Pl., S.E., Washington, D.C. 20003
POPKIN, Joel, 123 Duddington Pl., S.E., Washington, D.C. 20003
POPOVIC, Nenad D., 319 Wedgewood Ter., Syracuse-De Witt, N.Y. 13214
POPPE, Janus, 8416 Alyce Pl., Alexandria, Va. 22308
POPPER, Frederick E., Fisheries Div. F.A.O., Room 547, Viale delle Terme Di Caracalla, Rome, Italy
POPPER, Robert D., 11368 Fowlers Mill Rd., Chardon, Ohio 44024
POROY, Ibrahim I., Univ. of British Columbia, Econ. Dept., Vancouver, B.C., Canada
PORSCHE, Edgar C., 15045 S.W. 133rd Ave., Tigard, Ore. 97223
PORTER, Arthur R., Jr., Box 247, Hanover, Ind. 47243
PORTER, Donald G., 811 E. Squantum St, Quincy, Mass. 02171
PORTER, George T., Jr., 2729 Taft Ave., Santa Clara, Calif. 95051
PORTER, John H., Indiana Univ., Sch. of Bus., Bloomington, Ind. 47401
PORTER, Michael G., 1425 Alma St., Palo Alto, Calif. 94301
PORTER, Richard C., 1296 Newport Rd., Ann Arbor, Mich.
PORTER, Richard L., Marquette Univ., Milwaukee, Wis. 53233
PORTER, Robert B., 913 Southlea Dr., Lafayette, Ind.
PORTER, Robert J., Univ. of South Carolina, Sch. of Bus. Admin., Columbia, S.C. 29208
PORTERFIELD, Robert J., 404 E. Nelson Ave., Alexandria 2, Va.
PORTES, Richard D., Balliol Col., Oxford, England
PORTEUS, Homer S., 2800 Woodley Rd. N.W., Washington, D.C. 20008
PORTSCHE, Robert L., 1700 Crestline Dr., Lincoln, Neb.
PORTUGAL, Roy S., 30 Woodland Ave., Apt. 2, San Francisco, Calif. 94117
POSCHEL, Jerome R., 1301 Delaware Ave., S.W., N808, Washington, D.C. 20024
POSNER, Robert, Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48104
POSNIAK, Edward G., 5404 39th St. N.W., Washington 15, D.C.
POSPESCHIL, Fred W., 627 Prospect Ave., Janesville, Wis.
POST, Albert, 6322 32d Street N.W., Washington 15, D.C.
POST, George R., 671 Wavell Ave., Ottawa, Ont., Canada
POSTLE, Walter, 355 Scenic Rd., Fairfax, Calif. 94930
POSTWEILER, R. A., Univ. of Colorado, Sch. of Bus., Boulder, Colo.
POTTER, Helen, Loyola Univ. Dept. of Econ., 820 N. Michigan Ave., Chicago, Ill. 60611
POTTER, Laurence E., 1904 N. Johnson St., Arlington 7, Va.
POULIOT, Jean-Marie, Bldg. A-12, Apt. 8, New Slocum Hgts., Syracuse, N.Y. 13210
POULSEN, Ronald K., 206 Dryden Dr., Meriden, Conn.
POULSEN, Roy G., Univ. of Rhode Island, Dept. of Econ., Kingston, R.I. 02836
POULSON, Barry W., 775 37th St., Boulder, Colo.
POWELL, Burt, State Univ. of N.Y. at Buffalo, Dept. of Econ., Crosby Hall, Library Circle, Buffalo, N.Y. 14214
POWELL, George M., 816 Naudain Ave., Claymont, Del. 19809
POWELL, H. Robert, Jewel Tea Co., 135 S. La Salle St., Chicago, Ill.
POWELL, J. Richard, California State Col., Dept. of Econ., Long Beach, Calif. 90804
POWELL, (Mrs.) Joyce G., 2231 N. Columbus St., Arlington, Va.
POWELL, Jules V., U.S. Dept. of Agric., Econ. Res. Ser., Washington 25, D.C.
POWELL, Raymond P., Yale Univ., Dept. of Econ., New Haven, Conn.
POWELSON, John P., Univ. of Pittsburgh,

Grad. Sch. of Publ. and Int. Affairs, Pittsburgh 13, Pa.
POWER, John H., Gale Rd., Williamstown, Mass. 01267
POWERS, Edward J., 19 Greenway St., Pittsfield, Mass. 01202
POWERS, John A., Univ. of Cincinnati, Col. of Bus. Admin., Cincinnati, Ohio 45221
POWERS, Richard, 118 Oak Rd., James City County, Williamsburg, Va. 23185
POWERS, William J., Clark Equipment Co., Ind. Truck Div., Mkt. Plan., Battle Creek, Mich.
POWERS, William L., Youngstown Univ., 410 Wick Ave., Youngstown 3, Ohio
POWRIE, T. L., Univ. of Alberta, Dept. of Econ., Edmonton, Alta., Canada
POYTRESS, William H., 19324 Monte Vista Dr., Saratoga, Calif. 95070
PRACHOWNY, Martin F. J., 1016 E. Ann St., Ann Arbor, Mich.
PRACHT, Raymond W. T., c/o Amer. Consulate Gen., Zurich, Switzerland
PRAGER, Jonas, New York Univ., Dept. of Econ., Bronx, N.Y. 10453
PRAKASH, Som, Duquesne Univ., Sch. of Bus., Pittsburgh, Pa.
PRANGER, J. J., 2455 Borton Dr., Santa Barbara, Calif.
PRANZO, Mary L., 23-30 32nd St., Astoria, N.Y. 11105
PRASAD, S. Benjamin, Univ. of Nevada, Col. of Bus. Admin., Reno, Nev. 89507
PRASOW, Paul, 338 Oceano Dr., Los Angeles 49, Calif.
PRAST, William G., 141 E. 89th St. Apt. 6-K, New York 28, N.Y.
PRATHER, Charles Lee, 2200 Pkwy., Austin, Tex. 78703
PRATT, Robert W., Jr., 10 Old Wagon Rd., Old Greenwich, Conn. 06870
PRATT, Wilburn J., 61 Beaumont, Sheppard AFB, Tex. 76311
PREBISCH, Raul, Naciones Unidas Com. Econ. para Amer. Latina, Casilla 179-D, Santiago, Chile
PREEG, Ernest H., U.S. Mission, Geneva, Dept. of State, Washington, D.C.
PREHN, Edward C., Jr., 98 Louis St., Staten Island 4, N.Y.
PRENDERGAST, George A., 132 Edgehill Rd., Bala-Cynwyd, Pa. 19004
PRESCH, (Mrs.) Birdie K., 2 Carmello Dr., Edison, N.J.
PRESCOTT, Edward, 6038 Jackson Ave., Pittsburgh, Pa. 15206
PRESCOTT, James R., Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa
PREST, Wilfred, Univ. of Melbourne, Econ. Dept., Parkville N. 3, Victoria, Australia
PRESTON, (Mrs.) Esme, Monash Univ., Faculty of Econ., P.O. Box 92, Clayton, Victoria, Australia
PRESTON, Lee E., Jr., Univ. of Calif., Sch. of Bus. Admin., 113 South Hall, Berkeley 4, Calif.
PRESTON, Stanley W., 672 Nelson Dr., Baton Rouge, La. 70808
PRETTI, Reno C., 2905 S. 102nd St., Omaha, Neb.
PREU, Fred, 3616 Davis St., N.W., Washington 7, D.C.
PREUSS, Erwin L., Box 25, Madison, S.D. 57042
PREWITT, Roy A., 4339 26th St., N., Arlington, Va. 22207
PRIBRAM, (Mrs.) Edith, 3625 16th St., N.W., Washington, D.C. 20010
PRIBRAM, Karl, 3625 16th St., N.W., Washington, D.C. 20010
PRICE, C. Hoyt, 2809 McKinley St., N.W., Washington, D.C. 20015
PRICE, (Mrs.) Charlotte A., 501 W. 123rd St., Apt. 18A, New York, N.Y. 10027
PRICE, Daniel N., 10529 Truxton Rd., Adelphi, Md. 21710
PRICE, David W., 309 Sunset Dr., Pullman, Wash. 99163
PRICE, (Mrs.) Dorothy Z., 309 Sunset Dr., Pullman, Wash. 99163
PRICE, G., Univ. of Queensland, Dept. of Econ., St. Lucia, Brisbane, Queensland, Australia
PRICE, Harry E., P.O. Box 2077, Georgia Southern Branch, Statesboro, Ga. 30459
PRICE, Hugh B., Univ. of Kentucky, Dept. of Mkt. and Rural Fin., Lexington, Ky.
PRICE, James E., Syracuse Univ., Dept. of Econ., Syracuse, N.Y. 13210
PRICE, Lawrence E., Virginia State Col., Box 404, Petersburg, Va.
PRICE, Lee G., 8200 Colston Pl., Chevy Chase, Md. 20015
PRICE, Ralph B., Western Maryland Col., Westminster, Md. 21158
PRICE, Reginald C., 1358 40th St., Sacramento 19, Calif.
PRICE, William S., 402 Dawson St., Saulte Ste. Marie, Mich.
PRICHARD, Lewis G., c/o First Nat. Bank of Oregon, P.O.B. 3131, Portland 8, Oregon
PRIESTLAND, Carl H., 7717 Elba Rd., Alexandria, Va. 22306
PRIME, John Henry, New York Univ., Sch. of Com., Washington Sq., New York, N.Y.
PRIMEAUX, Walter J., Jr., 3773 Odin Ct., Apt. 1, Houston, Tex. 77021
PRIMUS, John C., 117½ S. Market St., Oskaloosa, Iowa 52577
PRINCE, Jack E., 607 4th St., Williamstown, W.Va. 26187
PRINCE, Thomas R., 1245 Maple Ave., Wilmette, Ill. 60091
PRINCE, Timothy P., 68-60 108th St., Forest Hills, N.Y. 12068
PRINDL, Andreas R., 215 Adams St., Apt. 9-G, Brooklyn, N.Y. 11201
PRINDLE, W. D., Prindle and Maland, Odd Fellows Bldg., Montevideo, Minn.
PRINGLE, Robert G., C.N.R., Room 253, Winnipeg, Man., Canada
PRINZ, Arthur M., 1807 Suncrest Dr., Mt. Rte. 1, Carlisle, Pa. 17013
PRIOR, Roger A., G.P.O. Box 244, Sydney, N.S.W., Australia
PRISCO, Joseph C., 301 Warren Ave., E. Providence, R.I. 02914
PRITCHARD, Benjamin F., Jr., 75-A Longhill St., Springfield, Mass. 01108
PRITCHARD, Leland J., Univ. of Kansas, Room 207 Summerfield, Lawrence, Kans.
PRITCHARD, Norris T., 11813 Caplinger Rd., Silver Spring, Md. 20904
PROCHNOW, Herbert V., First Nat. Bank of Chicago, 38 S. Dearborn St., Chicago, Ill. 60690
PROCHNOW, Herbert V., Jr., First Nat. Bank of Chicago, 38 S. Dearborn St., Chicago 90, Ill. 60690
PROCHNOW, Lloyd A., 7024 Hector Rd., McLean, Va. 20201
PROCTOR, Edward W., 6526 Byrnes Dr., McLean, Va. 22101
PRODROMIDIS, Kyprianos, Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
PROSCHAN, Arnold, 6545 South St., Falls Church, Va. 22042
PROSCHANSKY, Harris, 306 E. 96th St., New York, N.Y.
PROSPER, Peter A., Jr., Union Col., Div. of Soc. Sci., Schenectady, N.Y.
PROUDLEY, George E., Jr., 6209 12th St., N.W., Washington, D.C. 20011
PROULX, Pierre-P. G., 5567 Woodbury, Montreal, Que., Canada
PRUITT, Eleanor M., 199 Chestnut, San Carlos, Calif.
PRUNELLA, Warren J., 328 Ellen Dr., Cheektowago, N.Y. 14225
PRUSSIA, Leland S., Jr., 300 Montgomery St., Room 405, San Francisco 20, Calif.
PRUSSING, (Mrs.) Laurel L., 16 Allen St., Arlington, Mass. 02174
PRYBYLA, Jan S., Pennsylvania State Univ., Col. of Arts, Dept. of Econ., 329 Boucke Bldg., University Park, Pa. 16802
PRYOR, Frederic L., 205 Sheffield Ave., New Haven, Conn. 06714
PRYOR, (Mrs.) Zora, 205 Sheffield Ave., New Haven, Conn. 06714
PRZYGODA, Jacek, Loyola Univ., Los Angeles, Calif. 90045
PSACHAROPOULOS, George, 1453 E. 54th Pl., Chicago, Ill. 60615
PUCCI, Carlos A., Coronel Zelaya 128, San Miguel de Tucuman, Tucuman, Argentina

PUCHALSKI, Henry, c/o Domtar Construction Materials, 1 Pl. Ville Marie Suite 2210, Montreal, Que., Canada
PUCKETT, Richard H., 5905 Knollbrook Dr., Hyattsville, Md.
PUFFER, Claude E., Univ. of Buffalo Found., Hayes Hall, Administration Rd., Buffalo, N.Y. 14214
PUFFER, Frank W., Univ. of California at Los Angeles, Grad. Sch. of Bus. Admin., Los Angeles 24, Calif.
PUGH, Olin S., Univ. of South Carolina, Dept. of Econ., Columbia 1, S.C.
PUGLISI, Michael A., 190 Willoughby St., Apt. 8-D, Brooklyn, N.Y. 11201
PULLARA, Santo J., Lowell Tech. Inst., Lowell, Mass. 01854
PULLEN, Robert W., 85 Summer St., Waterville, Me.
PULSIPHER, Allan G., Texas A and M Univ., Dept. of Econ., College Station, Tex.
PUMA, John J., Boston Edison Co., 800 Boylston St., Boston, Mass. 02199
PURCELL, Bradford M., McGraw-Hill, Inc., F.W. Dodge Co., 330 W. 42nd St., New York, N.Y. 10036
PURCELL, Robert N., Univ. of California-Santa Barbara, Dept. of Econ., Santa Barbara, Calif. 93106
PURITANO, Vincent, 3403 Spring Lane, Apt. 102, Falls Church, Va. 22041
PURVES, Donald F., Canadian Nat. Railways, P.O. Box 8100, Montreal, Que., Canada
PUTH, Robert C., Earlham Col., Dept. of Econ., Richmond, Ind.
PUTMAN, Edward, 3208 A Glenview Ave., Austin, Tex. 78703
PYE, Gordon B., Univ. of California, 350 Barrows Hall, Berkeley, Calif.
PYE, John L., 92 Laurel Hill Rd., Northport, L.I., N.Y. 11768
PYLE, Hugh G., 743 McKee St., State College, Pa. 16801
PYLE, John F., 6912 Oakridge Rd., Hyattsville, Md.
PYLE, Rilla Q., c/o K. B. Thayer, 6703 Burning Tree Dr., Houston, Tex.
PYUN, Chong Soo, Mercer Univ., Dept. of Econ., Macon, Ga.

QUACKENBUSH, Charles C., 212 W. Washington St., Room 1601, Chicago, Ill. 60606
QUALLS, LeRoy L., 1017 Cherokee Dr., Tallahassee, Fla.
QUANCE, Calvin L., 1571-I Spartan Village, East Lansing, Mich.
QUANDT, Richard E., Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
QUANTIUS, Francis W., Ohio State Univ., 1775 S. College Rd., Columbus 10, Ohio
QUAST, Theodore E., Jr., 2727 29th St., N.W., Apt. 309, Washington, D.C. 20008
QUICK, Elwyn B., D. C. Heath and Co., 285 Columbus Ave., Boston, Mass. 02116
QUICK, George S., HQ. JBUSMC, APO New York 09676
QUIGLEY, John M., 2807 Arlington Blvd., Arlington, Va. 22201
QUIGLEY, Patrick J., 5704 Ave. H, Brooklyn, N.Y. 11234
QUIJANO, Carlos N., W. R. Grace and Co., 3 Hanover Sq., New York, N.Y. 10005
QUINDRY, Kenneth E., Dept. of Taxation, State Off. Bldg., Room 1000, 1 W. Wilson St., Madison, Wis.
QUINN, Brian, 5542 N. Morgan St., Alexandria, Va.
QUINN, Robert J., 28 Donna Rd., Framingham, Mass.
QUINTANA, Carlos, Sierra Guadarrama 155, Mexico 10, D.F., Mexico
QUINTERO, Carmelo, Nat. Econ. Coun., Padre Faura St., Manila, Philippines
QUINTERO-RAMOS, Angel M., Univ. of Puerto Rico, P.O. Box 21879, Rio Piedras, P.R. 00931
QUINTO, Leon, 200 E. 66th St., New York 21, N.Y.
QUIRIN, E. Lafayette, 5 Elm St., Wellesley, Mass. 02181
QUIRIN, George D., Univ. of British Columbia, Fac. of Com., Vancouver, B.C., Canada
QUIRIN, Paul, Koenigsalle 64, Duesseldorf, Germany
QUIRK, Charles B., Providence Col., Providence, R.I. 02908
QUITTMEYER, Charles L., Col. of William and Mary, Williamsburg, Va. 23185

RABEN, Steven A., 863 W. 41st St., Houston, Tex. 77018
RABIN, Joseph E., 7916 Maryknoll Ave., Bethesda, Md. 20034
RABITSCH, Elizabeth K., 3425 Ridgewood Ave., Montreal 26, Que., Canada
RABOLD, Robert W., 361 Woodland Ave., Williamsport, Pa. 17701
RACHAL, J. Valley, Jr., North Carolina State Univ., 216-1911 Bldg., Dept. of Econ., Raleigh, N.C.
RACHFORD, Patrick J., 1057 Meta Dr., Cincinnati 37, Ohio
RACHMEL, Leo, Box 228, R. 2, Prince George, Va. 23875
RACITI, Sebastian J., St. Peter's Col., Dept. of Econ., Jersey City, N.J. 07306
RACZ, Gabriel, 105 E. Bellows, Mount Pleasant, Mich. 48858
RADA, Edward L., Univ. of California, Sch. of Pub. Health, Los Angeles 24, Calif.
RADAKOVIC, George, 7530 Dickson St., Pittsburgh, Pa.
RADE, Arthur M., Academy Lane, Elkins Park, Pa. 19117
RADFORD, Gene A., 30 Indian Spring Lane, Rochester 18, N.Y.
RADHE, Stig, Kungsgatan 4B, Stockholm, Sweden
RADO, Emil R., Glasgow Univ., Dept. of Int. Econ. Studies, 10 Southpark Ter., Glasgow W.2, Scotland
RADOSTA, John M., 312 Jamesville Rd., DeWitt, N.Y. 13214
RADOV, Karl B., Univ. of California, Dept. of Econ., Div. of Soc. Sci., Irvine, Calif. 92664
RADSPIELER, Anthony, 2030 F St., N.W., Apt 707, Washington, D.C. 20006
RADUCHEL, William J., 224 Hubbell St., East Lansing, Mich. 49931
RADZIUL, Michael T., 451 Vermont St., Brooklyn, N.Y. 11207
RAE, James D., Univ. of British Columbia, Dept. of Econ. and Polit. Sci., Vancouver 8, B.C., Canada
RAFFAELE, Joseph A., 447 Barclay Rd., Rosemont, Pa. 19368
RAFFEL, (Mrs.) Helen, 5387 Montgomery Ave., Philadelphia 31, Pa.
RAFFERTY, John A., 320 W. 87th St., New York, N.Y. 10024
RAFUSE, Robert W., Jr., 111 Duddington Pl., S.E., Washington, D.C. 20003
RAGAN, Gilbert C., Dana Col., Dept. of Econ., Blair, Neb. 68008
RAHBANY, K. Philip, 3336 Kirkham, Columbus 21, Ohio
RAID, Howard D., Bluffton Col., Bluffton, Ohio 45817
RAIKES, Charles F., White and Case, 14 Wall St., New York 5, N.Y.
RAILING, William F., Gettysburg Col., Dept. of Econ. and Bus. Admin., Gettysburg, Pa. 17325
RAIMON, Robert L., Col. of Ind. and Labor Rela., Cornell Univ., Ithaca, N.Y.
RAINE, Jesse E., 1831 Wellington Dr., Murfreesboro, Tenn.
RAINES, Fredric Q., Washington Univ., Dept. of Econ., P.O. Box 208, St. Louis, Mo. 63130
RAITI, Joseph V., Hq. PACAF, Box 816, APO San Francisco, 96553
RAJ, James S., 13A Selvam Nagar, Tanjore, Madras State, India
RAJGRODSKI, Pinchas, 818 Argonne Dr., Baltimore, 18, Md.
RALLIS, John J., 167 Woodland St., South Natick, Mass. 01260
RAMAN, Chakaravarthi S., Univ. of Pennsylvania, Wharton Sch., Econ. Res. Unit, Philadelphia, Pa. 19104
RAMAN, N. Pattabhi, U.N. Development Program, 866 U.N. Plaza, New York, N.Y. 10017
RAMAY, Coye A., Mobil Oil Co., P.O. Box 5444, Terminal Annex, Denver, Colo. 80217

RAMIREZ, J. A., 328 Mt. Vernon Ave., San Francisco, Calif. 94112
RAMIREZ-PEREZ, Miguel A., P.O. Box 23010, University Sta., Rio Piedras, P.R.
RAMOS, Francisco A., Rua Antonio Bezerra, 911 Aldeota, Fortaleza-Ceara, Brazil
RAMSEY, Fredlyn, 3051 Idaho Ave., N.W., Washington, D.C. 20016
RAMSEY, (Mrs.) Gloria L., 602 Carroll Dr., Garland, Tex.
RAMSEY, James B., 703-E Eagle Hgts., Madison 5, Wis.
RAMUGLIA, Anthony Ignatzic, c/o California Dept. of Water Res., P.O. Box 2385, Fresno, Calif. 93721
RANA, Sher J., 1111 Arlington Blvd., Arlington, Va. 22209
RANCI, Pippo, 1521 S. State St., Apt. 201, Ann Arbor, Mich.
RAND, Robert N., 2719 Yale Sta., New Haven, Conn. 06520
RANDALL, Darrell, 3211 45th St. N.W., Washington 16, D.C.
RANDALL, Elbert, P.O. Box 358, Otisville, N.Y. 10963
RANDALL, (Mrs.) Laura R., 425 Riverside Dr., New York 25, N.Y.
RANDALL, R. J., Ministry of Fin., P.O. Box R.W. 62, Lusaka, N. Rhodesia
RANDOLPH, Isaac, Gov. of Liberia, Bur. of Econ. Res., Monrovia, Liberia
RANGARAJAN, C., Univ. of Rajasthan, Jaipur, Rajasthan, India
RANHAND, Samuel, 33-39 80th St., Jackson Hgts. 12, N.Y.
RANIS, Gustav, Yale Univ. Dept. of Econ., Econ. Growth Center, Box 1987, Yale Sta., New Haven, Conn.
RANLETT, John G., Sacramento State Col., 6000 Jay St., Sacramento 19, Calif.
RANSCHBURG, Herbert J., 65-15 38th Ave., Woodside 77, N.Y.
RANSOM, Roger L., Univ. of Virginia, Dept. of Econ., Charlottesville, Va.
RANSON, Thomas B., Western State Col., Dept. of Econ., Gunnison, Colo. 81230
RANT, Walter F., 19 Beresford Rd., Allendale, N.J. 07401
RAO, Nandirajo K., Univ. of California, Dept. of Econ., Santa Barbara, Calif.
RAPHAEL, Lawrence, 1729 S. Crest Dr., Los Angeles, Calif. 90035
RAPHAELSON, Arnold H., 8622 Manchester Ct., Apt. 1, Silver Spring, Md. 20901
RAPKIN, Chester, 473 West End Ave., New York 24, N.Y.
RAPOPORT, John, 4418 Osage Ave., Philadelphia, Pa.
RAPP, William V., 325 E. 72nd St., New York, N.Y.
RAPPING, Leonard A., Carnegie Inst. of Tech., Grad. Sch. Ind. Admin., Pittsburgh, Pa. 15213
RAPSON, Ira J., Jr., Westmont Col., Santa Barbara, Calif.
RASCHE, Robert H., 115 Glendale, Ann Arbor, Mich. 48103
RASHID, Abdul, 16 Arlington Ter., Edgewater, N.J. 07020
RASHISH, Myer, 3407 Rodman St., N.W., Washington 8, D.C.
RASKIND, Leo J., Ohio State Univ., Col. of Law, 1659 N. High St., Columbus, Ohio 43210
RASMUSSEN, David W., 6034 Kingsbury Ave., St. Louis, Mo. 63112
RASMUSSEN, Jewell J., Univ. of Utah, Dept. of Econ., Salt Lake City, Utah 84112
RASMUSSEN, Jon A., 2214 Maple Ave., Apt. 3-B, Evanston, Ill. 60201
RASMUSSEN, Poul N., Vangeleddei 87, Virum, Denmark
RASMUSSEN, Robert J., 1813 Vera Cruz MCSC, Barstow, Calif. 92311
RATCHFORD, B. U., 7311 W. Franklin St., Richmond 26, Va.
RATCLIFF, Richard U., Univ. of Wisconsin, Com. Bldg., Madison, Wis.
RATH, John J., 8250 Bramell, Detroit, Mich. 48239
RATLIFF, Charles E., Jr., Davidson Col., Dept. of Econ., Davidson, N.C. 28036
RATNER, Sidney, 11 Cleveland Lane, Princeton, N.J. 08540
RATURI, Anand S., DR-2, Banaras Hindu Univ., Varanasi 1, U.P., India
RAUCHSCHWALBE, Otto, P.O. Box 7572, Ben Franklin Sta., Washington, D.C. 20044
RAUNER, Robert M., Dept. of Com., Econ. Dev. Admin., Washington, D.C. 20230
RAUP, (Mrs.) Clara Eliot, 21 Claremont Ave., New York 27, N.Y.
RAUP, Philip M., Univ. of Minnesota, Dept. of Agric. Econ., Univ. Farm, St. Paul 1, Minn.
RAUSCH, Erwin, 112 Wilshire Dr., Cranford, N.J. 07016
RAUSHENBUSH, Paul A., 2110 Vilas Ave., Madison, Wis. 53711
RAWLINS, V. Lane, 224 Wilson St., Albany, Calif. 94706
RAWSON, William S., Box 9201, Duke Sta., Durham, N.C.
RAY, Anandarup, 5110 S. Kenwood Ave., Apt. 306, Chicago, Ill. 60637
RAY, Caldwell L., Texas Western Col., Dept. of Econ., Box 42, El Paso, Tex.
RAY, Donald P., 1505 28th St., S., Arlington, Va. 22206
RAY, Harold A., 4774 W. 17th St. Ter., Topeka, Kans.
RAY, Robert S., Ford Found., P.O. Box 1081, Nairobi, Kenya
RAY, Ronald G., DISTRIBUTION AGE, Chestnut and 56th Sts., Philadelphia, Pa. 19139
RAYACK, Elton, Univ. of Rhode Island, Dept. of Econ., Kingston, R.I. 02836
RAYMOND, Fred I., 1408 Golden Rain Rd., Apt. 3, Walnut Creek, Calif. 94529
RAYMOND, Jerral C., Sweet Briar Col., Sweet Briar, Va.
RAYMOND, Richard D., West Virginia Univ., Col. of Com., Morgantown, W.Va. 26506
RAYNAULD, André, Univ. of Montreal, Dept. of Econ., Case Postale 6128, Montreal, Que., Canada
RAYNSFORD, Robert W., Metallgesellchaft, Reuterweg 14, 6 Frankfurt a.M., Germany
REA, K. J., Univ. of Saskatchewan, Dept. of Econ. and Polit. Sci., Saskatoon, Sask., Canada
READ, Leonard E., Found. for Econ. Educa., Irvington-On-Hudson, N.Y.
REAGAN, (Mrs.) Barbara B., 3840 Greenbrier, Dallas, Tex.
REAGAN, Sydney C., 3840 Greenbrier, Dallas, Tex.
REAGEN, Edward P., Skidmore Col., Econ. Dept., Saratoga Springs, N.Y. 12866
REARDON, Paul A., 8 Aldeah, Columbia, Mo.
REBER, Hugh J., R.D. #2, Newton, Conn. 06470
RECHEL, Ralph E., 904 New Hampshire N.W., Washington 7, D.C.
RECORD, Jane C., 2397 Hillside Lane, Lake Oswego, Ore. 97034
REDDING, Alan D., A.I.D., APO New York 09254
REDDY, G. V. N., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
REDDY, M. Balwanth, Univ. of Essex, Dept. of Econ., Wivenhoe Park, Colchester, Essex, England
REDER, Melvin W., Stanford Univ., Dept. of Econ., Stanford, Calif. 94305
REDLICH, Fritz L., 42 Oak St., Belmont, Mass. 02019
REDMAN, John C., Univ. of Kentucky, Dept. of Agric. Econ., Lexington, Ky. 40506
REDMAN, Milton B., 314 E. Stoughton, Champaign, Ill.
REDMOND, Barron C., 2170 Fortress, Drayton Plains, Mich. 48020
REED, Douglas L., 154 Homestead Ave., Weymouth, Mass. 02188
REED, Edward W., Univ. of Oregon, Sch. of Bus. Admin., Eugene, Ore.
REED, Francis L. C., 1477 W. Pender St., 4th Fl., Vancouver 5, B.C., Canada
REED, Harold L., 422 Cayuga Hgts. Rd., Ithaca, N.Y.
REED, Richard W., 1115 Prospect Ave., Plainfield, N.J. 07060
REED, Ritchie H., 4186 Suitland Rd., Apt. 102, Suitland, Md. 20023
REED, Stanley F., 706 RCA Bldg., 1725 K St., N.W., Washington, D.C. 20006

REEDER, Charles B., E.I. du Pont de Nemours Co., Econ. Off., Wilmington 98, Del.
REEDY, John H., Arthur D. Little, Inc., Acorn Park, Cambridge, Mass. 02140
REEDY, Theodore W., 5437 20th St., N., Arlington, Va. 22205
REESE, Albert E., 1126 E. 59th St., Chicago, Ill. 60637
REESE, D. Gene, Harvard Advisory Group, P.O. Box 668, Lahore, Pakistan
REESE, Jim E., Univ. of Oklahoma, Dept. of Econ., Norman, Okla. 73069
REEVE, Joseph E., 8014 Park Overlook Dr., Bethesda, Md. 20034
REEVES, Hubert L., 1629 Columbia Rd., N.W., Apt. 317, Washington 9, D.C.
REFAKIS, Peter E., 4515 Indiana Ave., Ft. Wayne, Ind.
REFFNER, James A., R. 2, Box 60, Wausau, Wis. 54401
REFIOR, Everett L., 205 N. Fremont St., Whitewater, Wis. 53190
REGAN, Edward J., Jr., 10 Oak Ridge Pl., Eastchester, N.Y. 10709
REGAN, Lawrence G., Res. Anal. Corp., McLean, Va. 22101
REGAN, William J., 719 Barcelona Dr., Millbrae, Calif.
REGIER, Donald W., USDA, Econ. Res. Service, For. Dev. and Trade Div., Washington, D.C. 20250
REGUERO, Miguel A., Drexel Inst. of Tech., Col. of Bus. Admin., 32nd and Chestnut Sts., Philadelphia, Pa. 19104
REHER, William C., Arlington State Col., Sch. of Bus. and Econ., Arlington, Tex. 76010
REHME, Joseph L., Jr., 3117 Little Greek Lane, Alexandria, Va.
REHMET, Paul O., 19 Lesley Lane, Old Bethpage, N.Y. 11804
REHN, Henry J., Southern Illinois Univ., Carbondale, Ill.
REICH, Joseph, 8 Hampton Rd., Scarsdale, N.Y.
REICH, Ronald R., 6645 N. Newgard, Chicago, Ill. 60626
REICHARD, Robert S., 4555 Henry Hudson Pkwy., Bronx, N.Y. 10471
REICHLEY, Marlin S., Ind. Col. of the Armed Forces, Ft. L. J. McNair, Washington 25, D.C.
REID, Alvin A., 454 Cameron St., Daytona Beach, Fla. 32014
REID, Graham L., Univ. of Glasgow, Dept. of Soc. and Econ. Res., 12 Bute Gardens, Glasgow, W.2, Scotland
REID, John J., Col. of the Holy Cross, Dept. of Econ., Worcester, Mass.
REID, Margaret G., 1155 E. 56th St., Chicago, Ill. 60637
REID, William E., Jr., 1362 Somerset Pl., N.W., Washington, D.C. 20011
REIERSON, Roy L., 80 Cranberry St., Brooklyn 1, N.Y.
REIFLER, Ronald M., 915 Sheridan Rd., Evanston, Ill.
REIFMAN, Alfred, 7008 Wilson Lane, Bethesda, Md. 20034
REIGER, William, 241 Swartz St., Dunmore, Pa. 15930
REIGHARD, Edward, Fresno State Col., Div. of Bus., Fresno, Calif.
REILING, Eldon A., Northern Illinois Univ., Dept. of Econ., De Kalb, Ill. 60115
REILLY, Edward E., 1240 Richmond St., London, Ont., Canada
REILLY, Frank K., Univ. of Kansas, Sch. of Bus., Lawrence, Kans. 66045
REILLY, Rodney R., 5011 Allan Rd., Washington, D.C. 20016
REILLY, William R., Univ. of Connecticut, Col. of Agric., Dept. of Agric. Econ., Storrs, Conn. 06268
REIMER, Richard D., Col. of Wooster, Dept. of Econ., Wooster, Ohio
REINECKE, John A., Louisiana State Univ. in New Orleans, Div. of Bus. Admin., Lakefront, New Orleans, La.
REINERTSEN, Philip J., 271 North Quaker Lane, West Hartford, Conn. 06090
REING, Calvin H., 3238 Messick Ave., Oceanside, L.I., N.Y. 11770
REINHARDT, Hedwig, 39 Gramercy Park N., Apt. 16-B, New York, N.Y. 10010
REINHART, Blaise F., Sienna Col., Loudonville, N.Y. 13659
REINL, Harry C., 2506 N. 20th Rd., Arlington 1, Va.
REINSTEIN, Jacques J., 4105 46th St., Washington 16, D.C.
REISCHER, Otto R., c/o AID Mission, Amer. Embassy, Karachi, Pakistan, APO 271, New York, N.Y. 09271
REISER, Brian S., 5015 Kerby Hill Rd., Oxon Hill, Md. 20022
REISMAN, George G., 245 E. 24th St., New York, N.Y. 10010
REISS, Franklin J., 2003 S. Anderson St., Urbana, Ill. 61801
REISS, John B., Washington Univ., Dept. of Econ., St. Louis, Mo. 63130
REJA, George E., Univ. of Nebraska, Dept. of Econ., Lincoln, Neb.
RELLAHAN, John J., Florida State Univ., Graduate Program, Box 4487, Patrick AFB, Fla. 32925
REMBA, Oded I., 38 Georgia St., Cranford, N.J. 07016
REMER, Charles F., 1117 Portal Ave., Oakland, Calif.
REMINGTON, Lawrence J., Michigan Technological Univ., Dept. of Bus. Eng. Admin., Houghton, Mich. 49931
REMSON, Andrew C., Jr., Off. of the Army Attache, APO New York 09794
RENDICH, Michael F., Pinesbridge Rd., R.F.D. No. 1, Box 125, Mt. Kisco, N.Y. 10549
RENNE, Roland R., U.S. Dept. of the Interior, Off. of Water Res., Washington, D.C.
RENNIE, Henry G., 815 N. High St., Apt. 56, Columbus, Ohio 43215
RENNIE, Robert A., Nationwide Insurance Co., 246 N. High St., Columbus 16, Ohio
RENNING, H. Dieter, Kenyon Col., Dept. of Econ., Gambier, Ohio 43022
RENO, Philip, 44 Hermosa, N.E., Albuquerque, N.M.
RENSHAW, Robert H., Northern Illinois Univ., Dept. of Econ., Watson Hall, De Kalb, Ill. 60115
RENWICK, Donald D., 2864 Northwest Blvd., Columbus 21, Ohio
REPETTO, Robert C., Ford Found., 32 Ferozeshah Rd., New Delhi, India
REPS, David N., 140 S. Lang Ave., Pittsburgh 8, Pa.
REQUE, (Mrs.) Rose B., 2475 Virginia Ave., N.W., Washington, D.C. 20037
RESEK, Robert W., Univ. of Illinois, Dept. of Econ., Urbana, Ill.
RESNICK, Idrian N., The Univ. Col., P.O. Box 9184, Dar Es Salaam, Tanzania
RESNICK, (Mrs.) Jane R., Univ. Col., P.O. Box 9184, Dar es Salaam, Tanzania
RESNICK, Stephen A., Dept. of Econ., Box 1987, Yale Sta., New Haven, Conn.
RESNIK, Frank, 1285 N. Sweetzer, Los Angeles, Calif. 90069
RETTIG, Raymond B., Univ. of Washington, Econ. Dept., Seattle, Wash. 98105
REUBEN, Arthur L., Somerset County Plan. Bd., County Board Bldg., Somerville, N.J. 08873
REUBENS, (Mrs.) Beatrice G., City Col. of New York, Dept. of Econ., New York, N.Y. 10031
REUBENS, Edwin P., City Col. of New York, Dept. of Econ., Convent Ave. and 139th St., New York, N.Y. 10031
REUBER, G. L., Univ. Col. of Western Ontario, Univ. Col. of Arts and Sci., Dept. of Econ. and Soc., London, Ont., Canada
REUSS, Frederick G., 519 Murdock Rd., Baltimore 12, Md.
REUSS, (Mrs.) Margaret M., 6448 Brooks Lane, Washington, D.C. 20016
REVENSKY, John E., 750 3rd Ave., New York, N.Y. 10017
REVZAN, David A., 1515 Oxford, Apt. 3A, Berkeley, Calif. 94709
REVZAN, Lawrence H., 10026 Palms Blvd., Apt. 7, Los Angeles, Calif. 90034
REWOLDT, Stewart, Sch. of Bus. Admin., Univ. of Michigan, Ann Arbor, Mich.
REYBOLD, John E., 327 1/2 Scott Ave., Ft. Collins, Colo. 80521

REYES, John R., 310 Adams St., San Antonio, Tex. 78210
REYNOLDS, Charles W., 140 West St., Room 1800, New York 7, N.Y.
REYNOLDS, Clark W., Dept. of Econ., Econ. Growth Center, Box 1987, Yale Sta., New Haven, Conn.
REYNOLDS, Donald C., 1230 Park Ave., Apt. 16-B, New York, N.Y. 10028
REYNOLDS, Ellsworth G., P.O. Box 365, West Dennis, Mass. 02670
REYNOLDS, Hal F., 10208 Colvin Run Rd., Great Falls, Va.
REYNOLDS, Irvine W., 7776 Ivanhoe Ave., P.O. Box 1530, La Jolla, Calif. 92038
REYNOLDS, Lloyd G., 37 Hillhouse Ave., New Haven, Conn.
REYNOLDS, (Mrs.) Mary T., Yale Univ., Dept. of Econ., Econ. Growth Center, Box 1987, Yale Sta., New Haven, Conn.
REYNOLDS, Reed S., 5143 Yermo, Toledo, Ohio 43613
REYNOLDS, Roy R., Kaiser Industries Corp., 300 Lakeside Dr., Oakland, Calif. 94604
REYNOLDS, Thomas H., 152 S. Monticello Dr., Stillwater, Okla. 74074
REYNOLDS, Thomas J., Old Morristown Rd., Bernardsville, N.J. 07924
REYNOLDS, William A., 50 Florence Dr., Chappaqua, N.Y. 10514
REYNOLDS, William M., Defiance Col., Defiance, Ohio 43512
REZLER, Julius, Loyola Univ., 820 N. Michigan Ave., Chicago, Ill. 60611
RHEE, Charles H. T., George Williams Col., Drexel Ave. at 53rd St., Chicago, Ill. 60615
RHETT, Leigh C., 870 Moana Dr., San Diego 6, Calif.
RHI, Jung Han, 27-3, 1 ka Chuagpa-Dong, Yongsan-ku, Seoul, Korea
RHOADS, William G., USAID—Bogota, Dept. of State, Washington, D.C. 20521
RHODES, Roger S., 2341 N. Circle Dr., Colorado Springs, Colo. 80909
RHODES, V. James, 1718 Cliff Dr., Columbia, Mo.
RHODES, William D., 224 S. Stone Ave., La Grange, Ill. 60525
RHOMBERG, Rudolf R., 4113 Everett St., Kensington, Md. 20795
RIANHARD, Lockwood, Jr., 238 Bedford Ave., Buffalo, N.Y. 14216
RICCARDO, James J., 18 Union St., Morrisville, N.Y. 13408
RICCIARDI, Franc M., Walter Kidde and Co., 675 Main St., Belleville, N.J. 07109
RICE, Donald B., Jr., 117 Brownell Circle, Monterey, Calif. 93940
RICE, Howard D., 3849 Windemer Dr., Florissant, Mo. 63033
RICE, James P., 189 W. High St., Waynesburg, Pa. 15370
RICE, Timothy E., 1034 Madison St., Syracuse, N.Y. 13210
RICH, Georg, Brown Univ., Econ. Dept., Providence, R.I. 02912
RICHARD, George, 54-40 Little Neck Pkwy., Little Neck, N.Y. 14754
RICHARD, Willis J., Indiana State Col., Dept. of Econ., Indiana, Pa. 15701
RICHARDS, C. Sydney, S. African Journal of Econ., Univ. of Witwatersrand, Milner Park, Johannesburg, S. Africa
RICHARDS, (Mrs.) Elizabeth A., Batten, Barton, Durstine and Osborn, 383 Madison Ave., New York 17, N.Y.
RICHARDS, Henry M. M., Muhlenberg Col., Allentown, Pa.
RICHARDS, Verlyn D., Kansas State Univ., Col. of Com., Calvin Hall, Manhattan, Kans. 66504
RICHARDSON, Barrie, Bethany Col., Bethany, W.Va. 26032
RICHARDSON, George R. J., 332 W. 101st, Apt. 4R, New York 25, N.Y.
RICHARDSON, Hayes A., Central Methodist Col., Fayette, Mo. 65248
RICHARDSON, Lee, 7038 Huron St., Denver 21, Colo.
RICHARDSON, Richard W., 5012 Rodman Rd., Washington, D.C. 20016
RICHERS, Raimar, Escola de Admin., de Empresas de Sao Paulo, Rua Martins Fontes 109-9° andar, Sao Paulo, Brazil
RICHEY, Charles I., Box 7894 University Sta., Austin 12, Tex.
RICHMAN, Barry M., Univ. of California, Sch. of Bus. Admin., Los Angeles 24, Calif.
RICHMAN, Raymond L., Univ. of Massachusetts, Dept. of Econ., Amherst, Mass.
RICHMOND, (Mrs.) Jennie, 68 Park Ave., Wakefield, Mass. 01880
RICHMOND, Samuel B., Columbia Univ., Sch. of Bus., New York 27, N.Y.
RICHTER, Charles E., Univ. of Illinois, Dept. of Econ., Urbana, Ill.
RICHTER, J. H., 6110 Old Dominion Dr., McLean, Va. 22101
RICHTER, J. J., 609 Wavell Ave., Ottawa 13, Ont., Canada
RICHTER, (Mrs.) Luba O., 6110 Old Dominion Dr., McLean, Va. 22101
RICHTER, Marcel K., 2150 Carter Ave., St. Paul, Minn. 55108
RICHTER, Rudolf, Eichendorffstrasse 18, 6601 Scheidt-Saar, Germany
RICKARD, Wilbur A., Jr., 109 N. Bayview Ave., Freeport, N.Y. 11520
RICKARDS, A. E., Canadian Pulp and Paper, 2280 Sun Life Bldg., Montreal 2, Que., Canada
RICKER, Charles S., Nat. Bank of Detroit, Detroit, Mich. 48232
RICKETTS, Faye M., Wichita State Univ., Wichita, Kans. 67208
RICO, Leonard, Univ. of Pennsylvania, Wharton Sch., Dietrich Hall, Rm. 191, Philadelphia 4, Pa.
RIDDERHOFF, Andrew D., 10716 S. Parnell, Chicago, Ill. 60628
RIDDLE, Bruce Z., 9100 S. W. 64th St., Miami, Fla.
RIDEN, Neal, 1056 Sherman Ave., Apt. 6-H, New York 37, N.Y.
RIDER, Robert W., 46-286 Heeia St., Honolulu, Hawaii 96744
RIDER, Rol W., Jr., 6651 Pasado Rd., Goleta, Calif. 93017
RIDGEL, Gus T., Kentucky State Col., Dept. of Bus. and Econ., Frankfort, Ky. 40601
RIDKER, Ronald G., 9419 Crosby Rd., Silver Spring, Md. 20910
RIEBER, Michael, Wayne State Univ., Dept. of Econ., Detroit 2, Mich.
RIECKEN, Richard C., 15 Woodside Ave., Metuchen, N.J.
RIEDY, Dan R., 2328 Cherry Hill Dr., Springfield, Ill. 62704
RIEFLER, Roger F., 4640 22nd N.E. Apt. 1, Seattle, Wash. 98105
RIEGEL, John W., Univ. of Michigan, Bur. of Ind. Rela., 312 Bus. Admin. Bldg., Ann Arbor, Mich.
RIEGER, Arnold M., 86 Tompkins Circle, (Ward Hill), Staten Island, N.Y. 10301
RIEGER, Hans-Christoph, Robert-Koch-Str. 25a, 6902 Sandhausen, Germany
RIEGER, Hans G., 50 Decker Ave., North Merrick, N.Y. 12967
RIEGER, Willard R., 3620 Barcroft View Ter., Apt. 302, Baileys Crossroads, Va. 22041
RIEKE, Wolfgang, Int. Monetary Fund, 19th and 11th Sts., Washington 25, D.C.
RIETTI, Mario, Colonia la Campana, Tegucigalpa, D.C., Honduras
RIEW, John H., Penn. State Univ., Dept. of Econ., University Park, Pa. 17774
RIGBY, Paul H., Pennsylvania State Univ., Center for Res., 229 Bouke Bldg., University Park, Pa. 16802
RIGGAN, Wilson B., 698 Danbury Rd., Cincinnati, Ohio 45240
RIGGIN, Richard E., R.R. 3 Hamilton Park, Muncie, Ind.
RIGGS, (Mrs.) Judith Metzger, 2739 Chelsea Dr., Oakland, Calif. 94611
RIGGS, Robert J., 11679 Westwood, Detroit 28, Mich.
RILEY, Donald C., 6100 Valley View Dr., Alexandria, Va. 22310
RILEY, H. E., 5512 Trent St., Chevy Chase, Md. 20015
RILEY, James W., Merck and Co., Inc., Rahway, N.J.

RILEY, Merrill J., 1114 Claflin Rd., Manhattan, Kans. 66502
RILEY, Robert C., Lebanon Valley Col., Annville, Pa. 17003
RILEY, Robert T., 1533 Circlefield Dr., Cincinnati 46, Ohio
RILEY, Roderick H., 7021 Richard Dr., Bethesda, Md. 20034
RIMLINGER, Gaston V., Rice Univ., Dept. of Econ., Houston 1, Tex.
RINEHART, James R., Newberry Col., Dept. of Econ., Newberry, S.C.
RING, Alfred A., R. 3, Box 186A-1, Gainesville, Fla.
RINGGENBERG, Ralph G., Univ. of Colorado, Sch. of Bus., Boulder, Colo.
RINGE, (Mrs.) Miriam K., 5430 Washington St., Downers Grove, Ill. 60515
RIPKA, G. Elizabeth, 2011 White Horse Rd., Berwyn, Pa. 19312
RIPLEY, Richard M., G-1 Section, Hq. USARSO, Ft. Amador, C.Z, APO New York 09834
RIPPEY, Robert S., Jr., Central Connecticut State Col., Dept. of Soc. Sci., New Britain, Conn.
RIPPS, Evelyn L., 4000 Massachusetts Ave., N.W., Apt. 933, Washington, D.C. 20016
RISCHER, Gunther, Eurofinance, 9, Av. Hoche, Paris VIII, France
RISKIN, Carl A., 141-11 78th Rd., Flushing 67, N.Y.
RISSER, Hubert E., Univ. of Illinois Campus, 200 Nat. Res. Bldg., Urbana, Ill.
RIST, Leonard B., 3130 Ordway St., Washington 8, D.C.
RITA, Joseph, 22 Fletcher Ave., Mount Vernon, N.Y.
RITCHIE, John C., Temple Univ., Sch. of Bus., Broad and Montgomery Sts., Philadelphia, Pa. 19122
RITCHIN, Hyman B., 1912 New York Ave., Brooklyn, N.Y. 11210
RITTENOURE, R. Lynn, 2511 Woodmont, Austin, Tex.
RITTER, Lawrence S., New York Univ., Grad. Sch. of Bus., 100 Trinity Pl., New York, N.Y. 10006
RITTSCHOF, Louis W., State Univ. Col., Plattsburgh, N.Y.
RITZ, Philip M., 11710 Karen Dr., Potomac, Md. 20854
RIVARD, Loren, 1309 Spring St., Apt. 301, Madison, Wis.
RIVERIN, Alphonse, 2160 Vermont St., Sherbrooke, Que., Canada
RIVERS, Robert L., Univ. of Mass., Sch. of Bus. Admin., Amherst, Mass.
RIVLIN, (Mrs.) Alice Mitchell, 2842 Chesterfield Pl., N.W., Washington, D.C. 20008
RO, Kong K., Blue Cross of West Pennsylvania, Res. and Consulting, 1 Smithfield St., Pittsburgh, Pa. 15222
ROACH, Jesse E., 4648 S. 2nd St., Arlington, Va.
ROBB, T. Bruce, 308 S. 12th St., Salina, Kans. 67401
ROBBINS, Lee P., Franklin and Marshall Col., Dept. of Econ., Lancaster, Pa. 17604
ROBBINS, Lionel C., London Sch. of Econ. and Pol. Sci., Houton St., Aldwych, London, W.C. 2, England
ROBBINS, Rueben T., 2041 Murray Hill Rd., Cleveland, Ohio 44106
ROBBINS, Sidney, Columbia Univ., 610 Uris Hall, New York, N.Y. 10027
ROBELLAZ, (Mrs.) Martha Y., 5630 Sheridan Rd., Apt. 416, Chicago, Illinois 60626
ROBERTS, Daniel J., Northeastern Univ., Boston, Mass.
ROBERTS, David R., Butler Univ., College of Bus. Admin., Indianapolis 7, Ind.
ROBERTS, Edward B., 17 Fellsmere Rd., Newton, Mass. 02159
ROBERTS, Evan O., West Virginia Univ., Col. of Com., Morgantown, W.Va.
ROBERTS, Frank L., California State Col., Fullerton, Calif.
ROBERTS, G. Lawrence, 46 Kipling Plaza, Clearwater, Fla. 33515
ROBERTS, George B., 6 Cedar Ave., Larchmont, N.Y.
ROBERTS, Harold S., Univ. of Hawaii, Ind. Rela. Center, 2500 Campus Rd., Honolulu, Hawaii 96822
ROBERTS, Harry V., Univ. of Chicago, School of Bus., Chicago, Ill. 60637
ROBERTS, Markley, 4931 Albemarle St., N.W., Washington, D.C. 20016
ROBERTS, Merrill J., Univ. of Pittsburgh, Bur. of Bus. Res., Pittsburgh 13, Pa.
ROBERTS, Paul E., Jr., 204 Finkbine Park, Iowa City, Iowa
ROBERTS, Ray C., Old Dominion Col., Dept. of Econ., Norfolk, Va. 23508
ROBERTS, Richard S., Jr., The Ford Found., Cairo Field Off., 477 Madison Ave., New York, N.Y. 10022
ROBERTS, Roy H., 257 Lisgar St., Apt. 908, Ottawa, Ont., Canada
ROBERTS, Thomas, Philadelphian Apts., Apt. 9b-24, 2401 Pennsylvania Ave., Philadelphia, Pa.
ROBERTS, Verl E., 8704 Glenwick Lane, La Jolla, Calif. 92037
ROBERTS, Warren A., 410 Crawford St., Crawfordsville, Ind. 47933
ROBERTS, William E., Miss. State Col. for Women, Box 1117, Columbus, Miss. 39701
ROBERTS, William J., Glore Forgan, Wm. R. Staats, Inc., 135 S. La Salle St., Chicago, Ill. 60603
ROBERTSON, Donald J., 3 Grosvenor Crescent, Glasgow W.2, Scotland
ROBERTSON, Jack E., West Virginia Tech. Col., Montgomery, W.Va. 25136
ROBERTSON, John E. L., 931 N. 25th, Paducah, Ky. 42001
ROBERTSON, Laurie, 320 N. 20th, Apt. 162, Omaha, Neb. 68102
ROBERTSON, Robert A., Indiana State Univ., Econ. Dept., Terre Haute, Ind. 47809
ROBERTSON, Robert L., Jr., Mount Holyoke Col., Dept. of Econ. and Soc., South Hadley, Mass. 01075
ROBERTSON, Ronald G., 921-F Cherry Lane, East Lansing, Mich.
ROBERTSON, Ross M., Indiana Univ., Sch. of Bus., Bloomington, Ind.
ROBEY, Ralph W., 2926 Garfield St., N.W., Washington 8, D.C.
ROBICHEK, Alexander A., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 94305
ROBINS, Glenn A., 1914 N. 27th, Sheboygan, Wis. 53081
ROBINS, William R., American Can Co., 100 Park Ave., New York, N.Y. 10017
ROBINSON, Albert J., Econ. Dept., York Univ., 2275 Bayview Ave., Toronto, Ont., Canada
ROBINSON, Bruce N., Texas Southern Univ., Dept. of Econ., P.O. Box 93, Houston, Tex. 77004
ROBINSON, Derek, Inst. of Econ. and Statis., Manor Rd., Oxford, England
ROBINSON, Dwight E., Univ. of Washington, Col. of Bus. Admin., Seattle 5, Wash.
ROBINSON, E. Allen, 2462 72nd Ave., S.E., Mercer Island, Wash. 98040
ROBINSON, Frederic A., 167 E. 67th St., Apt. 10C, New York 21, N.Y.
ROBINSON, Herbert W., 7830 Persimmon Tree Lane, Bethesda, Md. 21811
ROBINSON, Jack L., Univ. of Oklahoma, Dept. of Econ., Norman, Okla.
ROBINSON, James M. A., 233 Eastcourt Rd., Oakville, Ont., Canada
ROBINSON, James W., Univ. of Missouri, Dept. of Econ., B and PA, Columbia, Mo.
ROBINSON, Jerald F., Univ. of Missouri, Dept. of Econ., Columbia, Mo. 65202
ROBINSON, (Mrs.) Joan, Girton Col., Cambridge, England
ROBINSON, Joe A., USOECD Amer. Embassy, APO New York 09777
ROBINSON, Leland R., Stoneleigh One, Bronxville 8, N.Y.
ROBINSON, Marshall A., Econ. Dev. and Admin. Program, Ford Found., 477 Madison Ave., New York, N.Y. 10022
ROBINSON, Mary E., 1320 31st St., N.W., Washington 7, D.C.
ROBINSON, Myles E., 11029 Waycroft Way, Wickford, Rockville, Md.
ROBINSON, Newton Y., Box 824, Alfred, N.Y. 14802
ROBINSON, Oliver L., 5951 Grand River Dr., Ada, Mich. 49301
ROBINSON, Robert M., 4641 Nickels Way, Sacramento, Calif. 95825

ROBINSON, Roland I., Michigan State Univ., Eppley Center, East Lansing, Mich.
ROBINSON, Romney, Garfield Rd., Concord, Mass 01742
ROBINSON, Warren C., Pennsylvania State Univ., Dept. of Econ., State College, Pa.
ROBINSON, William R., 576 Herbert St., El Cajon, Calif. 92020
ROBOCK, Stepfan H., Indiana Univ., Sch. of Bus., Box B, Bloomington, Ind.
ROBOZ, Otto, 1101 New Hampshire Ave., N.W., Washington 6, D.C.
ROBSON, R. Thayne, 3310 Weller Rd., Silver Spring, Md. 20906
ROCA, Ruben A., 2121 P St., N.W., Washington, D.C. 20007
ROCHA, Joseph R., Jr., Ball State Univ., Dept. of Econ., Muncie, Ind. 47306
ROCK, James M., 4359 Americana Dr. Apt. 203, Annandale, Va. 22003
ROCK, Vincent P., 10411 Greenacres Dr., Silver Spring, Md. 20903
ROCKAFELLOW, Robert, Rhode Island State Col., Kingston, R.I. 02836
ROCKWELL, Charles S., Economic Growth Center, 52 Hillhouse Ave., New Haven, Conn.
ROCKWOOD, Charles E., Florida State Univ., Dept. of Econ., Tallahassee, Fla.
RODD, R. Stephen, Ontario Agric. Col., Dept. of Agric. Econ., Guelph, Ont., Canada
RODDENBERY, Thaddeus H., Worcester Poly. Inst., Dept. of Econ. and Gov't., Worcester, Mass.
RODES, Milton M., 11032 Burnley Ter., Silver Spring, Md.
RODGERS, John M., 3301 Kenyon Ave., Baltimore, Md. 21213
RODRIGUES, Max, Univ. de los Andes, Facultad de Econ., Apt. Aeyeo #4976, Bogota, D.E., Colombia
RODRIGUES, Romulo I. J. L., Bairro de Belem, R.13-No. 129, Lisbon 3, Portugal
RODRIGUEZ, Emilio L., 10855 Peach Tree Dr., Miami, Fla. 33161
RODRIGUEZ, Ireneo T., Luis A. de Herrera # 1.346, Asuncion, Paraguay
RODRIGUEZ, Justine F., 254 Millwood Rd., Chappaqua, N.Y. 10514
RODRIGUEZ, Louis J., 207 Darolina St., Thibodaux, La. 70301
RODRIGUEZ, Luis A., Naciones Unidas CEPAL, Hamburgo 63, Mexico 6, D.F., Mexico
RODRIGUEZ, Miguel A., 950 Riley Dr., Albany 10, Calif.
RODRIGUEZ-LOPEZ, Carlos A., 9a. Calle # 4-30, Zona 1, Guatemala City, Guatemala
RODWIN, Lloyd, Massachusetts Inst. of Tech., Sch. of Architecture and Plan., 77 Massachusetts Ave., Cambridge, Mass. 01922
ROE, Stanley S., Auto Mfgrs. Assoc., Statis. Dept., 320 New Center Bldg., Detroit, Mich. 48202
ROERINK, Garret W., AnderClay Insurance Mgrs., Inc., Houston, Tex. 77001
ROESLER, Theodore W., Univ. of Nebraska, Econ. Dept., Lincoln 8, Neb.
ROESTI, Robert M., Sacramento State Col., Bus. Admin. Div., 6000 J St., Sacramento, Calif. 95819
ROETS, Perry J., Marquette Univ., Milwaukee, Wis.
ROFE, Robert L., Sydney Grammar Sch., College St., Sydney, Australia
ROFMAN, Alejandro B., Urquiza 1667, Rosario, Argentina
ROGALSKI, Thomas R., 1924 Newburg Rd., Louisville, Ky. 40205
ROGERS, Andrei, 1046 Middlefield Rd., Berkeley, Calif.
ROGERS, Augustus J., III, 1313-H Univ. Village, East Lansing, Mich. 48823
ROGERS, Beatrice A., Mass. Inst. of Tech., 50 Memorial Dr., Cambridge, Mass. 01922
ROGERS, Clayton T., Jr., 440 Steeplechase Lane, St. Louis, Mo. 63131
ROGERS, Daniel C., 1341 Chapel St., B-1, New Haven, Conn. 06511
ROGERS, George B., 8303 Verona Dr., Hyattsville, Md.
ROGERS, George W., 1790 Evergreen Ave., Juneau, Alaska 99801
ROGERS, H. Barrett, Northwestern Univ., Commerce Bldg., Evanston, Ill. 60201
ROGERS, Jack, Univ. of Calif., 310 Barrows Hall, Berkeley 4, Calif.
ROGERS, John D., 12934 Louise St., Garden Grove, Calif. 92641
ROGERS, Lawrence W., Jr., 2010 Peabody St., West Hyattsville, Md. 21794
ROGERS, Robert C., 1003 S. Country Club Dr., La Porte, Tex. 77571
ROGERS, Robert O., 2348 N. Early St., Alexandria, Va.
ROGERS, Terry E., 62 Wiggins St., Princeton, N.J. 08540
ROGERS, Thomas W., 4701 Churchwood Dr., Village of Oak Hill, Nashville 4, Tenn.
ROGGE, Benjamin A., 406 S. Water St., Crawfordsville, Ind. 47933
ROGIN, William E., 2785 Shasta Rd., Berkeley, Calif. 94708
ROGOW, Robert, Soc. Sci. Dept., Michigan State Univ., E. Lansing, Mich.
ROGSTAD, Barry K., Brown Univ., Dept. of Econ., Providence, R.I.
ROHR, David B., 694-1 Fairview Ave., Takoma Park, Md. 20012
ROHRLICH, George F., Univ. of Chicago, Sch. of Soc. Serv. Admin., Chicago, Ill. 60637
ROHRS, Walter F., 81-06 34th Ave., Jackson Heights, N.Y. 11372
ROHT, Jaanus, 315 Webster St., Marshfield, Mass. 02050
ROJKO, Anthony S., 1717 Macon St., McLean, Va. 22101
ROLFE, Cecil J., Southern Missionary Col., Collegedale, Tenn. 37315
ROLLER, George H., 8806 Flower Ave., Silver Spring, Md.
ROLLINS, Mabel A., 319 Mitchel St., Ithaca, N.Y.
ROLLMAN, Howard E., Northern Natural Gas Co., 2223 Dodge St., Omaha 2, Neb.
ROLNICK, R. Lillian, 315 Eighth Ave., New York, N.Y. 10001
ROLPH, Earl R., Univ. of California, Dept. of Econ., Berkeley, Calif.
ROLSTON, Reuel Y., 1518 N. 12th St., Arlington, Va.
ROMAIN, Louis A., 421 Crown St., Brooklyn, N.Y. 11225
ROMAN, Daniel D., American Univ., Sch. of Bus., Washington 16, D.C.
ROMAN, (Mrs.) Delores K., 4009 Lindell, Apt. 203, St. Louis, Mo. 63108
ROMAN, Eduardo, Walnut Grove, Trailer Ct. # 105, Bloomington, Ind. 47403
ROMAN, Paul D., 4009 Lindell, Apt. 203, St. Louis, Mo. 63108
ROMANO, Richard M., 52 Orton Ave., Binghamton, N.Y. 13905
ROMANS, J. Thomas, Univ. of Buffalo, Dept. of Econ., Buffalo, N.Y.
ROMERO, Jose, Jr., Econ. Dev. Found., Manila, Philippines
ROMERO-KOLBECK, Gustavo, Texas 118, Mexico D.F. 18, Mexico
ROMINE, (Mrs.) Laura E., 990 Ivycroft Rd., Wayne, Pa.
ROMITA, Joseph W., 208 S. Elm St., Apt. A, Greenville, N.C. 27834
RONALL, Joachim O., 333 Central Park W., New York 25, N.Y.
RONDEAU, Claude, 39 St. Louis, ch. 300, Quebec, P.Q., Canada
RONIGER, George P., 4410 Briarwood Ct., Annandale, Va. 22003
RONK, (Mrs.) Sally S., 7 W. 14th St., Apt. 19C, New York, N.Y. 10011
ROONEY, Robert F., 2143 Vallecito Dr., San Pedro, Calif.
ROOSA, Robert V., Brown Brothers Harriman and Co., 59 Wall St., New York, N.Y. 10005
ROOSE, Kenneth D., 691 Westerly Parkway, State College, Penn. 16801
ROOT, Franklin R., Univ. of Pennsylvania, Wharton Sch., Dept. of For. Com. and Mktg., Philadelphia 4, Pa.
ROOT, Norman, 11629 Lockwood Dr., Silver Spring, Md. 20904
ROPER, Burkhardt, 5 Kupferatrasse, Aachen 1, Germany
ROPER, Elmo, C.R.R.3, West Redding, Conn. 06896
ROREM, C. Rufus, Hospital Rev. and Plan. Council, 3 E. 54th St., New York, N.Y. 10022

ROSA, Joseph, 3503 Rodman St., N.W., Washington, D.C. 20008
ROSCHE, Jules T., 330 E. 46th St., Apt. 2K, New York, N.Y. 10017
ROSCOE, William L., Jr., 13459 Diena Dr., Warren, Mich 48093
ROSE, Bernard J., III, 123 Oak St., Woodmere, N.Y. 11598
ROSE, Harold B., London Sch. of Econ. and Polit. Sci., London W.C.2, England
ROSE, Hugh, Univ. of Rochester, Dept. of Econ., Rochester, N.Y. 14627
ROSE, Joseph R., W 283 Dietrich Hall, 3620 Locust St., Philadelphia 4, Pa.
ROSE, Louis A., Univ. of California, Dept. of Econ., Los Angeles 24, Calif.
ROSE, Malcolm L., 638 Fairview Ave., Takoma Park, Md. 20012
ROSE, (Mrs.) Norman Feinn, 47 Hall Dr., Orange, Conn.
ROSE, Susan G., 736 Orange St., New Haven, Conn.
ROSE, Warren, Univ. of Houston, Col. of Bus., Dept. of Trans., Houston 4, Tex.
ROSE, William M., 4821 Campbell, Kansas City, Mo. 64110
ROSEBERY, Frank D., 2737 Devonshire Pl., Washington, D.C. 20008
ROSEGGER, Gerhard, Case Inst. of Tech., Dept. of Mgt., Cleveland 6, Ohio
ROSEMAN, Frank, University of Western Ontario, Dept. of Econ. and Soc., London, Ont., Canada
ROSEMAN, M. Richard, 244 Carlo Dr., Goleta, Calif. 93107
ROSEN, Daniel L., 72-81 113th St. Apt. 7W, Forest Hills, N.Y. 12068
ROSEN, George, c/o The RAND Corp., 1700 Main St., Santa Monica, Calif.
ROSEN, Harold H., Slippery Rock State Col., Slippery Rock, Pa. 16057
ROSEN, Howard, 5204 Wyoming Rd., Glen Echo Hgts., Washington, D.C. 20016
ROSEN, Lewis J., 30 Orchard Pl., New Haven, Conn. 06511
ROSEN, Martha O., Abt Asso., 14 Concord Lane, Cambridge, Mass. 02138
ROSEN, Martin M., Int. Fin. Corp., 1818 H St., N.W., Washington, D.C. 20433
ROSEN, Norman E., 254 G St., S.W., Washington, D.C. 20024
ROSEN, Sam, Univ. of New Hampshire, Dept. of Econ., Durham, N.H. 03824
ROSEN, Sherwin, Univ. of Rochester, Dept. of Econ., Col. of Arts and Sci., Rochester, N.Y. 14627
ROSEN, Sol Z., 1718 Eye St., N.W., Washington, D.C.
ROSENBAUM, Edward, 8504 16th St., Apt. 416, Silver Spring, Md. 20910
ROSENBERG, Henry S., Ste.116, 119 York St., Toronto 1, Ont., Canada
ROSENBERG, Laurence C., 3320 Hartwell Ct., Falls Church, Va. 22042
ROSENBERG, Leon J., Sanger Harris, Dallas, Tex. 75202
ROSENBERG, Leonard G., Surveys and Res. Corp., 1030 15th St., N.W., Washington, D.C. 20005
ROSENBERG, Marvin, 542 W. 112th St., New York, N.Y. 10025
ROSENBERG, Nathan, Purdue Univ., Dept. of Econ., Lafayette, Ind.
ROSENBERG, Richard, Pennsylvania State Univ., Dept. of Econ., Boucke Bldg., University Park, Pa. 17774
ROSENBERG, Samuel A., Wilkes Col., Wilkes-Barre, Pa. 18703
ROSENBERG, Steven P., 249 Oberlin Ave., Claremont, Calif.
ROSENBLATT, Bernard, 325 Damascus Dr., Manassas, Va. 22111
ROSENBLATT, Samuel M., 7106 Pomander Lane, Chevy Chase, Md. 20015
ROSENBLUM, Harvey, 6706 Arrego Rd., Apt. 2, Goleta, Calif. 93017
ROSENBLUTH, Gideon, Univ. of British Columbia, Dept. of Econ., Vancouver 8, B.C., Canada
ROSENDORN, Robert J., 9059 W. Swan Circle, Brentwood 44, Mo.
ROSENSON, Alexander M., 6301 Dahlonega Rd., Washington 16, D.C.
ROSENTHAL, Arnold J., 8 Ford Hill Rd., Whippany, N.J. 07981
ROSENTHAL, Gerald D., 106 Lorimer Rd., Belmont, Mass. 02178
ROSENTHAL, Irving, c/o Guardian Life Ins. Co., 201 Park Ave. S., New York 3, N.Y.
ROSENTHAL, Joseph, 3602 Delano St., Wheaton, Md. 20902
ROSENTHAL, Lewis D., 15 Stuyvesant Oval, New York 9, N.Y.
ROSETT, Richard N., Univ. of Rochester, Dept. of Econ., Rochester, N.Y. 14627
ROS-JIMENO, Jose, San Francisco De Sales, 7, Madrid 3, Spain
ROSKAM, Bill, P.O. Box 910, Burbank, Calif.
ROSKAMP, Karl W., Wayne State Univ., Detroit 2, Mich.
ROSKO, Peter, Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
ROSLER, Alice, 2527 E. 76th St., Chicago, Ill. 60649
ROSOFF, Eric, 3718 Lovers Lane, Dallas, Tex. 75225
ROSOFF, Peter, 240 E. 76th St., Apt. 16S, New York, N.Y. 10021
ROSOVSKY, Henry, Harvard Univ., Dept. of Econ., Cambridge, Mass. 02138
ROSS, Albert, 3938 45th St., Long Island City, Queens, N.Y.
ROSS, David F., Cuttington Col., Bishop's House, Monrovia, Liberia
ROSS, David P., Dept. of Labor, Econ. and Res. Br., Sir Wilfrid Lauries Bldg., Rm. 1020, Ottawa 4, Ont., Canada
ROSS, Edwin B., 2099 W. 11th Ave., Vancouver 9, B.C., Canada
ROSS, Emerson, 5911 Onondaga Rd., Washington, D.C. 20016
ROSS, Harold G., 43 Surrey Rd., Stamford, Conn.
ROSS, Howard N., 360 West 22nd St., New York, N.Y. 10011
ROSS, Ira N., 66-22 Fleet St., Apt. 6-V, Forest Hills, N.Y. 11375
ROSS, James A., Jr., Willow Lane, Wiscasset, Me. 04578
ROSS, John B., 2671 46th Ave., San Francisco, Calif. 94116
ROSS, Marion, Mills College, Oakland, Calif. 94613
ROSS, Murray, 3540 Van Ness St., N.W., Washington 16, D.C.
ROSS, Myron H., 425 Coolidge Ave., Kalamazoo, Mich. 49007
ROSS, O. William, 531 E. 20th St., Apt. 11-E, New York, N.Y. 10010
ROSS, (Mrs.) Paula P., 425 Coolidge Ave., Kalamazoo, Mich. 49007
ROSS, Peter, 1044 Errol Rd., E., Sarnia, Ont., Canada
ROSS, Ray, Western Illinois Univ., Dept. of Bus., Macomb, Ill. 61455
ROSS, Robert P., East Tenn. State Univ., Substa. Box 2376, Johnson City, Tenn. 37602
ROSS, William B., 619 Pulman Pl., Alexandria, Va. 22305
ROSS, William D., Louisiana State Univ., Col. of Bus. Admin., Baton Rouge, La.
ROSSMILLER, Richard A., 5806 Cable Ave., Madison 5, Wis.
ROSTKER, Bernard D., 303 W. 66th St., New York, N.Y. 10023
ROSTVOLD, Gerhard N., Pomona Col., Dept. of Econ., Claremont, Calif.
ROTH, (Mrs.) Rosalind, 89-04 68th Ave., Forest Hills, N.Y. 11375
ROTH, Henry A., Jr., 2456 W. 229th St., Torrence, Calif. 90501
ROTH, Timothy P., 1007 Perry St., Reading, Pa.
ROTHBARD, Murray N., 215 W. 88th St., Apt. 2E, New York 2, N.Y.
ROTHBAUM, Melvin, Univ. of Illinois, Inst. of Labor and Ind. Rela., 504 E. Armory Ave., Champaign, Ill. 61822
ROTHBERG, Herman, 11910 Judson Rd., Silver Spring, Md.
ROTHENBERG, Jerome, Northwestern Univ., Dept. of Econ., Evanston, Ill. 60201
ROTHENBERG, Thomas J., Northwestern Univ., Dept. of Econ., Evanston, Ill.

ROTHSCHILD, Michael, 34 Cambridge Ter., Cambridge, Mass. 02140
ROTHSTEIN, Sheldon H., 295 West 11th St., New York 14, N.Y.
ROTHWELL, Kenneth J., Univ. of New Hampshire, Econs. Dept., Durham, N.H. 03824
ROTONDARO, Roberto H., Cachimayo 988, Buenos Aires, Argentina
ROTTENBERG, Simon, Duke Univ., Dept. of Econ., Durham, N.C.
ROTWEIN, Eugene, Queens Col., Dept. of Econ., Flushing 67, N.Y.
ROUMASSET, Charles A., 151 Wildwood Ave., San Carlos, Calif
ROURKE, Blair, Stanford Univ., Food Res. Inst., Stanford, Calif. 96128
ROUSE, Robert L., Texas Tech. Col., Sch. of Bus. Admin., Lubbock, Tex.
ROUSH, James L., Stu. Det. USAWC, Carlisle Barracks, Pa. 17013
ROUSSAKIS, Emmanuel N., 124-B Queen Sophia St., Athens 609, Greece
ROUSSEAS, Stephen W., New York Univ., Dept. of Econ., Washington Sq., New York, N.Y. 10003
ROVNER, Louise M., 4000 Tunlaw Rd., N.W., Apartment 806, Washington 7, D.C.
ROW, Clark, 605 Monroe House, 522 21st St., N.W., Washington, D.C. 20006
ROWE, Benjamin, 150 Riverside Dr., New York 24, N.Y.
ROWE, Harold B., 5815 Highland Dr., Chevy Chase, Md. 20015
ROWE, J. Z., 10140 Rockmoor Ct., Dallas, Tex. 75229
ROWE, William H., 3073 S. Buchanan St., Apt. A-2, Arlington, Va. 22206
ROWLAND, James P., Chestnut Hill Col., Dept. of Econ., Philadelphia 18, Pa.
ROWLAND, Theodore L., 50 E. 96th St., New York, N.Y. 10028
ROWLANDS, David T., Univ. of Penn., W-108 Dietrich Hall. Philadelphia 4, Pa.
ROWLEY, Charles K., Univ. of Kent, Eliot Col., Canterbury, Kent, England
ROWLEY, Jennifer A., Univ. of Connecticut, Dept. of Econ., Storrs, Conn. 06041
ROWNTREE, John T., Jr., 1822 Virginia, Berkeley, Calif. 94703
ROXAS, Sixto K., 206, Far East Bldg., Buendia Avenue, Makati, Rizal, Philippines
ROY, Delwin A., AID, APO New York 09254
ROY, P. Noel, 3301 St. Paul St., Baltimore, Md. 21218
ROYCE, Gerald A., MacMurray Col., Jacksonville, Ill.
ROYLE, John E., Middleburgh Heights, 16515 Elderdale Dr., Cleveland 30, Ohio
ROZEN, Marvin E., Pennsylvania State Univ., Dept. of Econ., Boucke Bldg., University Park, Pa. 17774
ROZENTAL, Alek A., USOM, APO San Francisco 96346
ROZNER, Morris, 5532 Tellina Way, Santa Barbara, Calif. 93105
RUBEN, Robert J., 4 Heritage Lane, Rye, N.Y. 10580
RUBIN, Claire B., 325 E. 41st St., New York 17, N.Y.
RUBIN, Edward P., 135 S. La Salle St., Rm. 2240, Chicago, Ill. 60603
RUBIN, Harold, 156 Chestnut St., Albany, N.Y. 12210
RUBIN, Israel I., 4 Godfrey Rd., Upper Montclair, N.J. 07087
RUBIN, Julius, 2373 Eldridge St., Pittsburgh, Pa. 15217
RUBIN, Milton, 54 Sunset Dr., Croton-on-Hudson, N.Y. 10520
RUBIN, Nathan, 19 Parkwood Lane, Penfield, N.Y. 14625
RUBIN, Paul H., 26-5 Ross Ade Dr., West Lafayette, Ind.
RUBIN, Samuel, 24 Monroe Pl., Apt. #10-A, Brooklyn Heights, N.Y. 11201
RUBIN, Seymour J., Surrey, Karasik, Gould, and Greene, Woodward Bldg., Washington, D.C. 20006
RUBINSON, Jacob, 11 McKeever Pl., Brooklyn, N.Y. 11225
RUBINYI, Paul, 183 Trenton Ave., Town of Mt. Royal, Montreal 16, P.Q., Canada
RUCHLIN, Hirsch S., 6407 Boulevard East, West New York, N.J. 07093
RUCKDESCHEL, Fred B., Bd. of Gov. of the Fed. Res. System, Div. of Int. Fin., Washington, D.C. 20551
RUCKER, Henry C., III, American Univ., Box 227, Washington, D.C. 20016
RUCKER, Neville L., Univ. of North Carolina, Sch. of Bus. Admin., Dept. of Econ., Carroll Hall, Chapel Hill, N.C. 27515
RUCKER, T. Donald, 3505 Hutch Pl., North Chevy Chase, Md. 20015
RUDBERG, Lawrence E., 5459 Streefkerk Dr., Warren, Mich. 48092
RUDEL, Ludwig, USAID, APO New York 09675
RUDERMAN, Armand P., 11006 Montrose Ave., Garrett Park, Md. 20766
RUDESEAL, Lillian C., Mary Baldwin Col., Staunton, Va.
RUDMAN, Leon M., 318 Hayes Circle, Fort Ord, Calif. 93941
RUDNEY, Gabriel G., 3506 Livingston St., N.W., Washington, D.C. 20015
RUDOLPH, Walter M., 9622 Lawndale Ct., Silver Spring, Md. 20901
RUFF, Gunther H., Georgetown Univ., Dept. of Econ., Washington, D.C. 20007
RUFF, Samuel O., 1200 S. Court House Rd., Arlington, Va. 22204
RUFFING, Kenneth G., 610 W. 115th St., No. 35, New York, N.Y. 10025
RUFFNER, Frederick G., Jr., 1400 Book Tower, 1249 Washington Blvd., Detroit, Mich. 48226
RUFINO, Emmanuel A., 1111 Arlington Blvd., Apt. M-617, Arlington, Va. 22209
RUGGLES, Richard, 88 Prospect St., New Haven, Conn.
RUGGLES, Robert T., Anthes Imperial Limited, Box 67, St. Catherines, Ont., Canada
RUGINA, Anghel, 145 Moss Hill Rd., Jamaica Plain, Mass. 02344
RUIBAL-H., Alberto, Juan de la Fuente 444, Miraflores, Lima, Peru
RUIZ, Angel L., A.P. #806 Tegucigalpa, D.C., Honduras, C.A.
RUIZ-LARA, Jorge, Univ. de Los Andes, Apartado Aereo 4976, Bogota, Colombia
RUKEYSER, Merryle S., 150 W. Pinebrook Dr., New Rochelle, N.Y.
RUMAYOR, Armand C., 23 Cary St., Binghamton, N.Y. 13901
RUMBLE, John C., Courier-Journal and Louisville Times, Louisville 2, Ky.
RUMMEL, Oscar W., 54 Washington Ave., Ft. Thomas, Ky. 41075
RUMMEL, Philip, 500 E. 33rd St., Apt. 1206, Chicago, Ill. 60616
RUNGELING, Brian, Univ. of Texas, Dept. of Econ., Austin, Tex. 78712
RUNKLE, Jerry C., Lamoni, Iowa 50140
RUNYON, Herbert R., Fed. Res. Bank of San Francisco, Res. Dept., 400 Sansome St., San Francisco 20, Calif.
RUPLEY, Lawrence A., 1106 W. University, Urbana, Ill.
RUPP, Daniel G., St. Benedict's Col., Atchison, Kans. 66002
RUPPENTHAL, Karl M., 1711 Guinda St., Palo Alto, Calif.
RUPPERT, Richard W., Univ. of California Santa Barbara, Dept. of Econ., Goleta, Calif. 93017
RUPRECHT, Theodore K., Box 318, Trinidad, Calif. 95570
RUSCH, Helen, 542 N. Pine Ave., Chicago, Ill. 60644
RUSER, Claus W., 3517 Leland St., Chevy Chase, Md. 21620
RUSIACKAS, James M., 43-25 Douglaston Pkwy., Douglaston, N.Y.
RUSINKO, Daniel J., 14 Briarheath, Englishtown, N.J. 07726
RUSK, G. Gareld, Sam Houston State Col., Huntsville, Tex.
RUSSELL, Brother Cornelius J., Marist Col., Poughkeepsie, N.Y. 12601
RUSSELL, Don J., 112-20 72nd Dr. Apt. D29, Forest Hills, N.Y. 11375
RUSSELL, J. Johnson, III, 700 Westmont Dr., Alhambra, Calif.
RUSSELL, Joe L., 3625 Barcroft View Ter., 103, Falls Church, Va. 22041

RUSSELL, Keith P., II, 244 S. Irving Blvd., Los Angeles, Calif. 90004
RUSSELL, Milton, Southern Illinois Univ., Dept. of Econ., Carbondale, Ill.
RUSSELL, P. Dean, Gray Rock Ter., Irvington, N.Y. 10533
RUSSELL, R. Robert, Council of Econ. Advisers, Exec. Off. of the President, Washington, D.C. 20500
RUSSELL, Ralph, 2930 Legation St., N.W., Washington 15, D.C.
RUSSELL, Vera K., 56 Groesbeck Pl., Delmar, N.Y. 12054
RUSSELL, William R., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
RUSSILLO, Paul T., 10904 Montrose Ave., Garrett Park, Md. 20766
RUSSO, Daniel M., 323 Cooper Lane, University Hgts., Piscataway, N.J. 08854
RUST, Albert O., 113 Weston Ave., Chatham, N.J. 07928
RUTH, Richard L., Hobart and William Smith Col., Geneva, N.Y. 14456
RUTMAN, Gilbert L., 1722 E. 7th St., Tucson, Ariz.
RUTTAN, Vernon W., Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
RUTTENBERG, Stanley H., 6310 Maiden Lane, Bethesda, Md. 21811
RYAN, Christopher M., Methodist Col., Fayetteville, N.C. 28301
RYAN, Edward W., 43 Ave. B, Bayonne, N.J. 07002
RYAN, Frederick L., San Diego State Col., Econ. Dept., San Diego, Calif.
RYAN, James J., 715 Ocean Pkwy., Brooklyn, N.Y. 11230
RYAN, John M., Standard Oil Co., N.J., 30 Rockefeller Plaza, New York, N.Y. 10020
RYAN, Robert J., Jr., c/o Amer. Embassy, APO New York 09676
RYAN, Robert M., Wichita State Univ., Econ. Dept., Wichita, Kans. 67208
RYAN, Walter F., Off. of Statis. Stan., Exec. Off. of the President, Bur. of the Budget, Washington, D.C. 20503
RYAN, William F., Loyola Col., Montreal, Que., Canada
RYANS, John K., Jr., Univ. of Maryland, Col. of Bus. and Public Admin., Dept. of Bus. and Pub. Admin., College Park, Md. 20742
RYBINSKI, Helga, Finckensteinallee 7, 1 Berlin 45, Germany
RYDER, Harl E., Jr., Brown Univ., Dept. of Econ., Providence, R.I. 02912
RYDORFT, Bengt-Ake, Ulrikedalsv. 4-E, Lund, Sweden
RYLE, (Mrs.) Patricia, 1401 Lake Ave., Apt. A-2, Metairie, La. 70005
RYNIKER, Richard C., 4504 Amherst Lane, Bethesda, Md. 20014
RYTER, Lyle, 9314 19th Ave., Adelphia, Md. 21710

SA, Nguyen Phuc, Deputy Dir. Gen. of Plan., Ben Chuong Duong, Saigon, Vietnam
SAALBACH, William F., Washington and Jefferson Col., Washington, Pa. 15301
SABA, Fauzi F., Box 4068, Beirut, Lebanon
SABATELLA, Raymond F., Jr., Hartford Nat. Bank and Trust Co., Hartford, Conn. 06115
SABATINO, Richard, Univ. of Rhode Island, Kingston, R.I. 02836
SABEN, Samuel, 2905 Radius Rd., Silver Spring, Md. 20902
SABHARWAL, Siri R., 2489 Nelwa St., Paharganj, Chuna Mandi, New Delhi, India
SACHS, William S., Gerdes Rd., New Canaan, Conn. 06840
SACKETT, Stanley M., First Nat. Bank of Oregon, Br. and Trade Area Dev. Dept., P.O. Box 3131, Portland, Ore. 97028
SACKRIN, Seymour M., 816 Talbert Lane, Hyattsville, Md. 20783
SACKS, (Mrs.) Barbara, 210 Edgemont Dr., Syracuse 3, N.Y.
SACKS, Seymour, 210 Edgemont Dr., Syracuse 3, N.Y.
SADIKLAR, Tayyar C., Ministry of Plan., Maliye Mufettisi, Ankara, Turkey
SADNICKI, Bronislaw, Ferry Rd., Doylestown, Pa. 18901
SA DOS-SANTOS, Adylson, R. Padre Roma 650, B. de Fatima, Fortaleza Ce., Brazil
SADOVE, A. Robert, 3402 Saul Rd., Rock Creek Hills, Kensington, Md. 20795
SADOWSKY, George, The Brookings Inst., 1775 Massachusetts Ave., N.W., Washington, D.C. 20036
SADTLER, William B., 73 Knoll St., Roslindale, Boston, Mass.
SAFARIAN, Albert E., Univ. of Saskatchewan, Dept. of Econ. and Pol. Sci., Saskatoon, Sask., Canada
SAFFRAN, Bernard, Univ. of California, Dept. of Econ., Berkeley, Calif.
SAFFRAN, H. William, 388 Post Ave., Rochester 19, N.Y.
SAGAN, John, 22149 Long Blvd., Dearborn, Mich.
SAGGESE, Arnold, 901 Dewhurst, No. 216, San Antonio, Tex. 78213
SAGRAVES, Ronald, 1430 Broadway, Pella, Iowa 50219
SAHNI, Balbir S., 3455 Aylmer St., Apt. 608, Montreal 18, P.Q., Canada
SAHOTA, Gian S., Vanderbilt Univ., Econ. Dept. and Bus. Admin., Nashville, Tenn. 37203
SAIDI, Ahmad, P.O. Box 4 Heathcote Sta., Scarsdale, N.Y. 10585
SAILORS, Joel W., Univ. of Houston, Dept. of Econ., Cullen Blvd., Houston, Tex.
SAINI, Krishan G., c/o Foreign Student Adviser, New York 27, N.Y.
SAINI, TejBhan S., 455 Seminary St., Napa, Calif.
ST. JOHN, Norman, 2541 Belmont, Salina, Kans
ST. LAURENT, Jacques, Univ. Laval, Faculte des Sci. Soc., Ste. Foy, Que., Canada
SAJJADI, Peter S., 620 New York Ave., Creston, Iowa 50801
SAKAMOTO, Jorge, 256 Edwards St., New Haven, Conn. 06511
SAKBANI, Mahmoud, 255 W. 12th St., Apt. 9, New York, N.Y. 10014
SALANT, Aaron B., 23 E. 74th St., New York, N.Y. 10021
SALANT, Nathan B., 4201 Cathedral Ave., Washington, D.C. 20016
SALANT, Walter S., 3921 Idaho Ave. N.W., Washington 8, D.C.
SALANT, William A., 2 Terrace Lane, Chico, Calif.
SALDANHA, Charles A., Hindu Col., Dept. of Econ. Hist. and Pol., Nagercoil 2, S. India
SALE, T. S., III, P.O. Box 509, Haynesville, La. 71038
SALERA, Virgil, 3175 Cromwell Pl., Hayward, Calif.
SALGADO, Herbert M., Brazilian Coffee Inst., 120 Wall St., New York 5, N.Y.
SALIM, Agoes, 534 Jalan Masjid, Port Dickson, Malaysia
SALISBURY, James, Jr., 3003 Arlington Blvd., Arlington, Va. 22201
SALKEVER, Louis R., 93 Jordan Blvd., Delmar, N.Y. 12054
SALMON, Raphael J., Res. Triangle Inst., P.O. Box 490, Durham, N.C.
SALSBURG, Sidney W., 17230 Lee St., Southfield, Mich.
SALT, Allan F., 8002 Gosport Lane, Springfield, Va. 22151
SALTZMAN, Arthur W. 30630 Woodside Dr., Franklin, Mich. 48025
SALZMAN, Lawrence, 8 Mileview Ave., White Plains, N.Y. 10606
SAMETZ, Arnold W., New York Univ., Grad. School of Bus. Admin., 100 Trinity Pl., New York 6, N.Y.
SAMLI, A. Coskun, Southern Illinois Univ., Bur. of Bus. Res., Carbondale, Ill. 62903
SAMMONS, Robert L., 812 Tanley Rd., Silver Spring, Md.
SAMOORE, Arthur S., 135 Westminster, Jacksonville, Ill.
SAMPER, Luis E., Calle 75 No. 12-44, Bogota, Colombia
SAMPSON, Roy J., Univ. of Oregon, Sch. of Bus. Admin., Eugene, Ore.

SAMUELS, L. H., Univ. of the Witwatersrand, Dept. of Com. and Applied Econ., Jan Smuts Ave., Johannesburg, S. Africa
SAMUELS, Norman J., 3900 Tunlaw Rd. N.W., Washington 7, D.C.
SAMUELS, Warren J., Univ. of Miami, Dept. of Econ., Coral Gables, Fla. 33926
SAMUELSON, Paul A., Mass. Inst. of Tech., Rm. 52-383, Cambridge, Mass. 01922
SANBORN, Henry N., Towson State Col., Towson, Md. 21204
SANCETTA, Anthony L., Col. of William and Mary, Dept. of Econ., Williamsburg, Va. 23185
SANCHEZ, Luis R., Plan. Res. Corp., 1875 Connecticut Ave. at T St., Washington, D.C. 20009
SANDAGE, C. H., Univ. of Illinois, 120-A Gregory Hall, Urbana, Ill.
SANDBERG, Lars G., 18 Robinson St., Cambridge, Mass. 02138
SANDELIN, G. Lincoln, The Westchester, 412-B, 4000 Cathedral Ave., N.W., Washington, D.C. 20016
SANDERS, Dean S., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48105
SANDERS, Donald P., Ohio State Univ., Div. of Res., 1775 S. College Rd., Columbus, Ohio 43210
SANDERS, Edward G., 175 Yale Sta., New Haven, Conn.
SANDERS, Thomas C., Fed. Res. Bank of Richmond, Richmond 13, Va.
SANDERSON, Fred Hugo, Apt. B-811, 3636 16th St., Washington 10, D.C.
SANDMEYER, Robert L., Oklahoma State Univ. Col. of Bus., Dept. of Econ., Stillwater, Okla. 74075
SANDOVAL, A. David, 518 Walter S.E., Albuquerque, N.M. 87102
SANDRETTO, Carmen L., Univ. of New Hampshire, ERS-RDED, USDA Dept. of Res. Econ., Morrill Hall, Durham, N.H. 03824
SANDS, Edith, 874 Carroll St., Brooklyn 15, N.Y.
SANFORD, Dorothy Z., 1222 Dore Ave., San Mateo, Calif.
SANGHA, Kehar, Old Dominion Col., Sch. of Bus. Admin., Norfolk 8, Va.
SANGREE, Gail M., 2871 Hampton Rd., Apt. 10, Cleveland, Ohio 44120
SANIMAN, Rais B., 2424 Pennsylvania Ave. N.W., #702, Washington, D.C. 20037
SANOW, Kenneth P., 6525 Elgin Lane, Bethesda, Md. 21811
SANSOM, Robert L., New Col., Oxford Univ., Oxford, England
SANTAMARINA, Everardo deJ., 132-48 41st Rd., Apt. 2-E, Flushing, N.Y. 11355
SANTIAGO, Francis X., Inst. Libre de Filosofia, Rio Hondo 1, Mexico 20 D.F., Mexico
SANTIAGO, Jose, 8010 Pilgrim, Apt. 6, Detroit, Mich. 48238
SANTORO, Vincenzina, 2015 Bruckner Blvd., New York, N.Y.
SAPIR, H. Michael, 6600 Melody Lane, Bethesda, Md. 20034
SAPOLSKY, Harvey M., Harvard Univ., Littauer 220, Cambridge, Mass. 02138
SAPONARA, Vincenzino, 3618 Webster St., San Francisco, Calif. 94123
SAPOSS, David J., 5701 Nebraska Ave., N.W., Washington 15, D.C.
SARACHEK, Bernard, Univ. of Missouri at Kansas City, Sch. of Bus. and Pub. Admin., 5100 Rockhill Rd., Kansas City, Mo.
SARAMES, George N., 10339 Keokuk Ave., Chatsworth, Calif. 91311
SARANTIDES, Romanos, 6 Neapoleos St., Agia Paraskevi, Attica, Greece
SARAYDAR, Edward, 2640 Dole St., Apt. C-8, Honolulu, Hawaii 96822
SARD, Edward L., 500 E. 77th St., Apt. 3215, New York, N.Y. 10021
SARDY, Hyman, 1249 E. 23rd St., Brooklyn 10, N.Y.
SARFATI, Joel R., 5999 Spring Hill Dr., Greenbelt, Md.
SARFATY, David E., Robert R. Nathan Asso., U. S. Embassy, San Salvador, El Salvador
SARGENT, Albert J., 337 Summer St., Manchester, Mass. 01944
SARGENT, Frank H., Norwich Univ., Dept. of Econ. and Bus. Admin., Northfield, Vt. 05663
SARGENT, Frederic O., Univ. of Vermont, Dept. of Agric. Econ., Morrill Hall, Burlington, Vt.
SARGENT, Noel, 12 Northfield Lane, Westbury, N.Y.
SARGENT, Thomas J., 100 Falmouth Rd. West, Arlington, Mass.
SARICH, B. Robert, 4525 Crest Lane, McLean, Va. 22101
SARKAR, Asit K., 1516 Hussion, Houston, Tex. 77003
SARKAR, Nihar K., United Nations, Rm. 2618 i, New York, N.Y. 10017
SARON, Elliot A., 215 W. 90th St., New York, 24, N.Y.
SAROS, Theodore A., D. Van Nostrand Co., Inc., 120 Alexander St., Princeton, N.J. 08540
SAROSI, Oliver, New Haven Col., Dept. of Bus. Admin., New Haven, Conn. 06505
SARPKAYA, S., The Canadian Bankers' Asso., 50 King St., W., Toronto 1, Ont., Canada
SARTORIUS, Lester C., Deere and Co., Econ. Res. Dept., John Deere Rd., Moline, Ill. 61265
SASAKI, Kyohei, Univ. of Hawaii, Col. of Bus. Admin., Honolulu, Hawaii
SASS, Frederick N., 6418 Lawnton Ave., Philadelphia, Pa. 19126
SASTRY, Vrudhula K., 147-68 Village Rd., Apt. 97-D, Jamaica, New York, N.Y.
SATO, Kazuo, United Nations, Rm. 2635, United Nations, N.Y. 10988
SATO, Ryuzo, Univ. of Hawaii, Dept. of Econ., Honolulu 16, Hawaii
SATOW, Philip M., 6711 Marlboro Rd., Washington, D.C. 20028
SAUER, Donald H., Indiana Univ., Sch. of Bus., Bloomington, Ind.
SAUERMANN, Heinz, Grafstr. 39 Pt., Frankfurt Main, Germany
SAULNIER, Raymond J., 410 Lehman Hall, Barnard Col., Columbia Univ., New York 27, N.Y.
SAUNDERS, Dwight D., Drake Univ., Dept. of Econ., Des Moines, Iowa 50311
SAUNDERS, George B., Syracuse Univ., Col. of Bus. Admin., 116 College Pl., Syracuse, N.Y. 13210
SAUNDERS, George S., 10 Kingsmill St., Ottawa 5, Ont., Canada
SAUNDERS, Gerald K., Missouri Valley Col., Marshall, Mo. 65340
SAUNDERS, Joseph L., 529 N. Clinton Ave., Trenton, N.J. 08638
SAUNDERS, (Mrs.) Laura Gilman, 2400 Daphne Lane, Alexandria, Va.
SAUNDERS, Philip, Jr., Winter St., R.F.D. 1, Lincoln, Mass. 01773
SAUNDERS, Phillip, Jr., 173 Morrison Dr., Pittsburgh, Pa. 15216
SAUNDERS, R. Duane, 2400 Daphne Lane, Alexandria, Va.
SAUNDERS, Robert J., West Virginia Univ., Col. of Com., Dept. of Econ., Morgantown, W. Va. 26506
SAUNDERS, S. Grant, 210 Montclair Ave., Upper Montclair, N.J. 07087
SAUNDERS, Stanley A., 56 St. Clair Ave., W., Apt. 6, Toronto 7, Ont., Canada
SAUNDERS, William B., 501 Cafritz Bldg., Washington 6, D.C.
SAUPE, William E., Univ. of Wisconsin, 316 Agric. Hall, Madison, Wis. 53706
SAUSE, George G., Jr., 622 W. Lafayette St., Easton, Pa. 18042
SAUSSY, Gordon, St. Marys Col., Saint Marys, Kans. 66536
SAVAGE, Christopher I., Univ. of W. Australia, Faculty of Econ. and Com., Nedlands, Western Australia
SAVAGE, James P., Jr., c/o David B. Goodstein, 630 Fifth Ave., New York, N.Y. 10020
SAVAGE, Steven A., 2314 19th St., N.W., Washington, D.C. 20009
SAVARESE, Ralph J., Howrey, Simon, Baker, and Murchison, 1707 H St., N.W., Washington D.C. 20006
SAVARY, James R., Univ. of Alberta, Dept. of Econ., Edmonton, Alta., Canada

SAVATE, Jack L., 505 Parkway Dr., Elmont, L.I., N.Y.
SAVILLE, Lloyd B., 1103 Anderson St., Durham, N.C.
SAVING, Thomas R., 1904 Oneida Dr., Okemos, Mich. 48864
SAWAYA, Charles P., P.O. Box 2194, Dhahran, Saudi Arabia
SAWHILL, John C., 500 Greenwood Rd., Baltimore, Md. 21204
SAWWAF, Husni A., P.O.B. 538, Damascus, Syria
SAWYER, Carole A., 2800 Wisconsin Ave., N.W., Apt. 101, Washington, D.C. 20007
SAWYER, John A., 118 Betty Ann Dr., Willowdale, Ont., Canada
SAWYER, John E., Williams Col., President's Off., Williamstown, Mass. 01267
SAWYER, (Mrs.) Virginia P., 118 Betty Ann Dr., Willowdale, Ont., Canada
SAX, Samuel W., Exchange Nat. Bank of Chicago, 130 S. La Salle St., Chicago, Ill. 60603
SAXE, J. W., c/o A.I.D., Dept. of State, Washington 25, D.C.
SAXL, Victor, 18 E. Elm St., 18 East Elm Apts., Chicago, Ill. 60611
SAXON, O. Glenn, Jr., 241 Old Hickory Rd., Fairfield, Conn. 06431
SAXONHOUSE, Gary R., 2 University Pl., New Haven, Conn.
SAYIGH, Yusif, American Univ. of Beirut, Beirut, Lebanon
SAYIN, Afife, Mgt. Mktg. and Bus. Statis., Hofstra Univ., Hempstead, L.I., N.Y.
SAYLES, Glenn, 1607 William Brewster, Irving, Tex. 75060
SAYRE, J. Woodrow, 20 Schuyler Rd., Loudonville, N.Y. 12211
SAZAMA, Gerald W., 608 S. Ingersoll St., Madison, Wis. 53703
SBAROUNIS, A. J., Athens Sch. of Econ. and Com. Sci., 12 Pandrossou St., Athens, Greece
SCALA, Aldo, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
SCAPERLANDA, Anthony E., Northern Illinois Univ., Dept. of Econ., DeKalb, Ill. 60115
SCARRY, Donald M., 369 Cherry Ave., Bound Brook, N.J. 08805
SCHAAF, Albert H., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
SCHAAKE, Richard, 36-10 206th St., Bayside 61, N.Y.
SCHACHTER, Gustav, Northeastern Univ., Sch. of Bus. Admin., Dept. of Econ., Boston, Mass.
SCHACHTER, Joseph, 647 W. 172nd St., New York, N.Y. 10032
SCHADE, Donald, Univ. of Dayton, Box 331, Dayton, Ohio
SCHADLER, Karl, Joh. Seb. Bachstr. 13, 8012 Munich-Ottobrunn, Germany
SCHADRACK, Frederick C., Jr., 61 S. Munn Ave., East Orange, N.J.
SCHAEFER, Donald F., 211 Pritchard Ave., Chapel Hill, N.C. 27514
SCHAEFER, Jeffrey, 368 B Hamilton St., Somerset, N.J. 08873
SCHAFER, David K., 439 E. 76th St., Apt. 1-A, New York, N.Y. 10021
SCHAFFER, (Mrs.) Beverly K., Georgia Inst. of Tech., Sch. of Ind. Mgt, Atlanta, Ga.
SCHAFFER, Sheldon, Southern Res. Inst., 2000 9th Ave., S., Birmingham 5, Ala.
SCHAFFER, William A., Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta, Ga.
SCHAFFNER, Joseph H., 1 Beekman Pl., New York, N.Y. 10022
SCHAFFNER, Philip P., 4200 Cathedral Ave., N.W., Apt. 519, Washington, D.C. 20016
SCHAIRER, Rosemary A., 324 E. 11th, Apt. C., Anchorage, Alaska 99501
SCHALL, Lawrence D., 7156 S. Cyril Ct., Apt. 409, Chicago, Ill. 60649
SCHALLAU, Con H., 6910 N.E. Hancock St., Portland, Ore. 97213
SCHALLER, Howard G., Dept. of Bus. Econ., Indiana Univ., Bloomington, Ind. 47405
SCHANTZ, Sidney, 6133 Leesburg Pike, Apt. 610, Falls Church, Va. 22041
SCHANZ, John J., Jr., 565 Westview Ave., State College, Pa. 16801
SCHARF, Robert, 2197 Willow Ave., N.E., Atlanta, Ga. 30305
SCHARY, Philip B., 20505 Londelius St., Canoga Park, Calif. 91306
SCHATZ, Edward R., Salem Col., Salem, W.Va. 26426
SCHATZ, (Mrs.) Gertrude, Salem Col., Salem, W.Va. 26426
SCHATZ, Sayre P., Brooklyn Col., Dept. of Econ., Brooklyn 10, N.Y.
SCHECHTER, Arthur, 996 Bell Ct., Valley Stream, N.Y.
SCHECHTER, Henry B., 1015 Robroy Dr., Silver Spring, Md.
SCHECK, Peter F., Texas Col. of A. and I., Dept. of Econ., Kingsville, Tex.
SCHEER, (Mrs.) Lorraine H., Univ. of Tulsa, Dept. of Econ., Tulsa, Okla.
SCHEID, Kenneth G., Kenneth G. Scheid and Asso., 5634 Northumberland St., Pittsburgh, Pa. 15217
SCHEIDELL, John M., Univ. of Georgia, Col. of Bus. Admin., Dept. of Econ. and Statis., Athens, Ga.
SCHELD, Karl A., Fed. Res. Bank of Chicago, Chicago, Ill. 60690
SCHELL, Kerry F., Univ. of Tennessee, Dept. of Forestry, Knoxville, Tenn. 37916
SCHELL, Steven R., 1779 Columbia, Eugene, Ore. 97401
SCHELLING, Thomas C., Harvard Univ. Int. Affairs, 6 Divinity Ave., Cambridge, Mass. 01922
SCHENCK, George H. K., 213 Mineral Sci. Bldg., University Park, Pa. 17774
SCHENDEL, D. E., Herman C. Krannert, Grad. Sch. of Ind. Admin., Purdue Univ., Lafayette, Ind. 47907
SCHENK, Darryl A., 321 S. Maple, Oak Park, Ill. 60302
SCHENK, William E., 1400 S. Joyce St., Apt. C-612, Arlington, Va. 22202
SCHENKEL, Walter, 6495 Broadway, Riverdale, New York, N.Y. 10471
SCHENKER, David, 1901 Wyoming Ave., N.W., Apartment 63, Washington 9, D.C.
SCHENKER, Eric, 3466 N. Frederick Ave., Milwaukee, Wis. 53211
SCHERER, Frederic M., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48104
SCHERER, Joseph, Fed. Res. Bank of New York Res. Dept., New York, N.Y. 10045
SCHERTZ, Lyle P., 701 S. Greenbrier St., Arlington, Va. 22204
SCHETTER, Thomas J., 1207-E Brackenridge Apts., Lake Austin Blvd., Austin, Tex.
SCHEUCH, Richard, 49 Mountain View Dr., West Hartford, Conn. 06090
SCHIAVO-CAMPO, Salvatore, 205 W. 91st St., New York, N.Y. 10024
SCHICK, Harry L., 100 W. 57th St., New York, N.Y. 10019
SCHICKELE, Rainer, c/o Agric. Dev. Council, Inc., 630 Fifth Ave., New York 20, N.Y.
SCHICKELE, (Mrs.) Sandra G., 33 Liberty St., New York, N.Y.
SCHIEBER, George, 2946 Murdock Rd., Wantagh, N.Y.
SCHIER, Robert J., 1471 Lance Dr., Tustin, Calif. 92680
SCHIFF, Eric, 4200 Cathedral Ave., Apt. 1116, Washington, D.C. 20016
SCHIFF, Frank W., Exec. Off. of the President, Coun. of Econ. Advisers, Washington, D.C. 20506
SCHIFF, Michael, New York Univ., Grad. Sch. of Bus. Admin., 100 Trinity Pl., New York 6, N.Y.
SCHIFFERS, Edmond A., 3400 Wren Ave., Ft. Worth, Tex. 76133
SCHIFRIN, Joseph A., 5112 Connecticut Ave., N.W., Washington, D.C. 20008
SCHIFRIN, Leonard G., Col. of William and Mary, Dept. of Econ., Williamsburg, Va.
SCHILLER, Herbert, Univ. of Illinois, Bur. of Econ. and Bus. Res., Rm. 408, D.K.H., Urbana, Ill.
SCHILLING, Donald J., Univ. of Missouri, 217 B. and P. A. Bldg., Columbia, Mo.
SCHILLING, Edward G., 79 Wyckoff St., New Brunswick, N.J. 08901
SCHILLO, Thomas J., 64 Bathurst Dr., Tonawanda, N.Y.
SCHINDLER, James S., State Univ. of New

York at Buffalo, Sch. of Bus. Admin., 105 Crosby Hall, Lib. Circle, Buffalo, 14, N.Y.
SCHIPPER, Lewis, 8105 New Hampshire Ave., W. Hyattsville, Md., 20783
SCHIRBER, Martin E., St. John's Univ., Collegeville, Minn. 56321
SCHIRO, Bruno A., US AID/PAD, APO New York 09271
SCHISA, Louis W., 5 Alpine Dr., Latham, N.Y. 12110
SCHKUDOR, Wasyl, 17185 Dean Ave., Detroit 12, Mich.
SCHLATTER, William J., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
SCHLESINGER, Eugene R., 3708 Oliver St., N.W., Washington, D.C. 20015
SCHLESINGER, James R., RAND Corp., 1700 Main St., Econ. Dept., Santa Monica, Calif. 90402
SCHLESINGER, Myron P., 122 6th St., Hicksville, N.Y.
SCHLICHTING, Cort B., 3312E Mordecai St., Durham, N. Carolina 27705
SCHLIEPER, Ulrich, Universitat, Sem. f Theor. Volkswirtsch., 66 Saarbrucken, Germany
SCHLINK, Frederick J., Consumers Res., Washington, N.J. 07882
SCHLOESSER, Fred W., c/o Sommer Internacional S.A., Lago Zurich 154, Mexico 17, D.F.
SCHLOSS, Hans S., German Consulate, Via Quintino Zella 77, Palermo, Italy
SCHLOSS, Henry H., Univ. of Southern California, Sch. of Bus., Los Angeles 7, Calif.
SCHLOTTHAUER, Julius P., c/o Cavins, 1126 6th St., Charleston, Ill. 61920
SCHMID, A. Allan, Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich. 48823
SCHMID, Stuart G., 1640 Macon St., McLean, Va. 22101
SCHMID, William A., 19 Round Top St., Pittsburgh, Pa. 15205
SCHMIDT, Alfred H., Arizona State Univ., Col. of Bus. Admin., Tempe, Ariz.
SCHMIDT, Andrew B., 2922 E. Helen St., Tucson, Ariz.
SCHMIDT, Billie J., 5523 Oak, Kansas City, Mo. 64110
SCHMIDT, Charles T., Jr., Michigan State Univ., Sch. of Labor and Indus, Rela., 5 Marshall Hall, East Lansing, Mich.
SCHMIDT, Edward B., Univ. of Neb., Col. of Bus. Admin., Lincoln, Neb.
SCHMIDT, Edward J., Univ. of Maryland, Col. of BPA, Econ. Dept., College Park, Md. 20742
SCHMIDT, Emerson P., 10220 Blake Lane, Oakton, Va. 22124
SCHMIDT, Gerhard, 203 W. 107th St., Apartment 8B, New York 25, N.Y.
SCHMIDT, Klaus-Dieter, Biesingerstr. 25, Tubingen, Germany
SCHMIDT, Kurt, Tech. Univ. Berlin, Lehrstuhl, fur Volkswirtschafts lehre u Volkswirtschafts., Politik, Hardenbergstr. 34, Berlin, West Germany
SCHMIDT, Orvis A., 3414 Cummings Lane, Chevy Chase, Md. 20015
SCHMIDT, Wilson E., 3621 Sprucedale Dr., Annandale, Va. 22003
SCHMOOKLER, Jacob, Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
SCHNABEL, Morton, California State Col. at Los Angeles, Dept. of Econ., 5151 State College Dr., Los Angeles, Calif. 90032
SCHNEIDER, Alfred, Univ. of Dayton, Dayton 9, Ohio
SCHNEIDER, Erich, Forstweg 56, 23 Kiel, Western Germany
SCHNEIDER, Fritzi M., Model, Roland and Co., 120 Broadway, New York, N.Y. 10005
SCHNEIDER, George, 2701 Turkeyfoot Rd., South Ft. Mitchell, Ky. 41012
SCHNEIDER, John H., Jr., 11 Hamilton St., Lynbrook, N.Y.
SCHNEIDER, Lewis M., Harvard Bus. Sch., Baker 130, Boston, Mass. 02163
SCHNEIDER, Norman, Univ. of California, Dept. of Econ., Davis, Calif. 95616
SCHNEIDER, Wilbert M., Pacific Union Col., Angwin, Calif. 94508
SCHNEIDER, William G., 5023-A Chippewa, St. Louis, Mo. 63109
SCHNELLE, Kenneth E., Michigan Col. Mining and Tech., Dept. of Bus. Eng. Admin., Houghton, Mich. 49931
SCHNITZEL, Paul E., 90 Berry St., Brooklyn, N.Y. 11211
SCHNITZER, Martin, Virginia Poly. Inst., Sch. of Bus., Blacksburg, Va.
SCHOCK, Robert L., 8001 21st St., W., Tacoma, Wash. 98466
SCHOEFFLER, Sidney, General Electric Co., 570 Lexington Ave., Rm. 1801, New York, N.Y. 10022
SCHOELLKOPF, Wolfgang, 245 E. 63rd St., New York, N.Y. 10021
SCHOEN, Sterling H., Washington Univ., Grad. Sch. of Bus. Admin., St. Louis 30, Mo.
SCHOENBECK, Henry, Upsala Col., East Orange, N.J.
SCHOENBROD, David S., 66 Norton St., New Haven, Conn. 06511
SCHOEPLEIN, Robert N., Univ. of Washington, Dept. of Econ., Seattle, Wash.
SCHOFF, Leonard H., K-G Ranch R.D., Atglen, Pa. 19310
SCHOGER, Harry G., 8208 Highland Pl., Munster, Ind.
SCHOOLER, Eugene W., Westchester State Col., Dept. of Soc. Studies, Westchester, Pa. 19380
SCHOONMAKER, E. Harold, Merry Point, Va. 22513
SCHOTT, Francis Helmut, Fed. Res. Bank of New York, Res. Dept., New York 45, N.Y.
SCHOTTA, Charles, Jr., Univ. of California-Davis, Dept. of Econ., Davis, Calif. 95616
SCHRADER, Ernst J., Jr., 306 W. 100th St., Apt. 86, New York, N.Y. 10025
SCHRAMM, John C., R.D.2, West Redding, Conn. 06896
SCHRAMM, Gunter, 2864 Bellewood, Ann Arbor, Mich.
SCHRAN, Peter, 1804 Carle Dr., Urbana, Ill. 61801
SCHRATZ, Walter A., 219 Castlegate Rd., Pittsburgh, Pa. 15221
SCHREDER, Harold X., Distributors Group, Inc., 80 Pine St., New York, N.Y. 10005
SCHREIBER, Robert W., Mobil Petroleum Co., Inc., 150 E. 42nd St., Room 1049, New York 17, N.Y.
SCHREIBER, William L., 809 West Glentana, Apt. 3, Covina, Calif.
SCHREINER, Dean F., Iowa State Univ., Dept. of Econ., Ames, Iowa
SCHRIMPER, Ronald A., North Carolina State Col., Sch. of Agric., Raleigh, N.C.
SCHRIVER, William R., R. 14, Schriver Rd., Knoxville, Tenn.
SCHROCK, Nicholas W., Univ. of Oregon, Dept. of Econ., Eugene, Ore. 97403
SCHROCK, Robert D., 211 E. 10th St., Lawrence, Kans.
SCHROEDER, Gertrude E., 3051 Porter St., N.W., Washington 8, D.C.
SCHROEDER, Robert G., 55 W. Santa Clara Ave., San Jose 6, Calif.
SCHROEDER, Theodore C., 2 Forest Dr., Pittsburgh, Pa. 15220
SCHRUBEN, Leonard W., 800 Wildcat Ridge, Manhattan, Kans. 66502
SCHULKIN, Peter A., 6812 Emerson St., Hyattsville, Md. 20784
SCHULLER, George J., 1609 16th St., N.W., Washington 9, D.C.
SCHULMAN, Albert A., 13012 Evanston St., Rockville, Md. 20853
SCHULMAN, H. E. C., 62 Aberdeen Ave., Montreal 6, P.Q., Canada
SCHULMAN, Joseph F., 5950 14th St. N.W., Apt. A-1, Washington, D.C. 20011
SCHULMAN, (Mrs.) Rosalind, 12B24 The Philadelphian, Philadelphia, Pa. 19130
SCHULMANN, Horst, 4 Eintrachstrasse, 46 Dortmund, Germany
SCHULTE, Sister M. Yolande, Col. of St. Teresa, Winona, Minn.
SCHULTHEIS, Michael J., Regis Col., 3425 Bayview Ave., Willowdale, Ont. Canada
SCHULTZ, George P., Univ. of Washington, Col. of Architecture, Seattle, Wash. 98105
SCHULTZ, Robert, 50 Fuller Ave., Chatham, N.J. 07928

SCHULTZ, Robert P., 434 Geron Dr., Springfield, Ohio
SCHULTZ, Robert S., III, Union Bag-Camp Paper Corp., Woolworth Bldg., New York 7, N.Y.
SCHULTZ, S. Ray, South Dakota State Univ., Econ. Dept., Brookings, S.D., 57006
SCHULTZ, T. Paul, RAND Corp., 1700 Main St., Santa Monica, Calif. 90406
SCHULTZ, Theodore W., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
SCHULTZ, Wolfgang M., Univ. of Alberta, Dept. of Agric. Econ., Edmonton, Alta., Canada
SCHULTZE, Charles L., Univ. of Maryland, Dept. of Econ., College Park, Md.
SCHULZ, James H., Univ. of New Hampshire, Whittemore Sch. of Bus. and Econ., Durham, N.H.
SCHULZ, John H. I., 2909 34th St., N.W., Washington 8, D.C.
SCHUMACHER, Arnold C., 2150 Sherman Ave., Evanston, Ill. 60201
SCHUMAIER, C. P., 428 So. Lee St., Alexandria, Va.
SCHUMANN, Jochen, Seminar fur Wirtschafts-wissen-schaftender Univ., Mertonstr. 17, Frankfurt/Main, Germany
SCHUPACK, Mark B., Brown Univ., Providence 12, R.I.
SCHUR, Leon M., Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee, Wis. 53211
SCHURMAN, Bernard, Univ. of Rhode Island, Dept. of Econ., Kingston, R.I. 02836
SCHURR, Edward J., Ferris State Col., Sch. of Com., Big Rapids, Mich. 49307
SCHURR, Sam H., Res. for the Future, Inc., 1755 Massachusetts Ave. N.W., Washington 36, D.C.
SCHUSSHEIM, Morton J., 9 Oxford St., Chevy Chase, Md. 20015
SCHUSTER, E. Elaine, 1224 N.W. 33rd, Oklahoma City, Okla. 73118
SCHUSTER, Jorge F., Apartado-del-Este 4985, Caracas, Venezuela
SCHUSTER, Joseph H., 5822 Greenlawn Dr., Bethesda, Md. 20014
SCHUSTER, Louis H., Univ. of Lagos, Lagos, Nigeria
SCHUSTER, Marvin, 69-06 138th St., Kew Gardens Hills, N.Y. 11367
SCHUTJER, Wayne, Pennsylvania State Univ., Dept. Agric. Econ., 1 Weaver Bldg., University Park, Pa. 17774
SCHUTZ, Robert R., 1304 Acton St., Berkeley 6, Calif.
SCHUYLER, F. D., Hqs. Special Troops, U.S. Army, Vietnam, APO San Francisco 96307
SCHWAB, Paul M., 5103 Pembridge Ave., Baltimore, Md.
SCHWALBERG, Barney K., Brandeis Univ., Dept. of Econ., Waltham, Mass. 02154
SCHWARTING, Joe L., R. 9, Box 189, Tucson, Ariz
SCHWARTZ, (Mrs.) Anna J., NBER, 261 Madison Ave., New York, N.Y. 10016
SCHWARTZ, Bella, 2222 Eye St., N.W., Washington 7, D.C.
SCHWARTZ, (Mrs.) Carol H., 110 Caton Ave., Brooklyn 18, N.Y.
SCHWARTZ, Charles F., 7504 Honeywell Lane, Bethesda, Md. 21811
SCHWARTZ, Charles M., St. Francis Col., Brooklyn, N.Y.
SCHWARTZ, David, 2630 Kingsbridge Ter., Apt. 1W, New York 63, N.Y.
SCHWARTZ, David L., Albright Col., Reading, Pa. 19604
SCHWARTZ, David S., 4000 65th Ave., Hyattsville, Md. 20784
SCHWARTZ, Donald A., 4248 N. 75th St., Milwaukee, Wis. 53216
SCHWARTZ, Eli, Lehigh Univ., Fin. Dept., Bethlehem, Pa.
SCHWARTZ, Harry, 50 Argyle Rd., Scarsdale, N.Y.
SCHWARTZ, Harry S., Federal Home Loan Bank Bldg., Washington 25, D.C.
SCHWARTZ, Harvey, 80 Grand Ravine Dr., Apt. 704, Downsview, Ont., Canada
SCHWARTZ, Hugh H., Univ. of Kansas, Dept. of Econ., Lawrence, Kans.
SCHWARTZ, Jonathan P., 3 E. 63rd St., New York, N.Y. 10021
SCHWARTZ, Joseph, 10651 Fairlawn Dr., Parma, Ohio 44130
SCHWARTZ, Lawrence, Center for Naval Analyses, 1401 Wilson Blvd., Arlington, Va.
SCHWARTZ, Lawrence E., 70-25 Yellowstone Blvd., Apt. 21-B, Forest Hills, N.Y. 11375
SCHWARTZ, Morton D., 159-00 Riverside Dr., West, Apt. 4M-70 Bldg., New York, N.Y. 10032
SCHWARTZ, Nancy L., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh, Pa. 15213
SCHWARTZ, Robert A., 360 E. 65th St., New York, N.Y. 10021
SCHWARTZ, Robert A., 62 Livingston St., Brooklyn, N.Y. 11201
SCHWARTZ, Robert J., Israel Discount Bank, Ltd., 511 Fifth Ave., New York 17, N.Y.
SCHWARTZBERG, Edward, 510 Oak St., Massapequa, N.Y.
SCHWARTZMAN, David, 285 Central Park West, New York, N.Y. 10024
SCHWARZ, Evelyn A., 65-30 38th Ave., Woodside, N.Y. 11377
SCHWARZSCHILD, Stuart, Georgia State Col., 33 Gilmer St., S.E., Atlanta, Ga. 30303
SCHWEIGER, Irving, Univ. of Chicago, Grad. Sch of Bus., Haskell Hall, Chicago, Ill. 60637
SCHWEITZER, Arthur, Indiana Univ., Bloomington, Ind.
SCHWEITZER, Paul R., 4513 46th St., N.W, Washington 16, D.C.
SCHWEITZER, Robert N., 2 McCarthy Ave, San Francisco, Calif. 94134
SCHWEITZER, Stuart, Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
SCHWEITZER, Thomas T., 319 Island Park Dr., Ottawa 3, Ont., Canada
SCHWENG, Lorand D., c/o Mr. L. Boor, 2529 S 19th St., Philadelphia 45, Pa.
SCHWER, R. Keith, P.O. Box 811, Knoxville, Tenn. 37901
SCHWIER, (Mrs.) Ann S., 18 No. Taylor, St. Louis 8, Mo.
SCHWIER, Jerome F., 18 N. Taylor, St. Louis 8, Mo.
SCHWITTER, Joseph P., Kent State Univ., Dept. of Ind. Admin. and Prod., Kent, Ohio
SCITOVSKY, Tibor, Development Centre, OECD, 91 Blvd. Exelmans, Paris 16, France
SCLAR, Elliott, 80-A Freemont St., Sonerville, Mass. 02145
SCOLL, E. Edward, 2801 N. Jefferson St., Arlington, Va. 22205
SCOTT, Anthony D., Univ. of British Columbia, Dept. of Econ. and Pol. Sci., Vancouver, B.C., Canada
SCOTT, (Mrs.) Charlotte H., 5242 S. Greenwood, Chicago, Ill. 60615
SCOTT, David L., Florida State Univ., Box 1863, Tallahassee, Fla.
SCOTT, Donald R., 8960 Hennepin Ave., Niagara Falls, N.Y. 14304
SCOTT, Eldred H., 815 Rivenoak, Birmingham, Mich. 48008
SCOTT, Helen J., 381 Broad St., Newark, N.J.
SCOTT, Ira O., Jr., Columbia Univ., Grad. Sch of Bus., New York 27, N.Y.
SCOTT, J. T., Univ. of California, Econ. Dept, 250 Barrows Hall, Berkeley, Calif.
SCOTT, John F., 810 Fairview Rd., Carlisle, Pa. 17013
SCOTT, John T., Jr., 5 Bellamy Ct., Champaign, Ill.
SCOTT, Ralston D., Lotus Dr., R.R. 5 Box 276, Edwardsville, Ill. 62025
SCOTT, Raymond C., 3606 N. Monroe St., Arlington, Va. 22207
SCOTT, Robert G., 3006 St. Paul St., Baltimore, Md. 21218
SCOTT, Robert H., Jr., Univ. of Pittsburgh Dept. of Econ., Pittsburgh 13, Pa
SCOTT, Robert H., Univ. of Washington, Col. of Bus. Admin., Seattle 5, Wash.
SCOTT, Stanley Z., Valley National Bank, Phoenix, Ariz.
SCOTT, William C., St. Ambrose Col., Davenport, Iowa 52803
SCOUFELIS, Aristides, 102-09 63rd Dr., Forest Hills, N.Y. 12068

SCOVEL, Victor L., Int. Engineering Co., P.O. Box 160, Ramna, Dacca 2, East Pakistan
SCOVILLE, James G., Harvard Univ., Rm. 130 R. O. T. C. Bldg., Cambridge, Mass. 02138
SCOVILLE, John A., 17 Long Ridge Rd., Manhasset, N.Y.
SCOVILLE, Warren C., Univ. of Calif. at Los Angeles, Econ. Dept., Los Angeles, Calif. 90024
SCRIMSHAW, Stewart, 8737 Park View Ct., Wauwatosa, Wis. 53226
SCRIVEN, Marc C., Case Inst. of Tech., Div. of Org. Sci., University Circle, Cleveland, Ohio 44106
SCUDDER, David B., 7513 Mendota Pl., Springfield, Va.
SCULLY, Charles J., 46 Sylvan Ave., Chelmsford, Mass. 01824
SCULLY, Gerald W., Bartle Court, Apt. 26B, Highland Park, N.J. 08904
SEAGER, Scott E., Southern Methodist Univ., Dallas 22, Tex.
SEAGLER, John C., 106-A Sunset Ave., Charlottesville, Va.
SEAGRAVES, James A., North Carolina State Col., Rm. 1, Patterson Hall, Raleigh, N.C.
SEALY, Desmond H., 1118 Park Rd., N.W., Washington 10, D.C.
SEARL, Milton F., 8808 Walnut Hill Rd., Gaithersburg, Md. 20760
SEARLES, Ernest W., 281 Wiley Ct., Claremont, Calif. 91711
SEARS, G. Alden, Moravian Col., Dept. of Econ. and Bus. Admin., Bethlehem, Pa. 18018
SEARS, Thomas A., c/o C. Kerby-Miller, 8 Appian Way, Cambridge, Mass. 02138
SEASTONE, Don A., Colorado State Univ., Dept. of Econ., Ft. Collins, Colo.
SEAY, Edmond E., Jr., Dept. of Econ., East Hall, Iowa State Univ., Ames, Iowa 50010
SEAY, Edward W., Centenary Col. for Women, Hackettstown, N.J. 07840
SEBEL, Roger, Roger Sebel and Co., Box 225 Post Office, Bankstown, N.S.W., Australia
SEBESTYEN, Zoltan, 423 W. 120th St., Apt. 96, New York 27, N.Y.
SECKLER, David, Colorado State Univ., Dept. of Econ., Ft. Collins, Colo. 80521
SEDJO, Roger A., 5703 18th, N.E., Apt. 6, Seattle 5, Wash.
SEEBER, Norton C., Carnegie Inst. of Tech., Pittsburgh, Pa. 15213
SEEBERGER, Harold M., 187 Fern St, Fairfield, Conn. 06430
SEELYE, Donald W., 325 Taylor St., Moscow, Idaho 83843
SEELYE, Frank D., Idaho State Univ., Col. of Bus. Admin., Pocatello, Idaho 83201
SEERY, Annette K., 970 Rexdale Dr., Cuyahoga Falls, Ohio
SEGAL, Harvey H., The Washington Post, Editorial Dept., 1515 L St., N.W., Washington 5, D.C.
SEGAL, (Mrs.) Judith A., 2161 N. Pierce St. 4, Arlington 9, Va.
SEGAL, Melvin J., 531 Kedzie Dr., East Lansing, Mich.
SEGAL, Robert M., 74 Sylvan Ave., West Newton, Mass. 01985
SEGALL, Joel, Sch. of Bus., Univ. of Chicago, Chicago, Ill. 60637
SEGALL, Maurice, 1301 Ave. of the Americas, New York, N.Y. 10019
SEGELHORST, Elbert W., 2617 E. 1st St., Long Beach, Calif.
SEGER, Martha R., The York, Apt. 814, 532 20th St., N.W., Washington, D.C. 20006
SEGRE, Claudio, 36, Rue de Comines, Brussels, Belgium
SEIBERT, Joseph C., Miami Univ., Oxford, Ohio 45056
SEIDEL, Eugene M., 2109 N.W. Couch St., Camas, Wash. 98607
SEIDEN, Martin H., 1954 Columbia Rd. N.W., Washington, D.C.
SEIDENFUS, Dr. Hellmuth S., Univ. Munster, Inst. fur Verkehrswissenschaft, Universitätstr. 14-16, 44 Münster, Germany
SEIDENSTAT, Paul, Univ. of Delaware, Dept. of Econ. and Bus. Admin., Newark, Del. 19711
SEIDENSTRICKER, K. C., 7075 Inca Way, Denver 21, Colo.
SEIDLER, Gustav, 8 E. Irving St., Chevy Chase, Md. 20015
SEIDMAN, Joel, 5421 Cornell Ave., Chicago, Ill. 60615
SEIFER, Daniel M., Michigan State Univ., Regional Center, 148 Ransom, N.E., Grand Rapids, Mich.
SEIJAS-ROMAN, Germán, United Nations, Ind. Dev. Div., Econ. Comm. for Latin America, Santiago, Chile
SEILER, Stuart, 8 Ardsley Rd., Glen Ridge, N.J. 07028
SEIMER, Stanley J., Syracuse Univ., Col. of Bus. Admin., 105 Slocum Hall, Syracuse 10, N.Y.
SEINKIEWICZ, Edward W., Jr., Univ. of Oregon, Dept. of Econ., Eugene, Ore. 97403
SEKIGUCHI, Harold S., Drake Univ., Col. of Bus. Admin., Des Moines, Iowa
SELANDER, John M., Eastern Washington State Col., Div. of Bus. and Ind., Martin Hall 119, Cheney, Wash. 99004
SELBY, Edward B., Jr., Louisiana State Univ., P.O. Box 16374, Baton Rouge, La.
SELDEN, Richard T., Cornell Univ., Dept. of Econ., Goldwin Smith Hall, Ithaca, N.Y.
SELDIN, Maurice, American Univ., Sch. of Bus. Admin., Massachusetts and Nebraska Aves. N.W., Washington, D.C. 20016
SELDONITZ, Estelle, 2800 Quebec St. N.W., Washington, D.C. 20008
SELIGMAN, Barnard, City Univ. of New York, Dept. of Econ., Baruch Sch., 17 Lexington Ave., New York, N.Y. 10010
SELIGMAN, Ben B., 53 Wildwood Lane, Amherst, Mass.
SELIM, Ezzat M., 76 Gamhouria St., Apt. 40, Cairo, Egypt
SELIM, Mohamed A., Col. of St. Thomas, Dept. of Econ., St. Paul 1, Minn.
SELLALI, Abdel H., 33 Blvd., Clemenceau, St. Eugene, Algiers, Algeria
SELTZER, Arthur O., 1 East Schiller St. Apt. 5C, Chicago, Ill. 60610
SELTZER, George O., 1917 E. River Rd., Minneapolis, Minn. 55414
SELTZER, Irving, 1215 16th St., Altoona, Pa.
SELTZER, Lawrence H., Wayne State Univ., 19575 Stratford Rd., Detroit 21, Mich.
SEMER, (Mrs.) Charlene E., 2 Bedford Lane, Alexandria, Va.
SEN, Bandhudas, Univ. of Wisconsin, Dept. of Agric. Econ., Madison, Wis.
SENF, Clinton E., Jr., 213 77th St., Niagara Falls, N.Y.
SENG, Mohammad, Djalan Kemakmuran No. 20-i, Djakarta, Indonesia
SENIOR, Clarence, 15 Claremont Ave., New York 27, N.Y.
SENN, Peter R., 1121 Hinman Ave., Evanston, Ill.
SENNHOLZ, Hans F., 200 E. Pine St., Grove City, Pa. 16127
SENTURIA, Joseph J., 5128 Linnean Ter., N.W., Washington 8, D.C.
SEQUIN, C. Joseph, 19204 Staffordshire, South Bend 17, Ind.
SERA, Sergio, Italian Embassy, Seoul, Korea
SERAFY, S. E. D., Econ. Intelligence Unit, Spencer House, 27 St. James's Pl., London, S.W 1, England
SEREX, Alfred M., 41-50 46th St., Long Island City, N.Y. 11104
SESNOWITZ, Michael L., 1935 Shore Pkwy., Brooklyn 14, N.Y.
SETHUR, Frederick, 651 E. 14th St., New York 9, N.Y.
SETON, John C., 66 E. 80th St., New York, N.Y. 10021
SEUFFERLE, Charles H., Univ. of Nevada, Col. of Agric., Reno, Nev. 89507
SEVERANCE, Malcolm F., RFD #1, Colchester, Vt. 05446
SEVERN, Alan K., 1831 23rd St., N.W., Washington, D.C. 20008
SEVERSON, Harry L., c/o Amer. Nat. Bank, Mobile, Ala.
SEVERSON, Robert F., Jr., Rutgers The State Univ., 406 Penn St., Camden, N.J.
SEVIN, Charles H., 1928 Wynnefield Ter., Philadelphia 31, Pa.

SEWELL, David O., Duke Univ., Dept. of Econ., Durham, N.C. 27706
SEWELL, Granville H., Battelle Memorial Inst., 505 King Ave., Columbus, Ohio 43201
SEXTON, Donald E., Jr., Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
SEYFRIED, Warren R., Jr., Univ. of Washington, Col. of Bus. Admin., Dept. of Urban Land Econ., Seattle 5, Wash.
SEYMOUR, Helen V., 17 Grenfell Dr., Great Neck, L.I., N.Y.
SEYMOUR, Robert G., Tec-Search, Inc., First Nat. Bank Bldg., Evanston, Ill.
SGONTZ, L. G., Bur. of Bus. and Econ. Res., State Univ. of Iowa, Iowa City, Iowa
SHACKLETON, L. Allan, 116 Huron Ave., Ottawa, Ont., Canada
SHAFER, Carl E., Dept. of Agric. Econ., Texas A and M Univ., College Sta., Tex. 77843
SHAFER, Joseph E., Alaska Methodist Univ., Anchorage, Alaska
SHAFER, Wayne J., 6711 El Colegio, Apt. 1, Goleta, Calif. 93011
SHAFEY, Erfan, 3730 S. McClintock, #240, Los Angeles, Calif. 90007
SHAFFER, Edward H., Western Washington State Col., Dept. of Econ., Bellingham, Wash. 98225
SHAFFER, Harry G., Univ. of Kansas, Dept. of Econ., Lawrence, Kans. 66045
SHAFFER, Jack N., R.R. No. 3, Carmi, Ill.
SHAFFER, James D., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich. 48823
SHAFFER, N. Manfred, Univ. of California, Adlai E. Stevenson Col., Santa Cruz, Calif. 95060
SHAFFER, Robert H., Bank of America, N.T. and S.A., Econ. Dept., San Francisco, Calif. 94120
SHAFFNER, Felix I., 1816 Queens Lane, Arlington 1, Va.
SHAH, Praful B., Hammer, Greene, Sider Asso., 1413 K St., N.W., Washington, D.C. 20005
SHAH, Umang G., 4306 Yoakum, No. 1-F, Houston, Tex.
SHAH, V. C., Indian Investment Center, Parliament St., New Delhi 1, India
SHAHAN, Ewing P., Vanderbilt Univ. Box 1502, Nashville, Tenn. 37205
SHAHEN, Timothy G., 852-m Graduate House, Lafayette, Ind.
SHAKERI, Khosrow, 15 Newerwall, 2 Hamburg 36, Germany
SHAM, Donald, 2124 Eye St., Washington, D.C. 20037
SHAMI, Charles H., 275 Central Park West, New York, N.Y. 10024
SHAMSEDDINE, Ahmad H., 121 Taussig Pl., N.E., Washington, D.C. 20011
SHANAHAN, John H., Jr., 5327 Longmont Dr., Houston, Tex. 77027
SHANE, Mathew D., Purdue Univ., Herman C. Krannert Grad. Sch. of Ind. Admin., Lafayette, Ind. 47907
SHANKLIN, James T., Westminster Col., Dept. of Econ., and Bus., New Wilmington, Pa. 16142
SHANKLIN, (Mrs.) Phyllis, 222 Circle Dr., Collinsville, Ill. 62234
SHANNON, John C., 78 Hillsdale Rd., Medford, Mass. 02155
SHANNON, Brother L. Robert, Lewis Col., Lockport, Ill. 60441
SHANNON, Richard E., Montana State Univ., Dept. of Econ., Missoula, Mont. 59801
SHANNON, Robert F., 85 Mill St., Athens, Ohio 45701
SHANNON, Russell D., Clemson Univ., Dept. of Soc. Sci., Clemson, S.C. 29631
SHAO, Stephen Pinyee, Old Dominion Col., Sch. of Bus., Norfolk 8, Va.
SHAPIRO, Alvin A., 19 Rochester Ter., Flat 3, London, N.W. 1, England
SHAPIRO, David L., 816 Riley Dr., Albany, Calif. 94706
SHAPIRO, Edward, Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
SHAPIRO, Eli, 514 Parker St., Newton Centre, Mass. 01355
SHAPIRO, Harvey, Bur. of the Budget, Off. of Budget Rev., Washington, D.C.
SHAPIRO, Howard, 939 Woodycrest Ave., Bronx, N.Y. 10452
SHAPIRO, Milton, 13122 LaSalle, Huntington Woods, Mich. 49748
SHAPIRO, Milton M., 451 Converse Ave., Claremont, Calif.
SHAPIRO, Perry, 1325 Arch St., Berkeley, Calif.
SHAPIRO, Robert, 2 Washington Sq. Village, New York 12, N.Y.
SHAPIRO, Sherman, Univ. of Illinois, Chicago, Col. of Bus. Admin. Box 4348, Chicago, Ill. 60680
SHAPIRO, Sigmund, Fed. Housing Admin., Longfellow Bldg., Div. of Res. Rm. 466, Washington, D.C. 20411
SHAPIRO, Solomon, 66-10 149th St., Flushing, N.Y. 11367
SHAPIRO, Stephen L., Old Dominion Col., Dept. of Econ., Norfolk, Va. 23508
SHARFMAN, I. Leo, 1108 Baldwin Ave., Ann Arbor, Mich.
SHARKEY, William K., Purdue Univ., Dept. of Econ., West Lafayette, Ind. 47178
SHARMA, Grace, 407 Eddy St., Ithaca, N.Y.
SHARMA, Prem P., c/o Dr. O. P. Gupta, B-59 Bajaj Nagar, Jaipur, India
SHARMA, Purushottam K., Naseem Manzil, New Area, Kadamkuan, Patna 3, Bihar, India
SHARP, J. Frank, Rutgers Univ., Col. of Engineering, New Brunswick, N.J.
SHARP, (Mrs.) Margaret L., 41 Hall Dr., London S.E. 26, England
SHARPE, Howard D., Jr., 78 Christopher St., New York, N.Y. 10014
SHARPE, William F., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98106
SHARRON, Arthur, 315 W. 57th St., Apt. 11-C, New York, N.Y. 10019
SHATTO, (Mrs.) Gloria, 8819 Bintliff Dr., Houston 36, Tex.
SHATTUCK, Leroy A., Jr., Univ. of Pittsburgh, Grad. Sch. of Bus., Pittsburgh, Pa. 15213
SHAVELL, Zalman A., 3213 Bancroft Rd., Baltimore 15, Md.
SHAVIRO, Frieda W., 130 Gale Pl., New York 63, N.Y.
SHAVIRO, Sol, 130 Gale Pl., New York 63, N.Y.
SHAW, Edward S., 525 Los Arboles Ave., Stanford, Calif. 96128
SHAW, Fayette B., R.R. 1, Mokena, Ill. 60448
SHAW, John A., Jr., 5665 N. Flora, Fresno, Calif. 93726
SHAW, Joseph F., 3724 Carmona Ave. No. 1, Los Angeles, Calif. 90016
SHAW, Lawrence H., c/o B. T. Shaw, 4501 Connecticut Ave., Washington, D.C. 20008
SHAW, (Mrs.) Sue O., c/o B. T. Shaw, 4501 Connecticut Ave., Washington, D.C. 20008
SHAW, W. Lawrence, 295 Keats Circle, Pleasant Hill, Calif. 94523
SHAW, William F., 6527 Byrnes Dr., McLean, Va. 22101
SHAW, William H., 2317 MacDonough Rd., Wawaset, Wilmington 5, Del.
SHAWVER, Donald L., Univ. of Missouri, New B. and Pa. Bldg., Rm. 233, Columbia, Mo.
SHAY, Robert P., Nat. Bur. of Econ. Res., 261 Madison Ave., New York 16, N.Y.
SHEA, John L., Fordham Univ., Bronx, N.Y. 10458
SHEA, John R., Univ. of Santa Clara, Sch. of Bus., Dept. of Econ., Santa Clara, Calif. 95053
SHEA, Thomas W., c/o Arabian Amer. Oil Co., Econ. Dept., 505 Park Ave., New York, N.Y. 10022
SHEAHAN, John B., Williams Col., Dept. of Econ., Williamstown, Mass. 01267
SHEARER, Henry K., 960 College Ave., Collegeville, Pa. 19426
SHEARER, John C., Pennsylvania State Univ., Dept. of Econ., 120 Boucke Bldg., University Park, Pa. 16802
SHEARER, Ronald A., Univ. of British Columbia, Dept. of Econ. and Polit. Sci., Vancouver 8, B.C. Canada
SHEARER, Warren W., 500 E. Wabash Ave., Crawfordsville, Ind. 47933
SHECHTER, Nathaniel E., Georgia Southern Col., Div. of Soc. Sci., Statesboro, Ga.
SHECHTER, Samuel, 175 W. 93rd St., New York 25, N.Y.

SHEEHAN, James G., Winter Park Arms, Winter Park, Fla.
SHEEHY, Seamus J., Fac. of Agric. (U.C.D.), Glasnevin, Dublin, Ireland
SHEFTEL, Harry B., 5813 Third Pl., N.W., Washington 11, D.C.
SHEIFER, Victor J., 8601 Manchester Rd., Silver Spring, Md.
SHEINBAUM, Stanley K., Center of Democratic Inst., Box 4068, Santa Barbara, Calif.
SHEINWOLD, Philip, 89-19 217th St., Queens Village, N.Y. 12141
SHELBY, George D., Northeastern Univ., Dept. of Econ., Boston 15, Mass.
SHELDON, Charles S., II, 3507 N. Piedmont St., Arlington, Va. 22207
SHELL, Karl L., Massachusetts Inst. of Tech., Dept. of Econ., Cambridge, Mass. 02139
SHELLENBERGER, Elmer B., Bethany Nazarene Col., Bethany, Okla. 73008
SHELTON, David H., Univ. of North Carolina, Dept. of Econ., Greensboro, N.C. 27412
SHELTON, John P., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles, Calif. 90024
SHEN, Ching Shen (A.), 1515 Shelburne Dr., Bethlehem, Pa. 18018
SHEN, Fa-Chun, Univ. of Santa Clara, Sch. of Bus., Santa Clara, Calif.
SHEN, Ruth R., 6065 San Pablo Dam Rd., El Sobrante, Calif. 94803
SHEN, Tsung-Yuen, 6065 San Pablo Dam Rd., El Sobrante, Calif. 94803
SHENEFIELD, Hale T., Main St. and Piney Rd., Cotuit, Mass. 02635
SHENKEL, William M., Univ. of Georgia, Col. of Bus. Admin., Athens, Ga. 30601
SHEPARD, E. Fenton, 2527 N. Utah St., Arlington, Va. 22207
SHEPHERD, Alvah R., 178 N. Taylor St., Oak Park, Ill. 60302
SHEPHERD, Geoffrey S., Iowa State Col., Dept. of Agric. Econ., Ames, Iowa
SHEPHERD, James, Univ. of British Columbia, Dept. of Econ., Vancouver 8, B.C., Canada
SHEPHERD, William G., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
SHEPPARD, Ernest J., 535 Hayes St., West Lafayette, Ind. 47178
SHER, William, Univ. of Alberta, Faculty of Com., Edmonton, Alta., Canada
SHERBANIUK, J. A., Univ. of Manitoba, Sch. of Com., Winnipeg, Canada
SHERE, Louis, Indiana Univ., Dept. of Econ., Bloomington, Ind.
SHERIDAN, Richard B., Univ. of Kansas, Dept. of Econ., Lawrence, Kans.
SHERIFF, Don R., Univ. of Iowa, Col. of Bus. Admin., Phillips Hall, Iowa City, Iowa 52240
SHERK, Donald R., U.S. Military Academy, Dept. of Soc. Sci., West Point, N.Y. 10996
SHERMAN, (Mrs.) Heidemarie S., Wayne State Univ., Dept. of Econ., Detroit 2, Mich.
SHERMAN, Howard J., Univ. of California, Dept. of Econ., Riverside, Calif.
SHERMAN, Joseph V., 160 Columbia Hgts., Brooklyn 1, N.Y.
SHERMAN, Lawrence W., Jr., Robert Morris Junior Col. at Carthage, College Ave., Carthage, Ill. 62321
SHERMAN, Michael D., 2450 Sycamore Lane, # 22-A, West Lafayette, Ind. 47906
SHERMAN, Nathan, Lehman Brothers, 1 William St., New York, N.Y. 10004
SHERMAN, Richard U., Jr., 3862 Waldo Pl., Columbus, Ohio 43221
SHERMAN, Roger, Univ. of Virginia, Dept. of Econ., Charlottesville, Va. 22903
SHERMAN, William R., 98 N. Norwood Ave., Hillsdale, Mich. 49242
SHERR, Lawrence A., Univ. of Kansas, Sch. of Bus., Lawrence, Kans.
SHERRARD, Alfred, 305 N. Washington St., Alexandria, Va.
SHERRARD, (Mrs.) Edith H., 305 N. Washington St., Alexandria, Va.
SHERRINGTON, Charles E. R., Byways, 20, Queens Rd., Belmont, Surrey, England
SHERRY, John C., 49 Norman Dr., Ramsey, N.J. 07446
SHERVA, Dennis G., 3433 25th Ave., S., Minneapolis, Minn. 55406
SHERWIN, Douglas S., Phillips Products Co. Inc., P.O. Box 697, Dexter, Mich. 48130
SHIBATA, Hirofumi, Queen's Univ., Dept. of Econ., Dunning Hall, Kingston, Ont., Canada
SHIBAYAMA, Koji, Gakuen-cho Minami 2-cho, Nara, Japan
SHIEH, John Ting-Chung, 517 12th St., Alva, Okla. 73717
SHIELDS, Hal D., 438 Wolcott, Middletown, R.I. 02844
SHIELDS, Murray W., Univ. of Florida, 314 Matherly Hall, Gainesville, Fla.
SHIELDS, Thomas K., Quito-FSO, Dept. of State, Washington, D.C. 20521
SHIH, (Mrs.) C. Ying, 9047 N. Mango Ave., Morton Grove, Ill. 60053
SHIH, Charles T., 42-55 Colden St., Flushing, N.Y. 11355
SHIH, Ko Ching, 9047 N. Mango Ave., Morton Grove, Ill. 60053
SHILLING, A. Gary, Standard Oil Co., N.J., Gen. Econ. Dept., 3050 Rockefeller Plaza, New York 20, N.Y.
SHILLING, John D., 135 Oxford St., Apt. 2, Cambridge, Mass. 02140
SHILLING, Ned, Penna. Furnace, Pa. 16865
SHILLINGLAW, Gordon, Columbia Univ., Grad. Sch. of Bus., New York 27, N.Y.
SHIN, Bong Ju, 7239 Stratton Way, Baltimore, Md. 21224
SHIN, Byong H., 8510 16th St., 601, Silver Spring, Md.
SHIN, Kilman, Denison Univ., College Town House, Granville, Ohio 43023
SHIN, Paul, Univ. of Nebraska, Dept. of Econ., Lincoln 8, Neb.
SHINGLETON, Kenneth L., 3919 S. Jamestown, Tulsa, Okla. 74135
SHINN, Glenn L., 4545 Connecticut Ave., Washington 8, D.C.
SHINOHARA, Miyohei, Hitosubashi Univ., Inst. of Econ. Research, Kunitachi, Tokyo, Japan
SHIPLEY, Jerry J., Stanford Univ., Dept. of Econ., Stanford, Calif. 96128
SHIPMAN, William D., Bowdoin Col., Econ. Dept., Brunswick, Me. 04011
SHIPPER, David T., 65-74 Saunders St. Apt. 4B, Rego Park, N.Y. 13679
SHIRER, John, One Montgomery Ave., Apt. 203, Bala Cynwyd, Pa. 19004
SHIRLEY, David E., 4528 E. Water, Tucson, Ariz.
SHIRLEY, Robert V., Senate Courts Apt. 305, 120 C St., N.E., Washington 2, D.C.
SHIRVEN, Maynard N., 1600 River Farm Dr., Alexandria, Va. 22308
SHISHKIN, Boris B., AFL-CIO, 815 16th St., N.W., Washington, D.C. 20006
SHISHKIN, Julius, 8920 Whitney St., Silver Spring, Md. 20901
SHISHKO, Irwin, Int. Consultants Asso., Alewives Rd., Norwalk, Conn.
SHISTER, Joseph, State Univ. of New York at Buffalo, Sch. of Bus. Admin., Buffalo, N.Y. 14214
SHLAKMAN, Vera, 130 St. Edward St., Brooklyn 1, N.Y.
SHOEMAKER, James H., Bank of Hawaii, Honolulu, Hawaii 96802
SHOLTS, Edwin A., 2518 E. Mifflin St., Madison 4, Wis.
SHONTZ, Patricia O., 16550 Greenview, Detroit, Mich.
SHORT, Brock K., Cornell Univ., 57-B Hasbrouck Apts., Ithaca, N.Y. 14850
SHORT, William, RD #2, Goode St., Ballston Spa, N.Y. 12020
SHORTER, Fred C., Princeton Univ., Dept. of Econ. and Soc., Princeton, N.J. 08540
SHORTT, Charles E., R. 2, Box 1-A, Germantown, Md. 20767
SHOTWELL, Larry R., Fed. Res. Bank of Cleveland, Res. Dept., Cleveland, Ohio 44101
SHOUL, Bernice, 33 Nobscot Rd., Newton Center, Mass. 01355
SHOUP, Carl S., Columbia Univ., Fayerweather Hall, New York 27, N.Y.
SHOUP, Donald C., Inst. of Public Admin., 55 W. 44th St., New York, N.Y. 10036
SHOUP, Donald S., Powissett St., Dover, Mass.
SHROPSHIRE, William O., Emory Univ., Sch. Bus. Admin., Atlanta 22, Ga.

SHUBIK, Martin, Yale Univ., 24 Hillhouse Ave., New Haven, Conn.
SHUBIN, John A., 12 East 8th St., New York 3, N.Y.
SHUCHMAN, Abraham, Columbia Univ., Sch. of Bus., 116th and Broadway, New York 27, N.Y.
SHUCHTER, Jerome P., Fed. Pacific Elect. Co., Mktg. Serv., 50 Paris St., Newark, N.J. 07101
SHUFORD, Harry L., 2145 Yale Sta., New Haven, Conn.
SHULL, Bernard, 36 Orchard Way S., Rockville, Md. 20854
SHULMAN, James S., 31 Marcellus Dr., Newton Center, Mass. 02159
SHULMAN, (Mrs.) Mary H., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
SHULMAN, Robert B., Ford Motor Company, The American Rd., P.O. Box 207, Dearborn, Mich.
SHULOFF, Emil H., 1078 St. Johns Pl., Brooklyn, N.Y.
SHULTZ, George P., Univ. of Chicago, Sch. of Bus., Chicago, Ill. 60637
SHUMAN, Ronald B., Univ. of Oklahoma, Dept. of Bus. Mgt., Norman, Okla.
SHUPP, Franklin R., 225 David Kinley Hall, Urbana, Ill.
SHURBERG, Merwin, 1718 Madison Rd., Apt. 20, Columbia, S.C. 29204
SHUTE, John, 3536 Edmunds St., N.W., Washington, D.C. 20007
SHUTE, Laurence, Bard Col., Annandale-on-Hudson, N.Y. 12504
SHWINGER, Pini, 26305 Student St., Detroit, Mich. 48239
SHYAMALA, K. M., 101 Smith Lane, Syracuse, N.Y. 13210
SIBBALD, Garth H., Box 2267, Stanford, Calif.
SIBLEY, Vernon E., 2330 Fifth Ave. Apt. 5-L, New York, N.Y. 10037
SICAT, Gerardo P., Univ. of the Philippines, Inst. of Econ. Dev. and Res., Quezon City, Philippines
SICHEL, Werner, Western Michigan Univ., Dept. of Econ., Kalamazoo, Mich.
SIDHU, (Mrs.) Nancy D., 1835 Valley Rd., Champaign, Ill. 61822
SIDHU, Victor S., 1835 Valley Rd., Champaign, Ill. 61822
SIEBERT, Calvin D., Univ. of Iowa, Dept. of Econ., Iowa City, Iowa 52240
SIEGEL, Abraham J., Massachusetts Inst. of Tech., Rm. 52-453B, Cambridge, Mass. 02139
SIEGEL, Barry N., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
SIEGEL, Irving H., 8312 Bryant Dr., Bethesda, Md. 20034
SIEGEL, Irwin, Albertus Magnus Col., New Haven, Conn.
SIEGEL, Lewis B., 110 65th St., West New York, N.J. 07093
SIEGEL, Norbert K., 67-114 Dartmouth St., Forest Hills, N.Y. 11375
SIEGEL, Philip H., West Virginia State Col., Dept. of Bus. Admin., Institute, W. Va. 25112
SIEGELMAN, Louis, First Nat. Bank of Chicago, Econ. Dept., 38 S. Dearborn St., Chicago, Ill. 60690
SIEGMAN, Charles J., 307 Kent Rd., Bala-Cynwyd, Pa. 19004
SIELAFF, Mark, Univ. of Minnesota, Duluth Br., Div. of Soc. Studies, Duluth 12, Minn.
SIELAFF, Richard O., Univ. of Minnesota-Duluth, Div. of Soc. Studies, Duluth 12, Minn.
SIELAFF, Theodore J., San Jose State Col., Div. of Bus., San Jose 14, Calif.
SIENKIEWICZ, Casimir A., 201 Belmont Ave., Doylestown, Pa. 18901
SIENKIEWICZ, Edward W., Jr., Univ. of Oregon, Dept. of Econ., Eugene, Ore. 97403
SIEU, Luong The, Complex Ind. An-Hoa Nong-Son, 31, Han Thuyen, Saigon, Vietnam
SIEVERS, Allen M., Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla. 32603
SIEVWRIGHT, Eric C., 1554 Yonge St. Suite 202, Toronto 7, Ont. Canada
SIGAFOOS, Robert A., P.O. Box E, South Pasadena, Calif. 91031
SIGEL, Stanley J., Fed. Res. Bldg., Washington, D.C.
SIGUENZA, Manuel, Univ. Com. de Deusto, Bilbao, Spain
SIHLER, William W., Harvard Univ., Grad. Sch. of Bus. Admin., Soldiers Field, Boston, Mass. 02163
SIKORA, John, 2800 20th St., Boulder, Colo.
SIKORSKI, Bennie W., 274 Bizerte Circle, Ft. Lee, Va. 23801
SIKORSKI, Lawrence J., 305 Victoria Ave., Regina, Sask., Canada
SILBER, William L., 110 Montgomery St., Apt. 3P, Highland Park, N.J. 07422
SILBERBERG, Eugene, State Univ. of New York, Harpur Col., Binghamton, N.Y.
SILBERMAN, Irwin H., 430 E. 8th St., Apt. 614, Brooklyn, N.Y.
SILBERT, Myron S., 255 Atlantic Ave., 6th Fl., Boston, Mass. 02110
SILBERZWEIG, Robert, 38 Delsan Ct., Buffalo, N.Y. 14216
SILK, Leonard, c/o Business Week, 330 W. 42nd St., New York 36, N.Y.
SILVA-HERZOG, Jesus, F. Cerro De Xico 44, Mexico 21, D.F.
SILVA, Luis F., Urbanizacion Romany, 1867 San Joaquin St., Rio Piedras, P.R. 00745
SILVA, Norma, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
SILVER, Sidney J., 2407 Kelsey Lane, Bowie, Md. 20715
SILVERBERG, Stanley, Off. of the Comptroller of the Currency, Rm. 4438, U.S. Treasury, Washington 25, D.C.
SILVERMAN, Stanford B., 105-15 66th Rd., Forest Hills, New York, N.Y. 11375
SILVERSTEIN, Benjamin, 8 Loretta Dr., Syosset, L.I., N.Y. 11791
SILVERSTEIN, (Mrs.) Ethel B., 8 Loretta Dr., Syosset, N.Y. 11791
SILVERSTEIN, Nathan L., Indiana Univ., Sch. of Bus. Admin., Bloomington, Ind.
SILVERSTRAND, Clarance F., 1805 N. Quebec St., Arlington, Va. 22207
SILVESTRE, Joaquin, Maestro Falla 15 40, Barcelona 17, Spain
SILVIA, Peter A., 25 Forrest Ave., Trenton, N.J. 08638
SIM, Herbert E., Univ. of Notre Dame, Dept. of Fin., Notre Dame, Ind. 46556
SIMATUPANG, Sutan M., Djalam Pegangsaan Timur No. 17, Djakarta, Indonesia
SIMEONE, William J., 28 Howard Ave., North Providence, R.I.
SIMHA, S. L. N., Industrial Dev. Bank, Res. Bank of India Bldg., Bombay 1, India
SIMKO, Timothy G., 397 Orchard Pl., Haworth, N.J. 07641
SIMKOWITZ, Michael A., 680 West End Ave., New York 25, N.Y.
SIMLER, Norman J., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
SIMMONDS, Kenneth, 113 N. Bryan Ave., Bloomington, Ind.
SIMMONS, Andre, P.O. Box 3005, Addis Ababa, Ethiopia
SIMMONS, George, Columbia Univ., Grad. Sch. of Bus., New York 27, N.Y.
SIMMONS, George B., 2211 Dwight Way, Berkeley 4, Calif.
SIMMONS, Harold E., 1004 W. Cross St., Ypsilanti, Mich. 48197
SIMMONS, John D., 360 E. 72nd St., Apt. B-508, New York 21, N.Y.
SIMMS, Robert M., 108 Koch, Ann Arbor, Mich. 48103
SIMON, Julian L., 119 Gregory Hall, Urbana, Ill.
SIMON, Martin S., 1003 Chimney Ridge Dr., Mountainside, N.J. 07046
SIMON, Matthew, 2505 Aqueduct Ave., Bronx, N.Y.
SIMON, (Mrs.) Nancy W., 4526 Albemarle St., N.W., Washington, D.C. 20016
SIMON, Sidney I., Rutgers Univ., New Brunswick, N.J.
SIMON, Simon M., 70 Pinewood Gardens, Hartsdale, N.Y. 10530
SIMONE, Albert J., Boston Col., Bus. Admin., Chestnut Hill, Mass. 01012
SIMONS, Nat, U.S. Bur. of Pub. Rds., Div. of Res., 1717 H St., N.W., Washington, D.C. 20235
SIMONSEN, Mario H., Fundacao Getulio Var-

gas, Inst. Brasileiro de Econ., Caixa Postal 4081 ZC-05, Rio de Janeiro, Brazil
SIMONSON, Gene R., Long Beach State Col., Dept. of Econ., Long Beach 4, Calif.
SIMONSON, Richard C., 2929 University Ter., N.W., Washington, D.C. 20016
SIMPSON, Brian W., 46 Westland St., Bangor, Me.
SIMPSON, David, 73 Lower Baggot St., Dublin 2, Ireland
SIMPSON, Dwight J., Robert Col., Posta Kutusu 8, Bebek, Istanbul, Turkey
SIMPSON, Floyd O., California State Col. at Los Angeles, Los Angeles 32, Calif.
SIMPSON, Glenn L., Colorado State Univ., Dept. of Econ., Ft. Collins, Colo.
SIMPSON, James R., 5025 Tilden St., N.W., Washington 16, D.C.
SIMPSON, Jerry P., Louisiana State Univ. in New Orleans, Bus. Admin., New Orleans, La.
SIMPSON, John W., 9 S. Summit St., Ypsilanti, Mich. 48197
SIMPSON, Karl F., Jr., 603 N. Grove Ave., Oak Park, Ill. 60302
SIMPSON, Littleton W., 68 W. Kearsage Circle, Wallops Island, Va. 23337
SIMPSON, Louis A., Stein, Roe, and Farnham, 135 S. La Salle St., Chicago, Ill. 60603
SIMPSON, Merlin C., Jr., c/o Mr. Lino Martini, P.O. Box 673, Linden, Calif. 95236
SIMPSON, Paul B., 2720 Elinor, Eugene, Ore.
SIMPSON, Robert E., 10315 Riverwood Dr., Potomac, Md. 20854
SIMPSON, Thomas D., 2016 E. 72nd St., Chicago, Ill. 60649
SIMPSON, William B., 1700 Grand View Dr., Apt. 4, Alhambra, Calif. 91803
SIMPSON-LEE, G. A. J., Univ. of Sydney, Dept. of Econ., Sydney, N.S.W., Australia
SIMS, Clarence A., 1121 Stafford, De Kalb, Ill. 60115
SIMS, J. T., Jr., Univ. of Illinois, Dept. of Mktg., Urbana, Ill. 61803
SINAI, Allen L., 165 Dover Dr., Apt. 13, Des Plaines, Ill.
SINCLAIR, Thomas J., 830 Tran. Bldg., Washington 6, D.C.
SINES, William W., Jr., Kalamazoo Col., 209 Hoben Hall, Kalamazoo, Mich.
SING, Francis P., 405 E. 16th St., Brooklyn 26, N.Y.
SINGER, David, 265 W. 72nd St., New York 23, N.Y.
SINGER, H. W., 146-28 Beech Ave., Flushing, N.Y. 11355
SINGER, Jacques J., 16 Aldbury Gardens, Toronto 12, Ont., Canada
SINGER, Morris, Univ. of Connecticut, Econ. Dept., Storrs, Conn. 06268
SINGER, Neil M., 8117 Lilly Stone Dr., Bethesda, Md. 20034
SINGER, Robert, 28 Burbury Lane, Great Neck, N.Y. 11020
SINGER, Sanford R., Lone Star Gas Co., Res. and Dev. Dept., 301 S. Harwood, Dallas 1, Tex.
SINGH, Ajmer, Oregon Col. of Educa., Dept. of Soc. Sci., Monmouth, Ore. 97361
SINGH, Balwant, Univ. of Pennsylvania, W-32 Dietrich Hall, Philadelphia, Pa. 19104
SINGH, Balwir, Allegheny Col., Dept. of Econ., Meadville, Pa. 16335
SINGH, Inderjit, 61 South St., Northampton, Mass.
SINGH, Sarjit, Clarion State Col., Dept. of Pub. Instruction, Clarion, Pa. 15025
SINGHAL, (Mrs.) Prem L., 1724 17th St., N.W., #73, Washington, D.C. 20009
SINGLETON, Carey B., Jr., 4908 Hollywood Rd., College Park, Md.
SINHA, J. N., Inst. of Econ. Growth, University Enclave, Delhi 7, India
SINHA, Rohini P., 35 N. 18th St., Allentown, Pa.
SINICROPI, Anthony V., Univ. of Iowa, Col. of Bus. Admin., Phillips Hall, Iowa City, Iowa 52240
SIRAGEL-DIN, Ismail, Univ. of Michigan, Inst. for Soc. Res., Survey Res. Center, Ann Arbor, Mich. 48106
SIRAK, Eyasu, Min. of Nat. Community Dev., P.O. Box 2056, Addis Ababa, Ethiopia
SIRIMATYA, Rit, Chiengmai Univ., Fac. of Soc. Sci., Chiengmai, Thailand
SIRKEN, Irving A., World Bank, 1818 H St., N.W., Washington 25, D.C.
SIRKIN, Gerald, 845 West End Ave., Apt. 15-B, New York, N.Y. 10025
SISKIND, David I., 304 Cavalier Ct., Silver Spring, Md.
SISLEY, Charles D., South-Western Pub. Co., Col. Dept., 5101 Madison Rd., Cincinnati, Ohio 45227
SISON, Rafael A., Baltimore City Dept. of Plan., Res. and Anal. Sect., 1200 Amer. Bldg., Baltimore, Md. 21202
SISSMAN, (Mrs.) Louise, Hotel Roosevelt, 2101 16th St., N.W., Washington, D.C. 20009
SITTON, Gordon R., 3105 Garfield St., Corvallis, Ore.
SIVAN, Yehuda, 17 Bnei-Moshe St., Tel-Aviv, Israel
SIVARD, Ruth L., 3013 Dumbarton Ave., N.W., Washington 7, D.C.
SJAASTAD, Larry A., Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
SJAH, Sutan M., Djalan Ternate 1, Djakarta, Indonesia
SJOBERG, Arne, Valhallavagen 77, Stockholm O., Sweden
SJOGREN, Per H., Oregon State Univ., Dept. of Bus. Admin., Corvallis, Ore. 97331
SJOQUIST, Andrew L., Itasca Junior Col., Coleraine, Minn. 55722
SKADBERG, J. Marvin, Iowa State Univ., Dept. of Econ., 411 East Hall, Ames, Iowa 50010
SKAGGS, Allen H., 1504 Bent Branch Ct., Falls Church, Va. 22041
SKALA, Charles E., c/o Kelly Girl Ser., Inc., P.O. Box 3589, Detroit 3, Mich.
SKALA, Hugo M., 38 Washington Ave., Rutherford, N.J.
SKEELS, Jack W., Northern Ill. Univ., Dept. of Econ., De Kalb, Ill. 60115
SKEELS, (Mrs.) Joyce G., Northern Ill. Univ., Dept. of Econ., De Kalb, Ill. 60115
SKEHAN, Joseph T., 220 Ward Pl., South Orange, N.J. 07079
SKELLY, Daniel F., 219 Beach 96 St., Rockaway Beach, N.Y.
SKEOCH, Lawrence A., Queens Univ., Dept. of Econ., Kingston, Ont., Canada
SKINNER, George A., N. E. Telephone Co., 185 Franklin St., Rm. 1101, Boston, Mass. 02107
SKINNER, Gordon, 6003 Connecticut Ct., Cincinnati 24, Ohio
SKINNER, John W., 8 Maplewood Dr., Guelph, Ont., Canada
SKOGSTAD, S. L., 4053 Arden Way, N.E., Atlanta, Ga. 30305
SKOK, Richard A., Univ. of Minnesota, Sch. of Forestry, St. Paul 1, Minn.
SKOLD, Melvin D., Kansas State Univ., Dept. of Econ. and Soc., Waters Hall, Manhattan, Kans. 66504
SKONBERG, Carl M., 6655 Van Winkle Dr., Falls Church, Va. 22044
SKOV, Iva L. M., 1100 Mar Vista, Seal Beach, Calif. 90740
SKOWBO, Neal F., 556 W. Parkway Rd., Riverside, Ill.
SKRALY, Emile B., 50 Woodbine Dr. E., Freeport, L.I., N.Y. 11520
SLABE, James F., Headquarters 502nd Military Intelligence Bn., APO San Francisco 96301
SLABY, (Mrs.) Carolyn K., P.O. Box 594, State College, Miss. 39762
SLACK, (Mrs.) Eileen S., 2100 Connecticut Ave., Washington, D.C. 20008
SLASOR, George, Univ. of Toronto, Dept. of Polit. Econ., Toronto 5, Ont., Canada
SLATE, Daniel M., Univ. of Illinois, Dept. of Mktg, 350-E Commerce West, Urbana, Ill.
SLATER, Charles C., 1939 Osage Dr., Okemos, Mich. 48864
SLATER, (Mrs.) Courtenay M., 5313 2nd St., N., Arlington 3, Va.
SLATER, David W., Queen's Univ., Dept. of Econ., Kingston, Ont., Canada
SLATER, George R., Harris Trust and Savings Bank, 111 W. Monroe St., Chicago, Ill. 60690
SLATER, J. E., Coverdale and Colpitts, 120 Wall St., New York, N.Y.

SLATIN, Benjamin, 38-23 Leslie Pl., Fair Lawn, N.J.
SLAVICK, Fred, Cornell Univ., New York State Sch. of Ind. Rela., Ithaca, N.Y.
SLAYMAN, W. O., Kaiser Aluminum and Chemical Sales, Inc., 300 Lakeside Dr., Oakland, Calif. 94604
SLEETER, Howard E., 2110 Sanborn St., Port Huron, Mich.
SLESINGER, Reuben E., Univ. of Pittsburgh, Dithridge House, Pittsburgh, Pa. 15213
SLETTEN, (Mrs.) Irma, Wisconsin State Univ., Dept. of Econ., Eau Claire, Wis.
SLIGER, Bernard F., 475 Magnolia Wood Dr., Baton Rouge, La. 70808
SLITER, Richard E., 9000 Burning Tree Rd., Bethesda, Md. 21811
SLOAN, Virginia B., 3828 Shellbrook Dr., Ft. Worth, Tex. 76109
SLOANE, Keith, Australian National Univ., Box 197, Canberra City, ACT, Australia
SLOANE, Peter E., Smith Col., Dept. of Econ., Wright Hall, Northampton, Mass.
SLOSS, James, 605 Skokie Lane, Glencoe, Ill. 60022
SLOTKIN, Richard L., 1907 S. Shenandoah St., Los Angeles, Calif. 90034
SLUTTER, Carl G., Sr., P.O. Box 522, Denville, N.J. 07834
SMALE, John G., Chico State Col., Chico, Calif.
SMALL, Albert H., 7119 Braeburn Pl., Bethesda, Md. 21811
SMALL, Ida F., 56 Concord Ave., Cambridge, Mass. 02138
SMALL, Joseph T., 300 E. 57th St., Apt. 13-G, New York 22, N.Y.
SMALL, Leslie W., F 104 Banta, Bloomington, Ind.
SMALL, Sylvia S., 7119 Braeburn Pl., Bethesda, Md. 21811
SMALLWOOD, Benjamin D., 2515-R St., S.E., Apt. 127, Washington, D.C. 20009
SMEDLEY, Lawrence T., 4820 Eades St., Rockville, Md.
SMELKER, (Mrs.) Mary W., U.S. Dept. of Labor, Bur. of Labor Statis., G.A.O. Bldg., Rm. 2016, Washington, D.C.
SMELKER, Merlin, U.S. Dept. of Labor, Bur. of Labor Statis., G.A.O. Bldg., Rm. 2016, Washington, D.C.
SMIDT, Seymour, B.P.A., Cornell Univ., Ithaca, N.Y.
SMIGEL, Stanley E., 5020 McCall St., Rockville, Md.
SMILAND, Mitchell M., Blackburn Col., Dept. of Econ., Carlinville, Ill. 62626
SMILEY, David J., P.O. Box 41, Newport, Tenn. 37821
SMIT, Jan E. Gustaaf, c/o The Shell Co., (W.I.) Ltd., Apartado 767, Panama, Republic of Panama
SMITH, Alfred G., Jr., Univ. of South Carolina, Dept. of Econ., Columbia, S.C.
SMITH, Arthur A., First Nat. Bank, Dallas, Tex.
SMITH, Arthur J. R., Econ. Coun. of Canada, Royal Trust Bldg., P.O. Box 527, Ottawa, Ont., Canada
SMITH, Arthur L., 6204 Cumberland Ave., Apt. 203, Springfield, Va. 22150
SMITH, Austin M., Jr., 104 W. Florida, Urbana, Ill.
SMITH, Bernard J., 22 Sea Cliff Ave., San Francisco 21, Calif.
SMITH, Bernard J. C., 22 Sea Cliff Ave., San Francisco, Calif. 94121
SMITH, Bracy D., U.S. Steel Corp., 71 Broadway, New York, N.Y. 10006
SMITH, Bradford B., 7 Heathcote Rd., Scarsdale, N.Y.
SMITH, Carl L., 225 University Ave., Elyria, Ohio
SMITH, Carol L., 114 Susanna Wesley Hall, Albion, Mich. 49224
SMITH, Cecil N., USAID/Fla. Contract, U. S. Embassy, San Jose, Costa Rica
SMITH, Charles W., 1810 Park Ave., Baltimore 27, Md.
SMITH, Colin L., 4 Seminary St., Casenovia, N.Y.
SMITH, Dan T., Morgan Hall, Soldiers Field, Boston, Mass. 02163
SMITH, David B., 351 Soule, Apt. 401, Laramie, Wyo.
SMITH, David C., Queen's Univ., Dept. of Econ., Kingston, Ont., Canada
SMITH, David K., Chipman Heights, Middlebury, Vt. 05753
SMITH, David L., Univ. of Arizona, Bur. of Bus. and Pub. Res., Tucson, Ariz. 85721
SMITH, Dickson K., USAID, APO 271, New York 09271
SMITH, Edmund A., Univ. of Portland, Col. of Bus. Admin., Portland 3, Ore.
SMITH, Edward J., U.S.D.A., Econ. Res. Ser., Farm Prod. Econ. Div., Washington, D.C. 22250
SMITH, Edward K., 7839 Midday Lane, Alexandria, Va. 22306
SMITH, Edward L., 676 Riverside Dr., New York, N.Y. 10031
SMITH, Edward N., 374 N. Rengstorff Ave., Mountain View, Calif.
SMITH, (Mrs.) Elizabeth M., Marquette Univ., Col. of Bus. Admin., Milwaukee, Wis. 53233
SMITH, Emery W., Jr., Concord Col., Box 216, Athens, W.Va. 24712
SMITH, F. DeVere, 319 King St., Columbia 5, S.C.
SMITH, Frank A., Univ. of Mass., Dept. of Econ., Amherst, Mass.
SMITH, Frank A., 8912 Walden Rd., Silver Spring, Md.
SMITH, Frank G. R., 15 Layton St., Freeport, N.Y. 11520
SMITH, Frank L., 123 Highland Lane, Mill Valley, Calif.
SMITH, G. Ralph, Loyola Univ., 6363 St. Charles Ave., New Orleans, La. 70118
SMITH, George A., Jr., Harvard Univ., Grad. Sch. Bus. Admin., Soldiers Field, Boston, Mass.
SMITH, George C., Jr., c/o Mackay-Shields, Inc., 551 Fifth Ave., New York 17, N.Y.
SMITH, Gerald E., 9220 Carpenter Rd., Flushing, Mich. 48433
SMITH, Gerard C., 3508 Moylan Dr., Bowie, Md. 20715
SMITH, Gilbert N., 326½ N. 3rd St., River Falls, Wis. 54022
SMITH, Glen R., 138 Westdale Ave., Decatur, Ill.
SMITH, Hadley E., Inst. of Pub. Admin., Univ. Col., P.O. Box 616, Dar Es Salaam, Tanzania
SMITH, Harlan M., Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
SMITH, Harold C., 215 W. 259th St., New York 71, N.Y.
SMITH, Harold T., 300 S. Westnedge Ave., Kalamazoo, Mich. 49007
SMITH, Hayden W., Esso Standard Eastern, Inc., 15 W. 51st St., New York, N.Y. 10020
SMITH, Herbert D., City of Indianapolis, Dept. of Redev., 1942 City-County Bldg., Indianapolis, Ind. 46204
SMITH, (Mrs.) Hilda C., Loyola Univ., 6363 St. Charles Ave., New Orleans, La. 70118
SMITH, Howard F., USAID, APO New York 09662
SMITH, J. N., 606 E. Wayland St., Springfield, Mo. 65804
SMITH, James D., 7819 Lee Ave., Alexandria, Va. 22308
SMITH, James D., Box 121, St. Benedict Col., Atchison, Kans. 66002
SMITH, James F., Sears Roebuck Co., Mktg. Res. Dept., 2000 Spring Rd., Oak Brook, Ill. 60523
SMITH, James P., Duke Univ., Dept. of Econ., Durham, N.C.
SMITH, (Mrs.) Janet R., 1673-B Oxford St., Berkeley, Calif. 94709
SMITH, Jay A., Jr., 3204 Toledo Pl., Apt. 202, Hyattsville, Md.
SMITH, John D., 1823 W. John St., Champaign, Ill. 61822
SMITH, Joseph E., Youngstown Col., Youngstown, Ohio
SMITH, Keith V., Purdue Univ., Dept. of Econ., West Lafayette, Ind.
SMITH, Kenneth H., Hunter Col., Econ. Dept., 695 Park Ave., New York 21, N.Y.
SMITH, Kenneth R., 629 Noyes St., Evanston, Ill. 60201

SMITH, L. George, Northern Illinois Univ., Col. of Bus., De Kalb, Ill. 60115
SMITH, Lawrence, 88 Leighton Rd., Wellesley, Mass. 02181
SMITH, Lawrence B., 395 Broadway, Apt. 553, Cambridge, Mass. 02139
SMITH, Lawrence G., 1344 Viola St., San Diego, Calif. 92110
SMITH, Sister Maria P., Col. of Mt. St. Vincent, New York 71, N.Y.
SMITH, Melvin W., R. 8, Cullman, Ala. 35055
SMITH, Nelson L., P.O. Box 603, Madison, Conn. 06443
SMITH, Newlin R., Tufts Univ., Dept. of Econ., Medford, Mass. 02155
SMITH, Norbert J., 225 E. 36th St., New York, N.Y. 10016
SMITH, Paul E., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
SMITH, Paul F., 31 Highview Dr., Radnor, Pa. 16144
SMITH, Paul H., Miami Dade Col., Bus. Admin. Dept., 11380 N.W. 27th Ave., Miami, Fla. 33167
SMITH, Peter F., 168 Milltown Rd., East Brunswick, N.J. 08816
SMITH, R. Elberton, AID, APO New York 09254
SMITH, Ralph W., Jr., 3407 Tulane Dr., Apt. 14, West Hyattsville, Md. 20783
SMITH, Richard C., Univ. Missouri, Sch. of Forestry, Columbia, Mo.
SMITH, Robert C., 1108 Trinity Dr., Alexandria, Va. 22314
SMITH, Robert D., Jr., Lyons St., Ludlow, Mass.
SMITH, Robert E., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
SMITH, Robert F., 2412 July St., Baton Rouge, La.
SMITH, Robert G., Fin. Programs, Inc., 950 Broadway, Denver, Colo.
SMITH, Robert H., Elmendorf and Co., Inc., 1640 Union Com. Bldg., Cleveland, Ohio 44115
SMITH, Robert J., 5502 S. Everett Ave., Chicago, Ill. 60637
SMITH, Robert S., Duke Univ., Dept. of Econ., Durham, N.C.
SMITH, Rodney F., C-6 Forest Park, Durham, N.H. 03824
SMITH, Roy J., 8158 Sherwood Pl., Riverside, Calif.
SMITH, Saul I., 2400 Nostrand Ave., Brooklyn, N.Y. 11210
SMITH, Stanley A., 3280 Nostrand Ave., Brooklyn, N.Y. 11229
SMITH, Stephen C., 1402 Peterson St., Ft. Collins, Colo.
SMITH, Stewart J., 2107 Suitland Ter., Apt. 201, Washington 20, D.C.
SMITH, T. Arthur, 1609 Simmons Ct., McLean, Va. 22101
SMITH, Thomas Lee, 5808 Warwick Pl., Chevy Chase, Md. 20015
SMITH, Tynan, 1135 Cameron Rd., Wellington, Alexandria, Va. 22308
SMITH, Vernon L., Purdue Univ. Sch. of Ind. Mgt., Dept. of Econ., Lafayette, Ind. 47178
SMITH, Victor E., Michigan State Col., Dept. of Econ., East Lansing, Mich.
SMITH, Wallace F., Univ. of California, Sch. of Bus. Admin., 350 Barrows Hall, Berkeley, Calif. 94720
SMITH, Walter A., Univ. of Tulsa, Tulsa, Okla.
SMITH, Warren L., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48104
SMITH, Wil J., Univ. of Wisconsin, Univ. Ext. Div., 802½ First St., Wausau, Wis. 54401
SMITH, Willard S., 6613 Denny Pl., McLean, Va. 22101
SMITH, William J. J., 167 E. 67th St., 17-B, New York 21, N.Y.
SMITH, William P., Fed. Res. System, Banking Mkts. Unit, Washington, D.C. 20551
SMITH, William R., 419 E. Cass, Apt. 1, Albion, Mich. 49224
SMITH, William R., CMR #1, Box 935, APO Seattle, Wash. 98742
SMITH, William Y., Hqs. USAFE DCS/O, APO New York 09633
SMITH, Winfield S., 5825 S. Blackstone Ave., Chicago, Ill. 60637
SMITH, Zennis, P.O. Box 22—Sta. I, Avondale, Cincinnati, Ohio 45229
SMITHIES, Arthur, Harvard Univ., Dept. of Econ., Cambridge, Mass. 01922
SMOLENSKY, Eugene, 1470 E. 56th St., Chicago, Ill. 60637
SMOLINSKI, Leon, Boston Col., Dept. of Econ., Chestnut Hill, Mass. 01012
SMOLOWITZ, Ira E., 30 Ave. V., Apt. 4-B, Brooklyn, N.Y. 11223
SNARE, John L., Duke Univ., Econ. Dept., Durham, N.C. 27705
SNAVELY, Carl M., Jr., W. B. Saunders and Co., 1625 I St., N.W., Washington 6, D.C.
SNAVELY, Tipton R., Univ. of Virginia, Dept. of Econ., University Sta., Charlottesville, Va.
SNAVELY, William P., Univ. of Connecticut, Box U-63, Storrs, Conn. 06268
SNELL, David A., Texas Nat. Bank of Com., P.O. Box 2558, Houston, Tex. 77001
SNELL, Hampton K., Univ. of Texas, 712 BEOB, Austin, Tex. 78712
SNELL, Warner C., Delta State Col., Box 331, Cleveland, Miss. 38732
SNELLINGS, Aubrey N., Fed. Res. Bank, Res. Dept., Richmond, Va.
SNIDER, Delbert A., Miami Univ., Oxford, Ohio 45056
SNIDER, Thomas E., Univ. of Minnesota, Dept. of Agric. Econ., St. Paul, Minn. 55101
SNODGRASS, Donald R., c/o Ford Found., P.O. Box 2255, Kuala Lampur, Malaysia
SNODGRASS, Milton M., Purdue Univ., Dept. of Agric. Econ., Krannert Bldg., Rm. 686, Lafayette, Ind. 47907
SNYDER, Barth J., 3270 Bellbrook-Fairfield Rd., Bellbrook, Ohio 45305
SNYDER, Carl D., 939 Raymond St., Ann Arbor, Mich.
SNYDER, Christopher L., Jr., Rd. 3, Bethlehem, Pa. 18015
SNYDER, Edward P., 710 Villa Ridge Rd., Falls Church, Va. 22046
SNYDER, Eleanor M., 145 Fourth Ave., Apt. 13B, New York, N.Y.
SNYDER, Jack H., 102-A Eagle Heights, Madison, Wis. 53705
SNYDER, James C., 216 Pawnee Dr., West Lafayette, Ind. 47178
SNYDER, Jerome, 3709 Calvert Pl, Kensington, Md. 20795
SNYDER, Richard E., 221 N. La Salle St., Suite 416, Chicago, Ill. 60601
SNYDER, Richard M., Plantation Key, R. 1, Islamorada, Fla. 33036
SNYDER, Robert E., 350 Irvington Ave., Apt. 510, Elizabeth, N.J.
SNYDER, Russell A., 1310 N. 9th St., Fargo, N.D. 58102
SNYDER, Theodore R., Jr., Box 24 J-2 USTDC, APO San Francisco 96263
SNYDER, Thorn K., Arabian Amer. Oil Co., Box 2601, Dhahran, Saudi Arabia
SNYDER, Wayne W., 130 Bd. du Montparnasse, Paris 14e, France
SOARES, Pedro P., Rua Almirante Alexandri, No. 787, apte 301, Santa Teresa, Rio de Janeiro, Brazil
SOBECZEK, Clarence G., 1331 Schley Ave., San Antonio, Tex.
SOBEL, Irvin, 8447 Colonial Lane, Ladue, Mo. 64758
SOBERON, Oscar M., Luis Kuhne 10, Mexico D.F. 20, Mexico
SOBIN, Bernard, 11504 Yates St., Silver Spring, Md. 20902
SOBOL, (Mrs.) Marion B., 4 Dawson Rd., Kendall Park, N.J. 08824
SOBOTKA, Stephen P., Shell Oil Co., 50 W. 50th St., New York 20, N.Y.
SOCAS, Salvador C., Univ. of Minnesota, Dept. of Agric. Econ., 302 Haecker Hall, St. Paul, Minn. 55101
SODERHOLM, Thor R., P.O. Box 1583, Madison, Wis.
SODERMAN, Robert E., James Beam Distilling Co., Mkt. Res. Dept., 65 E. South Water St., Chicago, Ill. 60601
SODERSTEN, Bo, Univ. of California, Dept. of Econ., Berkeley, Calif. 94720
SODERSTROM, Lee A., 1922 Virginia St., Apt. 11, Berkeley, Calif. 94709

SOFFER, Benson, 103 G St., S.W., Apt. B-118, Washington, D.C. 20024
SOHMEN, Egon, Univ. Des Saarlandes, Europaisches Forschungs, Saarbrucken 15, Germany
SOIFFER, Joseph, 215 Calle de Arboles, Redondo Beach, Calif. 90277
SOKOL, John, Columbia Univ., Teachers Col., New York, N.Y. 10027
SOKOL, Jose B., Apartado 3395, Panama 4, Rep. de Panama
SOKOLOFF, Joel J., 243 Wortman Ave., Brooklyn, N.Y. 11207
SOKOLSKI, Alan, 915 Hyde Rd., Silver Spring, Md. 20902
SOLANA, J. R., Amer. Embassy (USAID), Managua, Nicaragua
SOLAR, Donald, 1591 Bruckner Blvd., Bronx 72, N.Y.
SOLDAN, Thomas D., 14016 Rossini Dr., Detroit, Mich. 48205
SOLDOFSKY, Robert M., 229 Lowell, Iowa City, Iowa
SOLECKI, Jan, Univ. of British Columbia, Dept. of Slavonic Studies, Vancouver 8, B.C., Canada
SOLEH, Mohannad, Fakultas Ekonomi, P.O. Box 100, Makassar, Indonesia
SOLGANICK, Allen, Univ. of Maryland, Dept. of Econ., College Park, Md. 20742
SOLIE, Richard J., Centre Col., Dept. of Econ., Danville, Ky. 40422
SOLIGO, Ronald, Yale Univ. Econ. Growth Center, Box 1984, Yale Sta., New Haven, Conn.
SOLLMAN, (Mrs.) Theresa S., 4320 Dresden St., Kensington, Md. 20795
SOLMON, Lewis C., 7250 S. Shore Dr., Apt. 203, Chicago, Ill. 60649
SOLMSSEN, K. A., 1530 Spring Garden St., Philadelphia 1, Pa.
SOLOMON, Benjamin, 5432 S. Woodlawn, Chicago, Ill. 60615
SOLOMON, E. B., 2515 10th Ave., #9, Oakland, Calif. 94606
SOLOMON, Ezra, Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 94305
SOLOMON, Frederic, Bd. of Gov. of the Fed. Res. System, Washington, D.C. 20551
SOLOMON, Henry, George Washington Univ., Logistics Res. Project, 707 22nd St., N.W., Washington 7, D.C.
SOLOMON, Louis A., 256 Barnard Rd., Larchmont, N.Y. 10538
SOLOMON, Martin B., Jr., 1362 Bordeaux, Lexington, Ky.
SOLOMON, (Mrs.) Miriam A., 404 E. Indian Spring Dr., Silver Spring, Md. 20901
SOLOMON, Morris J., 404 E. Indian Spring Dr., Silver Spring, Md. 20901
SOLOMON, Robert, Fed. Res. Bd., Washington 25, D.C.
SOLOMONIDES, Panos D., P.O. Box 210, Limassol, Cyprus
SOLOMONS, David, Univ. of Pennsylvania, Wharton Sch. of Fin. and Com., Dept. of Acctg., Philadelphia 4, Pa.
SOLON, Babette S., 800 Grand Concourse, New York, N.Y. 10451
SOLORZANO, Tulio, Banco Central de Nicaragua, Managua, D.N., Nicaragua
SOLOW, Robert, Massachusetts Inst. of Tech., Dept. of Econ. and Soc. Sci., Cambridge, Mass. 01922
SOLOWAY, Arnold M., Jamaicaway Tower, Boston, Mass. 02130
SOLSON, Stanley J., 10 St. Lukes Pl., New York, N.Y. 10014
SOLTANI, Manoucher, 3217 Toledo Pl., Hyattsville, Md. 20782
SOLTERER, Josef, 518 Meridian Ave., Arlington 13, Va.
SOLTIS, Charles, Kent State Univ., Col. of Bus. Admin., Kent, Ohio
SOLTOW, Allen R., Texas Western Col., Box 26, El Paso, Tex. 79902
SOLTOW, Lee C., 47 Madison Ave., Athens, Ohio 45701
SOMBERG, Seymour I., Proyecto Forestal, UNSF no. 116, Apartado 1407, Lima, Peru
SOMERS, Gerald G., Univ. of Wisconsin, Dept. of Econ., Madison 6, Wis.
SOMERS, Harold M., Univ. of California, Dept. of Econ., Los Angeles 24, Calif.
SOMMERS, Albert T., 17 Burbury Lane, Great Neck, N.Y.
SONG, Myung, 4209 Baltimore, Philadelphia, Pa.
SONENBLUM, Sidney, 2939 Van Ness St., N.W., Washington, D.C.
SONEVYTSKY, Rostyslaw, 2466 Webb Ave., New York 68, N.Y.
SONNETT, John P., 80 Pine St., New York 5, N.Y.
SONNINO, Mario A., 2023 Jameson St., Hillcrest Heights, Md. 20031
SONNY, Jacob, 108 De Kalb Ave., Jersey City, N.J.
SONSTEGAARD, Miles H., Ind. Res. and Extension Center, P.O. Box 3017, Little Rock, Ark.
SOONTHORNSIMA, C., UN Econ. Com. for Asia and Far East, Res. and Plan. Div., Sala Santitham, Bangkok, Thailand
SOPER, John C., Hampton House, Apt. 7, 77 Belchertown Rd., Amherst, Mass. 01002
SOPIARZ, Edward R., Univ. of Toledo, Dept. of Econ., Toledo 6, Ohio
SORENSEN, Duane L., 1612 W. William, Champaign, Ill. 61822
SORENSON, Philip E., Univ. of Calif., Dept. of Econ., Santa Barbara, Calif. 93106
SORENSON, Gary, Washington State Univ., Econ. Dept., Pullman, Wash.
SORGE, Barthold W., 723 Muskingum Ave., Pacific Palisades, Calif. 90272
SORIA, Raul, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
SORICH, Richard, 425 Riverside Dr., New York 25, N.Y.
SORRENSON, Gerald F., California State Col., 5151 State College Dr., Los Angeles 32, Calif.
SOSIN, (Mrs.) Kim, 2526 A St., Lincoln, Neb.
SOSNICK, Stephen H., 621 Elmwood Dr., Davis, Calif. 95616
SOSNOVY, T., 2709 30th St., S.E., Washington 20, D.C.
SOTILLO, Jeronimo, Qta. Cecilia, Calle Maracaibo, Prados del Este, Caracas, Venezuela
SOUDEK, Josef, 900 W. End Ave., New York, N.Y. 10025
SOUDER, William E., 1331 Somoa Dr., St. Louis 26, Mo.
SOUKUP, Gervase J., St. Johns Univ., Collegeville, Minn. 56321
SOULE, George, 31 Gibson St., Cambridge, Mass. 02138
SOUTHALL, John L., Jr., Kentucky State Col., Dept. of Econ., Frankfort, Ky. 40601
SOUTHARD, Frank A., Jr., Int. Monetary Fund, 19th and H Sts., N.W., Washington 25, D.C.
SOUTHERN, John H., 3809 Forest Grove Dr., Annandale, Va. 22003
SOUTHWICK, Lawrence, Jr., 5715 Elmer St., #1, Pittsburgh 32, Pa.
SOUTHWORTH, Constant, 4000 Cathedral Ave., Washington, D.C.
SOVEREIGN, Michael G., 203 Bliss Dr., Urbana, Ill. 63801
SOWELL, Thomas, Cornell Univ., Dept. of Econ., Ithaca, N.Y. 14850
SPADARO, Louis M., 1670 W. 8th St., Brooklyn, N.Y. 11223
SPAETHE, Jurgen, European Inst. of Bus. Admin., 3 rue du Pere Jacques, Fontainebleau (Seine-et-Marne), France
SPAFFORD, Carroll C., 103 Springview Dr., Battle Creek, Mich. 49017
SPAHR, Walter E., 8 Michigan Ave., Bellerose, L.I., N.Y. 14711
SPAIN, Harold L., 1711 Massachusetts Ave., N.W., Washington, D.C. 20036
SPALDING, James B., Jr., Bradley Univ., Dept. of Mktg., Peoria, Ill. 61606
SPANGLER, Harold G., Box 57233, Webster, Tex. 77598
SPANGLER, Miller B., St. Louis Regional Ind. Dev. Corp., 7701 Forsyth Blvd., St. Louis, Mo. 63105
SPANGLER, Richard C., Fresno State Col., Dept. of Econ., Fresno 26, Calif.
SPANOS, Theodore G., 119-02 9th Ave., College Point, N.Y. 11356
SPARKS, Gordon R., Massachusetts Inst. of Tech., Dept. of Econ. and Soc. Sci., Cambridge, Mass.

SPARKS, Jared, Jr., Univ. of Arkansas, Col. of Bus. Admin., Fayetteville, Ark.
SPARLING, Ronald L., Nutt Apt. D-100, Bloomington, Ind. 47403
SPARROW, Dorothy G., 192 Wiswall Rd., Newton Centre, Mass. 02159
SPAULDING, J. L., Bethel Col., North Newton, Kans. 67117
SPAULDING, Kenneth C., B 2522, E.T.S.U., Johnson City, Tenn.
SPEAGLE, Richard E., 223 Snowden Lane, Princeton, N.J.
SPEAKER, Lawrence M., 6210 Kimbark Ave., Chicago, Ill. 60637
SPECE, Paul S., Central Michigan Univ., Sch. of Bus., Mt. Pleasant, Mich. 48858
SPECHLER, Jay, 1903 Portsmouth, Apt. 15, Houston, Tex. 77006
SPECHLER, Martin C., 5C Gibson Ter., Cambridge, Mass. 01922
SPECTOR, Norman M., 4 The Tulips, Roslyn Estates, L.I., N.Y.
SPENCE, Ernest J., 166 Lytton Blvd., Toronto, Ont., Canada
SPENCER, Austin H., Wisconsin State Col., Dept. of Econ., La Crosse, Wis.
SPENCER, Byron G., Rice Univ., Dept. of Econ., Houston, Tex. 77001
SPENCER, Daniel L., Howard Univ., Dept. of Econ., Washington, D.C.
SPENCER, Francis A., 4259 Dempster St., Skokie, Ill. 60077
SPENCER, Milton H., Wayne State Univ., Sch. of Bus. Admin., Detroit 2, Mich.
SPENCER, Myron J., WGBH-TV (Educ.), Dept. of Pub. Affairs, 125 Western Ave., Boston, Mass. 02134
SPENCER, R. B., 74 Babcombe Dr., Thornhill, Ont., Canada
SPENGLER, Edwin H., 134 Fonda Rd., Rockville Centre, N.Y. 11570
SPENGLER, Joseph J., Duke Univ., Durham, N.C. 27706
SPERLING, Celia, 2704 Arundel Rd., Mt. Ranier, Md. 20822
SPERRY, Jeffrey M., 5317 Holmes, Kansas City, Mo.
SPETH, Conrad W., 16552 S.E. 28th St., Bellevue, Wash. 98004
SPICHIGER, Jean R., Univ. de Puerto Rico, Dept. de Ciencias Soc., Mayaguez, P.R. 00709
SPIEGEL, Harold R., c/o Arabian Amer. Oil Co. (ARAMCO), Dhahran, Saudi Arabia
SPIEGEL, Henry W., Catholic Univ. of Amer., Dept. of Econ., Washington, D.C.
SPIEGELGLAS, Stephan, Temple Univ., Sch. of Bus. Admin., Philadelphia 22, Pa.
SPIEGELMAN, Robert G., 6 Manor Pl., Menlo Park, Calif.
SPIELMANS, John V., 733 N. 12th St., Milwaukee 3, Wis.
SPIER, Leo, Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
SPILDE, Roger H., Concordia Col., Moorhead, Minn.
SPILLER, Earl A., Jr., Washington Univ., Sch. of Bus., St. Louis 5, Mo.
SPILLMAN, Nancy Z., 7618 Gazette Ave., Canoga Park, Calif.
SPINDLER, Zane A., E 153 Owen Grad. Center, East Lansing, Mich.
SPINGARN, Edward D. W., 3400 Rodman St., N.W., Washington 8, D.C.
SPIRO, Alan, 360 E. 65th St., New York 21, N.Y.
SPIRO, Benjamin, 24 California St., San Francisco 11, Calif.
SPIRO, (Mrs.) Evelyn S., 6806 Renita Lane, Bethesda, Md. 21811
SPIRO, Julius, 6806 Renita Lane, Bethesda, Md. 21811
SPIRO, Michael H., 5540 Fifth Ave., Pittsburgh, Pa. 15232
SPITZ, A. E., 321 Aylesford Pl., Lexington, Ky. 40508
SPITZ, Herbert, 319 W. 18th St., New York 11, N.Y.
SPITZ, John V., Salem Col., Winston-Salem, N.C.
SPITZ, (Mrs.) Ruth S., 100 Webster Park Ave., Columbus, Ohio 43214
SPITZE, Robert G. F., Univ. of Illinois, Dept. of Agric. Econ., Urbana, Ill.
SPITZER, Emil G., 4631 49th St., N.W., Washington 16, D.C.
SPITZNAS, Thomas J., Western Reserve Univ., Dept. of Econ., Cleveland 6, Ohio
SPIVACK, Ellen, 230 Riverside Dr., New York, N.Y. 10025
SPIVEY, W. A., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
SPLANE, Francis X., 12 Mercer Dr., Brookside, Newark, Del.
SPONAUGLE, (Mrs.) Judith B., 11516 Hickory Cluster, Reston, Va. 22070
SPOONER, Frank C., 31, Chatsworth Ave., Bromley, Kent, England
SPORLEDER, Thomas L., 557 Riverview Dr., Apt. A-4, Columbus, Ohio 43202
SPRAGUE, John W., 5413 85th Ave., #201, Lanham, Md. 20801
SPRATLEN, Thaddeus H., Western Washington State Col., Dept. of Econ., Bus. and Gov., Bellingham, Wash. 98225
SPRATT, J. S., Southern Methodist Univ., Dept. of Econ., Dallas, Tex.
SPRAY, David E., Int. Econ. Publishers, Inc., 4000 Brandywine St., N.W., Washington, D.C. 20016
SPRECHER, William M., 3329 Southgate Dr., Apt. 103, Alexandria, Va.
SPRENG, Frank, 1501 Lowrie St., Pittsburgh, Pa. 15212
SPRENKLE, Case M., 1913 Harding Dr., Urbana, Ill.
SPRIEGEL, William R., 1906 Hopi Trail, Austin, Tex.
SPRIGGS, Darrell L., Univ. of Arkansas, Col. of Bus. Admin., B.A. 225, Fayetteville, Ark.
SPRINGER, Augustus W., Jr., P.O. Box 1092, Wilmington, Del. 19899
SPRINGER, John Y., 801 Enchanted Way, Pacific Palisades, Calif. 90272
SPRINKEL, Beryl W., 1429 Bunker Ave., Flossmoor, Ill. 60422
SPRITZER, Allen D., New York State Sch. of Ind. and Labor Rela., Box 48, Cornell Univ., Ithaca, N.Y. 14850
SPROTT, John T., Duquesne Univ., Sch. of Bus. Admin., 600 Forbes Ave., Pittsburgh, Pa. 15219
SPROULL, Mary A., R. 3, Box 103, Elkhart, Ind. 46514
SPROUSE, Robert T., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif.
SPRUILL, Corydon P., Univ. of North Carolina, Chapel Hill, N.C.
SPULBER, Nicolas, 920 S. Manor Rd., Bloomington, Ind.
SPURR, William A., Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 96128
SPYCHALSKI, John C., Univ. of Maryland, Dept. of Bus. Admin., College Park, Md. 20742
SQUIRES, Richard K., Univ. of California-Los Angeles, 250 De Neve Circle, Los Angeles, Calif. 90024
STAAB, Josephine H., Univ. of Wisconsin, Sch. of Home Econ., Madison 6, Wis.
STAATS, William F., Fed. Res. Bank of Philadelphia, Philadelphia, Pa. 19101
STACEY, Benjamin F., 48 Old Brook Circle, Melrose, Mass. 02053
STADNIK, Milos, Pusvkinovo nam. 7, Prague 6, Czechoslovakia
STAEBLER, Neil, 408 Wolverine Bldg., 202 E. Washington St., Ann Arbor, Mich. 48108
STAEPELAERE, Raymond L., Stanford Res. Inst., Econ. Div., Menlo Park, Calif.
STAGER, David A. A., Univ. of Toronto, New Col., Toronto 5, Ont., Canada
STAHR, Ernst H., Inst. Finanzwissensch Univ., Saarbrucken 15, Germany
STAIGER, David S., 4612 Drummond Ave., Chevy Chase, Md. 20015
STAKELON, Elizabeth, 94 Clinton Ave., Brooklyn, N.Y. 11205
STALEY, A. E., Stanford Res. Inst., Menlo Park, Calif.
STALEY, Charles E., State Univ. of New York at Stony Brook, Dept. of Econ., Stony Brook, L.I., N.Y. 11790
STALLA, Robert J., 2211 Woodward Ave., Lakewood, Ohio 44107
STALLER, George J., Cornell Univ., Dept. of Econ., Goldwin Smith Hall, Ithaca, N.Y.
STALLEY, Douglas J., Univ. of New South

Wales, Sch. of Econ., Box 1, Post Off., Kensington, N.S.W., Australia
STALLINGS, James L., West Virginia Univ., Dept. of Agric. Econ., Morgantown, W.Va. 26506
STALLINGS, Kerwin B., Morgan Guaranty Trust Co., 23 Wall St., New York, N.Y. 10015
STALNAKER, Armand C., General American Ins. Co., 1501 Locust St., St. Louis 66, Mo.
STALON, Charles G., Southern Illinois Univ., Econ. Dept., Carbondale, Ill. 62903
STAM, Jerome, 1107-A University Village, East Lansing, Mich.
STAMAS, Stephen, 50 Stratton St., London, W. 1, England
STANBACK, Thomas M., Jr., 19 Wildwood Circle, Larchmont, N.Y. 10538
STANCILL, James M., Jr., 3642 Mountain View Ave., Pasadena, Calif. 91107
STANDISH, Thomas K., 307 Jurleyville Rd., Storrs, Conn. 06268
STANG, Sister M. Blandina, Aquinas Col., Grand Rapids, Mich.
STANGL, Peter E., 8 Knollwood Acres Apts., Storrs, Conn. 06268
STANLEY, John D., 4009 Fairway Ct., Arlington, Tex. 76010
STANLEY, (Mrs.) Marjorie T., 4009 Fairway Ct., Arlington, Tex. 76010
STANLEY, Roy J., Univ. of California, Dept. of Econ., 405 Hilgard Ave., Los Angeles, Calif. 90024
STANTON, Henry B., Addison-Wesley Pub. Co., Jacob Way, Reading, Mass. 01867
STAPHOLZ, (Mrs.) Judith, 91 Tulip Ave., Bldg. C, Apt. C-2, Floral Park, N.Y.
STAPLES, George H., Xaverian H. Sch., 7100 Shore Rd., Brooklyn, N.Y. 11209
STAPLETON, William R., 3960 STRAT WG., APO San Francisco, Calif. 96334
STARCH, Karl E., 877 18th St., Boulder, Colo.
STARK, Dennis E., Old Stone Bank, 86 S. Main St., Providence, R.I. 02901
STARK, Harry, Univ. of Miami, Econ. Dept., Coral Gables, Fla. 33926
STARK, Harry F., Rutgers, the State Univ., Univ. Extension Div., New Brunswick, N.J. 08903
STARK, John R., Com. on Banking and Currency, 1301 Longworth House Off. Bldg., Washington, D.C.
STARK, Walter R., 45 Garrison Rd., Hingham, Mass. 02043
STARLER, Norman H., Iowa State Univ., Dept. of Econ., 207 E. Hall, Ames, Iowa 50010
STARLIGHT, Lawrence L., 191 Overlook Dr., Greenwich, Conn.
STARR, Edward, Oakland Univ., Dept. of Econ., Rochester, Mich.
STARR, Gerald F., 95 Simpson Ave., Toronto 18, Ont., Canada
STARR, Philip C., 757 W. 7th St., Claremont, Calif. 91711
STARR, Robert G., Jr., Dandy Dr., Cos Cob, Conn. 06807
STARSHAK, Edward J., Barat Col., Dept. of Econ., Lake Forest, Ill. 60045
STARTZ, Arthur, Milton St. and Emerson Ave., Hartsdale, N.Y. 10530
STAUBER, Edward F., 425 Edgewood Ave., Rochester 18, N.Y.
STAUDINGER, Hans, 45 Sutton Pl., S., New York, N.Y. 10022
STAUSS, James H., Grinnell Col., Dept. of Econ., Grinnell, Iowa 50112
STEARNS, Robert N., 16 Winchester Ave., New Haven, Conn. 06511
STEBBING, Davis H., 2220 30th St., S.E., Washington, D.C. 20020
STECK, Leon J., 6234 33rd St., N.W., Washington 15, D.C.
STEDRY, Andrew C., Carnegie Inst. of Tech., Grad. Sch. of Ind. Admin., Pittsburgh, Pa. 15213
STEED, Guy P. F., Univ. of Washington, Dept. of Geography, Seattle, Wash. 98105
STEELE, Elroy J., Omaha Univ., Econ. Dept., Omaha 1, Neb.
STEELE, H. E., Auburn Univ., Dept. of Econ. and Bus. Admin., Auburn, Ala. 36830
STEELE, Henry M., 6987 Copperglow Ct., Cincinnati, Ohio 45244
STEELE, U. M., 255 Clover St., Athens, Ga. 30601
STEEVES, R. N., 433 Dufferin Ave., No. 23, London, Ont., Canada
STEG, Gail, 72-11 110th St., Forest Hills, N.Y. 11375
STEGEMANN, Klaus, Massachusetts Inst. of Tech., Rm. 52-280, Cambridge, Mass. 02139
STEGER, Wilbur A., CONSAD Res. Corp., 3406 Fifth Ave., Suite 301, Pittsburgh, Pa. 15213
STEIN, Bruno, New York Univ., Dept. of Econ., Washington Sq., New York, N.Y. 10003
STEIN, Emanuel, New York Univ., Washington Sq. Col., New York, N.Y. 10003
STEIN, Harold, 4574 Kings Highway, Brooklyn 34, N.Y.
STEIN, Herbert, 9342 Harvey Rd., Silver Spring, Md.
STEIN, Herbert A., c/o Katzen, Box 27, Reynoldsville, Pa. 15851
STEIN, Jerome L., Hebrew Univ., Eliezer Kaplan Sch. of Econ., Jerusalem, Israel
STEIN, Karl H., Roosevelt Univ., 430 S. Michigan Ave., Chicago, Ill. 60605
STEIN, Martin M., 1832 Metzeroff Rd., Apt. 38, Adelphi, Md. 20783
STEIN, Morris L., Concord Manor, 401 Marianna Dr., Wilmington 3, Del.
STEINBERG, Abner J. H. S., 317 Walmer Rd., Toronto 10, Ont., Canada
STEINBERG, David J., 7216 Stafford Rd., Hollin Hills, Alexandria, Va. 22307
STEINBERG, Gunther T., Western Electric Co., 222 Broadway, New York 38, N.Y.
STEINBERGH, Alex M., 16000 Terrace Rd., Apt. 705, East Cleveland, Ohio 44033
STEINDL, Frank G., Dept. of Econ., Oklahoma State Univ., Stillwater, Okla.
STEINER, Delbert M., 12407 Connecticut Ave., Silver Spring, Md.
STEINER, Fred, Jr., 129 Glenada Ct., Freeport, L.I., N.Y. 11520
STEINER, George A., 13943 Cumpston, Van Nuys, Calif.
STEINER, Joseph L., 3936 Red Bud Ave., Cincinnati, Ohio 45229
STEINER, Peter O., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
STEINER, Robert L., 912 Sycamore St., Cincinnati 2, Ohio
STEINER, William H., 2 Tudor City Pl., Apt. 8A N., New York 17, N.Y.
STEINHARDT, Hermann, 1915 S.W. DeWitt, Portland, Ore. 97201
STEINHAUER, Jules V., 138-10 Franklin Ave., Apt. 10A, Flushing, N.Y. 11355
STEINKAMP, Stanley W., Univ. of Illinois Col. of Com. and Bus. Admin., Dept. of Econ., 226 David Kinley Hall, Urbana, Ill.
STEKLER, Herman O., Bd. of Gov. Fed. Res. System, Div. of Res. and Statis., Washington, D.C. 20551
STELCNER, Morton, 2780 rue Benjamin-Sulte, Montreal 12, Que., Canada
STELLMACHER, H. B., Univ. of Hawaii, Honolulu 14, Hawaii
STELZER, Irwin M., 80 Pine St., New York 5, N.Y.
STEMP, Isay, 336 Redmont Rd., West Hempstead, L.I., N.Y. 10993
STENASON, W. J., 140 Pointe Claire Ave., Pointe Claire, P.Q., Canada
STENGER, J. D., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh, Pa.
STEPHAN, Daniel J., Northeastern Illinois Plan. Comm., 400 W. Madison St., Chicago, Ill. 60614
STEPHANSON, Earl M., 219 Drake Lane, Des Plaines, Ill.
STEPHENS, J. K., 105 E. Springfield, Champaign, Ill.
STEPHENSON, Donald R., 150 Herkimer St., Hamilton, Ont., Canada
STEPHENSON, Harriet, Seattle Pacific Col., Dept. of Econ. and Bus., Seattle, Wash. 98119
STEPHENSON, James A., Iowa State Univ., Dept. of Econ., Ames, Iowa
STEPHENSON, Matthew, Wofford Col., Spartanburg, S.C.
STEPP, James M., Clemson Col., Agric. Econ. Dept., Clemson, S.C.

STERCHO, Peter G., 340 Meeting House Lane, Nartbeth, Pa. 19072
STERGIANOPOULOS, Panagiotis G., 620 W. 189th St., New York, N.Y. 10040
STERN, Carl, Randolph-Macon Woman's Col., Econ. Dept., Lynchburg, Va.
STERN, David H., 819 Chalcedony St., San Diego, Calif. 92109
STERN, Ernest, USAID, APO New York 09675
STERN, Frank I., Oak Pl., Croton-On-Hudson, N.Y. 10520
STERN, James L., Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706
STERN, Joseph J., c/o Ford Found., P.O. 7282, Karachi 3, Pakistan
STERN, Louis H., Nat. Plan. Ass., 1606 New Hampshire Ave., Washington 9, D.C.
STERN, Robert M., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich. 48104
STERNBERG, Marvin J., Int. Labour Off., Coop., Rural, and Related Inst., Geneva, Switzerland
STERNBERGER, Howard I., 1001 3rd St., S.W., Washington, D.C.
STERNITZKE, Donald L., Univ. of Arizona, Dept. of Econ., Tucson, Ariz.
STERNLIEB, George, Rutgers Univ., Sch. of Bus., 18 Washington Pl., Newark, N.J. 07102
STERNLIGHT, (Mrs.) Lenore F., 8006 Maple Ridge Rd., Bethesda, Md.
STERNLIGHT, Peter D., 8006 Maple Ridge Rd., Bethesda, Md.
STETTING, Lauge, Vibevaenget 141, Copenhagen, Bagsvaerd, Denmark
STETTLER, H. L., III, 332 Tunbridge Rd., Baltimore, Md. 21212
STETTNER, Walter F., Dept. of State, USAID /Buenos Aires, Washington, D.C.
STEUBER, Fritz, 1620 Fuller St., N.W., Washington 9, D.C.
STEVENS, Benjamin H., Univ. of Pennsylvania, Wharton Sch., Dept. of Econ., Philadelphia 4, Pa.
STEVENS, Carl M., Reed Col., Portland 2, Ore.
STEVENS, David, 1231 S. Second Ave., Walla Walla, Wash. 99362
STEVENS, David W., Pennsylvania State Univ., University Park, Pa. 16802
STEVENS, Edward J., 5816 Hanover Ave., Springfield, Va. 22150
STEVENS, Guy V. G., 165 Bishop St., New Haven, Conn.
STEVENS, Morris L., Kansas State Col. of Pittsburg, Pittsburg, Kans.
STEVENS, Robert D., 1137 Alton Rd., East Lansing, Mich.
STEVENS, Robert W., 90 Summit Dr., Manhasset, N.Y.
STEVENS, W. M., US/AID Amer. Embassy, Managua, Nicaragua
STEVENS, William H. S., 312 Queen St., Alexandria, Va.
STEVENSON, Alexander, Int. Bank for Recon. and Dev., Washington 25, D.C.
STEVENSON, Frederick W., 5415 Netherlands Ave., New York 71, N.Y.
STEVENSON, Harold W., 1948 Drew Ave., S., Minneapolis, Minn. 55416
STEVENSON, John C., 2 Park Lane, Mt. Vernon, N.Y. 10552
STEVENSON, Richard A., 528 Wellington Ave., Apt. 1C, Chicago, Ill. 60657
STEVENSON, Thomas M., Jr., St. Louis Univ., Col. of Com., Dept. of Econ., St. Louis, Mo.
STEVENSON, William W., 213A New Sprankle Bldg., TVA, Knoxville 2, Tenn.
STEWART, Charles F., Columbia Univ., Grad. Sch. of Bus., New York 27, N.Y.
STEWART, Charles T., Jr., 5147 Macomb St., N.W., Washington, D.C. 20016
STEWART, G. T., Troy State Col., Dept. of Bus. Admin., Troy, Ala. 36081
STEWART, Gerald A., 229 E. 142nd St., Dolton, Ill. 60419
STEWART, Ian A., Box 65, Norwich, Vt.
STEWART, Ian G., Univ. of Edinburgh, Dept. of Pol. Econ., 31 Buccleuch Pl., Edinburgh 8, Scotland
STEWART, James H., Box 2305, Greenville, N.C.
STEWART, James I., 50 Alexander St., Apt. 2804, Toronto 5, Ont., Canada
STEWART, James S., 350 Harmon Ave., Garden City, N.Y.
STEWART, Jimmy D., 303 51st, Des Moines, Iowa 50312
STEWART, (Mrs.) Joann P., 40 Church St., Winchester, Mass. 01890
STEWART, Lewis M., 210 N. 8th St., Columbus, Miss.
STEWART, Max D., Univ. of Alberta, Dept. of Econ., Edmonton, Alta., Canada
STEWART, Milton D., Jr., Univ. of Maryland, ED, APO New York 09403
STEWART, Robert H., Jr., Gulf Oil Corp., Box 1166, Rm. 3519, Pittsburgh 30, Pa.
STEWART, Roosevelt S., Bluefield State Col., Bluefield, W.Va.
STEWART, W. H., 215 Market St., San Francisco, Calif. 94105
STEWART, W. B., 1014 N. State St., Chicago, Ill. 60610
STEWART, William C., Walsh Inst., 120 Madison Ave., Detroit, Mich. 48226
STICKNEY, H. C., 91 Remsen St., Brooklyn, N.Y. 11201
STIEBER, Jack, 543 Charles St., East Lansing, Mich.
STIFEL, Laurence D., USOM APO, San Francisco 96346
STIGLER, George J., Univ. of Chicago, Sch. of Bus., Chicago, Ill. 60637
STIGLITZ, Joseph E., Massachusetts Inst. of Tech., Dept. of Econ., Rm. 52-371a, Cambridge, Mass
STILES, Lynn A., 2629 Wildwood Lane, Deerfield, Ill. 60015
STILLSON, Richard T., 46-A Escondido Village, Stanford, Calif. 94305
STIMSON, David H., 1760 Walnut, Berkeley 9, Calif.
STINEBOWER, Leroy D., Loch Lane, Greenwich, Conn. 06832
STINNEFORD, Claude L., 404 College Ave., Richmond, Ind.
STITZEL, Thomas E., 1416 Villard St., Eugene, Ore. 97403
STOBER, William J., II, Louisiana State Univ., Col. of Bus. Admin., Baton Rouge, La.
STOCHAJ, John M., 65 Riceman Rd., Berkeley Heights, N.J. 07922
STOCKDER, Archibald H., 5 Coppell Dr., Tenafly, N.J. 07670
STOCKER, Frederick D., Col. of Com. and Admin., Ohio State Univ., Columbus, Ohio 43210
STOCKFISCH, Jacob A., 50 Castro Rd., Monterey, Calif.
STOCKING, Collis A., 3225 N. Glebe Rd., Arlington, Va.
STOCKING, George W., 1000 6th St., N.W., Apt. 813, Washington, D.C. 20024
STOCKMAN, Lynn H., Northwestern Univ., Sch. of Bus., Evanston, Ill. 60201
STOCKS, Anthony H., 1265 Fairlawns Ave., Morgantown, W.Va. 26501
STOCKTON, Frank T., 720 Indiana St., Lawrence, Kans.
STOCKTON, John R., 1010 Gaston Ave., Austin 3, Tex.
STOCKTON, Philip B., The Citadel, Charleston, S.C.
STOCKWELL, Richard E., 621 Dinsmore Dr., Apt. B, Cincinnati, Ohio 45240
STOELZLE, Manfred, Wellingsbuetteler Landstr. 28, 2 Hamburg 63, Germany
STOEVENER, Herbert H., Oregon State Univ., Dept. of Agric. Econ., 203 Extension Hall, Corvallis, Ore.
STOIKOV, Vladimir L., Univ. of Illinois, Inst. of Labour and Ind. Rela., Champaign, Ill.
STOKELY, Hugh L., Jr., The Chase Manhattan Bank, 1 Chase Manhattan Plaza, New York 15, N.Y.
STOKER, Thomas M., Box 23, Shelby, Iowa 51570
STOKES, Charles J., Pepper Crossing, Stepney, Conn. 06468
STOKES, Milton L., P.O. Box 72, Annville, Pa. 17003
STOKES, William, American Book Co., 55 Fifth Ave., New York, N.Y. 10003
STOKKE, Baard R., 56, chemin Ancienne-Route, Grand Saconnex, Geneva, Switzerland

STOLEN, Justin D., Univ. of Illinois, 113 Commerce Annex, Urbana, Ill.
STOLERU, Lionel, Commissariat General du Plan, 18 Rue de Martignac, Paris 7, France
STOLL, Hans R., 5436 S. University, Chicago, Ill. 60615
STOLLAR, Andrew J., Court St., R.F.D. 2, Exeter, N.H. 03833
STOLNITZ, George J., Res. for the Future, 1755 Massachusetts Ave., N.W., Washington, D.C. 20036
STOLPER, Wolfgang F., Univ. of Michigan, Center for Res. on Econ. Dev., 307 S. State St., Ann Arbor, Mich. 48104
STOLTENBERG, Carl H., Iowa State Univ., Forestry Dept., Ames, Iowa
STOLTZ, Merton P., Brown Univ., Providence, R.I.
STOLZBERG, Alexander, 2621 Hubbard St., Brooklyn, N.Y. 11235
STONE, Allan D., Wartburg Col., Dept. of Econ., Waverly, Iowa 50677
STONE, David J., 780 Greenwich St., Apt. 5-G, New York, N.Y. 10014
STONE, Edward, c/o Mrs. David Stone, 160 Riverside Dr., New York 24, N.Y.
STONE, Edward A., 520 Marion E. Taylor Bldg., Louisville, Ky. 40202
STONE, Frances, 154-27 Willets Pt. Blvd., Whitestone, New York 9, N.Y.
STONE, Irving, 300 Riverside Dr. 7E, New York, N.Y. 10025
STONE, John L., 3147 N. 21st St., Arlington, Va. 22201
STONE, Robert W., Nat. Bank of Detroit, Woodward at Fort St., Detroit, Mich. 48232
STONEHILL, Arthur, 4016 Salem St., Concord, Calif.
STONESIFER, Gilbert R., Mt. Union Col., Dept. of Econ., Alliance, Ohio
STORER, James A., 2604 N. 24th St., Arlington, Va. 22207
STOREY, Reed K., Accounting Res. AICPA, 666 Fifth Ave., New York, N.Y. 10019
STORRS, Thomas I., North Carolina Nat. Bank, Greensboro, N.C.
STORY, Charles F., Muskingum Col., New Concord, Ohio 43762
STORY, John N., 2520 Russell St., Berkeley, Calif. 94705
STORY, Joseph C., 10934 S. Ewing Ave., Chicago, Ill. 60612
STORY, Richard M., III, 171 W. Berrien St., Galesburg, Ill. 61401
STOTZ, (Mrs.) Margaret S., 5415 Connecticut Ave., N.W., Apt. 429, Washington, D.C. 20015
STOUT, Randall S., Pennsylvania State Univ., Col. of Liberal Arts, Rm. 329, Boucke Bldg., University Park, Pa. 16802
STOVALL, Rollo P., 5623 Namakagan Rd., Washington, D.C. 20016
STOVEL, John A., c/o Canadian Embassy, Mexico, D.F., Mexico
STOYLE, Judith, Boucke Building, University Park, Pa. 17774
STRAIN, Robert E., Long Beach State Col., Dept. of Econ., Long Beach 15, Calif.
STRAND, Kenneth T., Amer. Embassy—OECD, APO 777, New York, N.Y. 09777
STRANEY, William, 4030 Hubbard Pl., Brooklyn, N.Y.
STRANGWAYS, Raymond S., Old Dominion Col., Dept. of Econ., Norfolk, Va. 23508
STRANTZ, Maurice K., Box 4006, Fresno, Calif. 97744
STRASMA, John D., Land Tenure Center, 310 King Hall, Madison, Wis. 53706
STRASSER, Arnold, 1505 Hampshire West Ct., Silver Spring, Md.
STRASSMAN, Wolfgang P., Michigan State Univ., Dept. of Econ., East Lansing, Mich. 48823
STRASZHEIM, Mahlon R., 15 Everett St., Apt. 2, Cambridge 38, Mass.
STRATTON, James W., Drexel and Co., 1500 Walnut St., Philadelphia, Pa. 19101
STRATTON, Samuel S., 118 S. Main St., Middlebury, Vt. 05753
STRAUBER, Rachel L. S., 215 E. 68th St., New York, N.Y. 10021
STRAUS, Everet M., 18057 Sorrento, Detroit 35, Mich.
STRAWN, James R., 6553 Welch St., Arvada, Colo. 80002
STRAWN, William H., Chicago Teachers Col., 6800 S. Stewart, Chicago, Ill. 60621
STREAMO, James P., 503 S. 26th St., Philadelphia, Pa. 19146
STREET, Donald R., Auburn Univ., Dept. of Econ., Auburn, Ala. 36830
STREET, James H., 11 Lexington Dr., Metuchen, N.J.
STREETEN, Paul P., Balliol Col., Oxford, England
STREEVER, Donald C., Jr., 1430 Dublin Rd., Maple Glen, Pa. 16933
STREJCEK, Barry W., Urban League of Essex County, 589 Central Ave., East Orange, N.J. 07018
STRELL, Peter F., 5345 S. Harper Ave., Apt. 405, Chicago, Ill. 60615
STRETCH, George W., 25 Ridgecrest St., Huntington, L.I., N.Y. 11743
STRICK, J., Univ. of Windsor, Dept. of Econ., Windsor, Ont., Canada
STRICKLER, Lester B., Oregon State Univ., Sch. of Bus. and Tech., Corvallis, Ore.
STRING, Joseph, Jr., c/o RAND Corporation, 1700 Main St., Santa Monica, Calif.
STROBEL, Frederick, 51 Walworth St., Worcester, Mass. 01602
STROESSLIN, Werner, 41 Schaeferweg, 4000 Basle, Switzerland
STROH, Craig M., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
STROHBEHN, Roger, U.S. Dept. of Agric., Econ. Res. Ser., Natural Res. Econ. Div., Washington, D.C. 20250
STROMSDORFER, Ernst W., Pennsylvania State Univ., Dept. of Econ., University Park, Pa. 16802
STRONG, Jerome C., 14 Hamilton St., Hamilton, N.Y. 13346
STRONG, Warren T., 13273 S. Norfolk, Detroit, Mich. 48235
STROTZ, Robert H., Northwestern Univ., Kresge Hall, Rm. 29, Evanston, Ill. 60201
STROUP, Earl B., 7308 Austin St., Annandale, Va. 22003
STROUP, Kenneth T., 225 Adams St., Apt. 11-C, Brooklyn, N.Y. 11201
STROUP, Robert H., Univ. of Kentucky, Dept. of Econ., Lexington, Ky. 40506
STROUT, Alan M., 5102 Baltimore Ave., Washington, D.C. 20016
STROVER, Russell G., 5612 N. 40th St., Milwaukee, Wis. 53209
STRUBLE, Frederick M., 2215 Overton, Independence, Mo.
STRUCK, Stuart H., 5018 Milward Dr., Madison 11, Wis.
STRUVE, Louis W., 111 S. Lynwood Ave., Glenside P.O., Pa. 19038
STRYKER, Josiah D., 414 W. 121st St., Apt. 51, New York 27, N.Y.
STUART, Fredric, 84 The Promenade, Glen Head, N.Y. 11545
STUART, Gary M., 209 W. Willow St., Normal, Ill. 61761
STUART, Richard K., 560 University St., Walla Walla, Wash. 99362
STUART, Robert C., c/o F. M. McCaig, 1231 E. Oak St., Othello, Wash. 99344
STUBBLEBINE, William C., Univ. of Delaware, Dept. of Econ., Newark, Del. 19711
STUBBS, Francis L., 310 Manor Dr., Columbia, Mo.
STUBER, Kenneth R., RR 5, Box 295, Terre Haute, Ind. 47803
STUCKEY, R. A., E. I. duPont de Nemours and Co., Econ. Off., Wilmington 98, Del.
STUCKI, Roland, Univ. of Utah, Col. of Bus., Salt Lake City, Utah 84112
STUDYBAKER, Aaron D., 450 Golden Gate Ave., Box 36017, San Francisco, Calif. 94102
STUETZEL, Wolfgang, Univ. Saarbruecken, Saarbruecken, Germany
STUHR, David P., 20 Cleverdon Rd., Hohokus, N.J. 07423
STUPAY, Arthur M., 17 N. Wickom Dr., Westfield, N.J. 07090
STURC, Ernest, c/o Int. Monetary Fund, 19th and H Sts., N.W., Washington 25, D.C.
STURGEON, Robert R., R.R. 3, Nashville, Ind. 47448

STURGIS, Margaret R., 519 Virginia Ave., Montgomery, Ala. 36104
STURMTHAL, Adolf F., Univ. of Illinois, Inst. of Labor and Ind. Rela., Urbana, Ill.
STYLES, Michael H., 1050 26th St., S., Arlington 2, Va.
SU, Tsauh-An, 231 Upson Lane, University Hgts., New Brunswick, N.J. 08904
SUAREZ, James M., 1025 67th St., Brooklyn, N.Y. 11219
SUBAT, John E., Purdue Univ., Dept. of Agric. Econ., Lafayette, Ind. 47907
SUBRAHMANYAM, R., P8/5, New Colony, Bhadaini, Banaras-1, India
SUCRE, Juan M., 30 Gardena St., Brighton, Mass. 02135
SUDOL, Mary A., 605 Walnut Ave., Syracuse, N.Y. 13210
SUFRIN, Sidney C., 133 Humbert Rd., Lewiston Manor, Syracuse 10, N.Y.
SUH, Suck Joon, Vanderbilt Univ., Box 1141, Station B., Nashville, Tenn.
SUHR, Harry A., Wisconsin State Univ.-Whitewater, Sch. of Bus. and Econ., Dept. of Econ., Whitewater, Wis.
SUITS, Daniel B., Univ. of Michigan, Econ. Dept., Ann Arbor, Mich. 48104
SULLIVAN, Charles M., Bentley Col., 921 Boylston St., Boston, Mass.
SULLIVAN, (Mrs.) Clara K., 112 Manthorne Rd., West Roxbury, Mass. 02791
SULLIVAN, Daniel L., Jr., 202 Washington St., Marblehead, Mass. 01945
SULLIVAN, Edward T., Univ. of Florida, Sch. of Forestry, Gainesville, Fla.
SULLIVAN, James J., Univ. of California, Dept. of Econ., Santa Barbara, Calif. 93106
SULLIVAN, John F., Nat. Asso. of Mutual Savings Banks, 200 Park Ave., New York, N.Y. 10017
SULLIVAN, Maureen T., 506 Lee St., Evanston, Ill. 60202
SULLIVAN, Oliver R., 220 Ave. D, Billings, Mont.
SULLIVAN, Richard F., 1019 Keith, Berkeley 8, Calif.
SULLIVAN, Thomas A., 1521 South Shore Dr., Erie, Pa.
SULLIVAN, Timothy J., Naess and Thomas, 2 Broadway, New York 4, N.Y.
SULTAN, Paul E., 718 N. Live Oak, Glendora, Calif. 91740
SULTAN, Ralph G. M., 33 Rolling Lane, Weston, Mass. 02193
SUMADHI, Mohamed, Djl. Widjaja XV No. 40, Kebajoran Baru, Djakarta, Indonesia
SUMANTORO, National Planning Coun., Taman Suropati 2, Djakarta, Indonesia
SUMBERG, Theodore A., 5802 Wilmett Rd., Bethesda, Md. 21811
SUMICHRAST, Michael M., 5416 Harwood Rd., Bethesda, Md. 21811
SUMMERFIELD, John R., Douglas Aircraft Co., Inc., 3000 Ocean Park Blvd., Santa Monica, Calif.
SUMMERS, (Mrs.) Anita A., 641 Revere Rd., Merion Station, Pa. 19066
SUMMERS, Robert, 641 Revere Rd., Merion Station, Pa. 19066
SUN, Emily Mung-chio Chao, 87-46 Chelsea St., Jamaica, N.Y. 11432
SUN, Norman, Int. Christian Univ., Mitaka, Tokyo, Japan
SUNDAY, Alexander A., 1010 W. Green St., 0099, Urbana, Ill. 61801
SUNDERHAUF, Milo B., 3400 Marlbrough Ct., College Park, Md. 20741
SUNGA, P. S., 1287 Field St., Ottawa, Ont., Canada
SUNSHINE, Michael R., 592 Jones St., Ventura, Calif.
SUOJANEN, Waino W., Univ. of Miami, Sch. of Bus. Admin., Dept. of Mgt., Coral Gables, Fla. 33926
SUPEL, (Mrs.) Mary R., 1703 S. Ferry St., Anoka, Minn. 55303
SUPEL, Thomas M., 6306 Kyle Ave., N., Minneapolis, Minn. 55429
SUPPLE, Markis J., 1977 Biltmore St., N.W., Washington, D.C.
SURANYI-UNGER, T., Econ. Seminar, Univ., Weender Landstrasse 2, Goettingen, Germany
SURANYI-UNGER, Theodore, Jr., 940 Spring Hill Rd., McLean, Va. 22101
SURIYAKUMARAN, C., c/o Econ. Comm. for Asia and Far East, Bangkok, Thailand
SURLA, Leandro T., 982 Royal Arms Apts., 2111 Lansill Rd., Lexington, Ky.
SURLS, Frederic M., 2307 Pittsfield Blvd., Ann Arbor, Mich.
SURMEIER, John J., 5903 Johnson Ave., Bethesda, Md. 20034
SURNAMER, Charles R., 645 S. Crouse Ave., Syracuse 10, N.Y.
SUSKO, John P., 920 Chestnut St., Erie, Pa.
SUSSMAN, (Mrs.) Gennifer M. P., 1314 Birch St., Denver, Colo. 80220
SUSSNA, Edward, Univ. of Pittsburgh, Grad. Sch. of Bus., Pittsburgh 13, Pa.
SUTCH, Richard, Massachusetts Inst. of Tech., Econ. Dept., Cambridge, Mass. 02139
SUTHERLAND, Alister, 81 S. Hill Park, Flat 6, London N.W. 3, England
SUTHERLAND, Ian R., 12819 Buckingham Dr., Bowie, Md. 20715
SUTTON, Antony C., 1810 Las Lunas, Pasadena, Calif.
SUTTON, Sterling, 2111 Jefferson Davis Hwy., Arlington, Va. 22202
SUTTOR, Richard E., Univ. of Maryland, Dept. of Agric. Econ., College Park, Md.
SUVER, C. William, St. Martin's Col., Olympia, Wash. 98502
SUVIRANTA, Bruno, Vuorimies 19, Helsinki, Finland
SUZUKI, Atsushi, Wakayama Univ., Sch. of Econ., 278 Sekito, Wakayami-shi, Japan
SUZUKI, Gengo, Int. Monetary Fund and Int. Bank for Recon. and Dev., Washington, D.C.
SVEHLA, George, 9314 Headlands Rd., Mentor, Ohio 44060
SVEIKAUSKAS, Leo, Rutgers Univ., Dept. of Econ., New Brunswick, N.J.
SVESTKA, M. J., 1724 Taylor St., N.W., Washington 11, D.C.
SWAGLER, Roger M., 1730 King Ave., Apt. D., Columbus, Ohio 43212
SWAINE, Howard R., c/o RAND Corp., 1700 Main St., Santa Monica, Calif. 90406
SWAMY, Subramanian, Harvard Univ., Lowell House M-24, Cambridge, Mass. 02138
SWAN, Eliot J., 106 St. Albans Rd., Berkeley, Calif. 94708
SWANISH, Peter T., 6527 Glenwood Ave., Chicago, Ill.
SWANSON, Arthur E., 2310 Central Park Ave., Evanston, Ill. 60201
SWANSON, Carl V., Massachusetts Inst. of Tech., 50 Memorial Dr. Rm. 52-560, Cambridge, Mass. 01922
SWANSON, David H., Iowa Southern Utilities Co., Area Dev., Centerville, Iowa
SWANSON, Donald F., Univ. of South Carolina, Sch. of Bus. Admin., Columbia, S.C. 29208
SWANSON, Earl R., Univ. of Illinois, Rm. 305 Mumford Hall, Urbana, Ill.
SWANSON, Edward S., R.F.D. #25 Nutmeg Lane, Madison, Conn. 06443
SWANSON, Ernst W., North Carolina State Univ., 201 Patterson Hall, Raleigh, N.C. 27607
SWANSON, Gerald J., 102 Hopkins Hall, URH, Champaign, Ill.
SWANSON, Jack O., 1201 N.E. Campus Pkwy., Seattle, Wash. 98105
SWANSON, Robert L., Montgomery Ward and Co., Corporate Res. Div., 619 W. Chicago Ave., Chicago, Ill. 60607
SWANSON, Rodney B., Jr., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
SWANSON, Stephen P., 2100 Linwood Ave., Apt. 18N, Fort Lee, N.J. 07024
SWANTZ, Alexander, 5-Q Magie Apts., Faculty Rd., Princeton, N.J. 08540
SWEARINGEN, Eugene L., Oklahoma State Univ., Vice Pres. for Dev., Stillwater, Okla. 74075
SWEEN, O'NEIL L., Jr., 7322 Lee Hwy., Apt., 202, Falls Church, Va. 22046
SWEENEY, John G., Jr., 2998 Yorkshire Rd., Cleveland Heights 18, Ohio
SWEENEY, Robert E., Cochise Col., Drawer L, Douglas, Ariz. 85607
SWEENEY, Timothy D., Int. Monetary Fund, 19th and H Sts., N.W., Washington, D.C.

SWEENEY, Vernon E., 2516 Hartford Rd., Austin, Tex. 78703
SWEET, James A., 3315 Williamsburg, Ann Arbor, Mich. 48104
SWEET, Morris L., 185 Hall St., Brooklyn, N.Y. 11205
SWEETSER, Albert G., 346 Locust St., Edgewood, Pittsburgh, Pa. 15218
SWEEZY, Alan R., California Inst. of Tech., Dabney Hall, Pasadena, Calif.
SWEEZY, Paul M., 2 Lindsley Dr., Larchmont, N.Y. 10538
SWENSON, Bernard D., 2126 Auhuhu St., Pearl City, Hawaii 96782
SWENSON, Norman P., 5783 Kingsbury Pl., No. 4, St. Louis, Mo. 63112
SWERDLOW, (Mrs.) Gertrude, Syracuse Univ., Dept. of Econ., Syracuse 10, N.Y.
SWERDLOW, Irving, Syracuse Univ., Dept. of Econ., Syracuse 10, N.Y.
SWERLING, Boris C., Regency House Apt. 517, 5201 Connecticut Ave., N.W., Washington, D.C. 20015
SWIFT, John R., 1345-1/2 W. Touhy Ave., Chicago, Ill. 60626
SWIFT, William Judd, Temple Univ., Econ. Dept., Philadelphia, Pa. 19122
SWINDLER, E. J., 2925 Meadow View Rd., Falls Church, Va. 22042
SWINYARD, Alfred W., Univ. of Michigan, Grad. Sch. of Bus. Admin., Ann Arbor, Mich.
SWITZER, Brian C., Western Reserve Univ., Sch. of Bus., Cleveland, Ohio 44106
SWOPE, Daniel A., National Canners Asso., 1133 20th St., N.W., Washington, D.C. 20036
SYDNEYSMITH, Sam, 3424 E. Glasson St., Durham, N.C.
SYDOR, Leon P., Univ. of Waterloo, Dept. of Econ., Waterloo, Ont., Canada
SYLLA, Richard E., 206F Holden Green, Cambridge, Mass. 02138
SYMONS, Harry C., R.D. 2, Pottstown, Pa.
SYNNOTT, Thomas W., III, 77 Washington Pl., New York, N.Y. 10011
SYPIN, Laura D., 2209 Gough St., San Francisco, Calif. 94109
SYVRUD, Donald E., Fin. Dept., APO New York 09676
SZARZYNSKI, Anthony, 421 1/2 S. 10th, Laramie, Wyo.
SZE, Palmer C., 50 Garden Ave., Chatham, N.J. 07928
SZEJMAN, Alexander, 41 W. 90th St., New York, N.Y. 10024
SZEP, Louis L., Oklahoma City Golf and Country Club, 7000 N.W. Grand Blvd., Oklahoma City, Okla.
SZUCH, Rudy S., 519 Randolph Rd., Plainfield, N.J. 07060
SZYMANSKI, Joseph, R.D. 1, Box 215, South Somerville, N.J. 08876

TABBARAH, Riad, Dept. of Soc. Affairs, United Nations, N.Y. 10988
TABOR, Ralph L., 966 National Press Bldg., Washington, D.C. 20004
TABOR, Stanley, 2724 University Ave., New York 68, N.Y.
TABRIZTCHI, Sirousse, 303 W. 106th St., New York, N.Y. 10027
TACHINO, Tad, 4616 S. 7th St., Arlington, Va. 22204
TADDEO, Mario, Monmouth Col., West Long Branch, N.J. 07764
TAFF, Charles A., Univ. of Maryland, College Park, Md.
TAFFET, Joseph, 555 Kappock St., Riverdale, N.Y. 10463
TAFT, Philip, 43 Irving Ave., Providence 6, R.I.
TAGGART, Joseph H., 100 Trinity Pl., New York 6, N.Y.
TAGUCHI, Yoshihiro, Doshisha Univ., Dept. of Econ., Kyoto, Japan
TAIBI, Calogero, 60 First Ave., Apt. 121, New York, N.Y. 10009
TAILBY, Donald G., 275 Southview Dr., Athens, Ga.
TAIRA, Koji, Stanford Univ., Dept. of Econ., Stanford, Calif.
TAITEL, Martin, 1155 E. 57th St., Chicago, Ill. 60637
TAITT, Arthur T., Univ. of Cincinnati, Col. of Bus. Admin., Cincinnati 21, Ohio
TAKAHASHI, Akira, 273 Burrows St., Apt. 1003, Pittsburgh 13, Pa.
TAKAI, Shigeo W., 1225 Greenview Dr., Lakeland, Fla. 33802
TAKATA, Kaoru, 4214 Ave. G., Austin, Tex. 78751
TAKAYAMA, Akira, Univ. of Minnesota, Dept. of Econ., Minneapolis, Minn. 55455
TAKENAKA, Tatsuo, 3 Nakatani, Kanno, Hyogo Prefecture, Nishinomiya City, Japan
TALARICO, Joseph F., 321 New Market Rd., Dunellen, N.J.
TALBOT, Homer, 1230 Hudson St., Denver 20, Colo.
TALLEY, Claude A., Jr., Victoria Col., Victoria, Tex.
TALMERS, William N., 150 Greenway Ter., Forest Hills, L.I., N.Y. 12068
TAMAGNA, Frank M., American Univ., Dept. of Econ., Washington 16, D.C.
TAMI, Felipe S., José Hernández 1955, Piso 9 Dto 39, Buenos Aires, Argentina
TAMM, Feliks, 2109 Fort Davis St., S.E., Washington, D.C. 20020
TAN, Augustine H. H., 58-F Escondido Village, Stanford, Calif. 94305
TAN, Bui Quang, Dir. Gen. of Trade Ser., Ministry of Nat. Econ., 59 Gia Long, Saigon, Vietnam
TAN, Bulent, Turkali Mah. Misirli Bahce Sok., No. 32/2, Besiktas-Istanbul, Turkey
TANADA, Leonardo E., 140 W. 69th St., PH 3, New York, N.Y. 10023
TANDROW, Walter Steven, 363 S. Burnside Ave., Los Angeles, Calif. 90036
TANDY, W. Lou, Central State Col., Wilberforce, Ohio 45384
TANESE, Shigeru, c/o Hitotsubashi Univ., Kunitachi, Tokyo, Japan
TANG, Anthony M., Vanderbilt Univ., Dept. of Econ., Nashville, Tenn. 37203
TANG, James, 3611 Kenway St., Silver Spring, Md.
TANGEN, Dolores L., 35-10 35th St., Astoria, N.Y. 11106
TANGRI, Om P., Univ. of Manitoba, Dept. of Agric. Econ., Rm. 420, Agric. Bldg., Winnipeg 19, Man., Canada
TANGRI, Shanti S., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
TANIGUCHI, Izumi, Fresno State Col., Dept. of Econ., Fresno, Calif. 93726
TANNENBAUM, Frank, Columbia Univ., Dept. of History, New York, N.Y.
TANNENBAUM, R., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles 24, Calif.
TANNER, J. Ernest, Brown Univ., Dept. of Econ., Providence, R.I.
TANNER, John W., Conzaga Univ., Dept. of Econ. and Bus., Spokane, Wash.
TANNIAN, Francis X., Duquesne Univ., Sch. of Bus. Admin., 600 Forbes Ave., Pittsburgh, Pa. 15219
TANSEY, Frank, 1511 Sheridan Ave., New York, N.Y. 10457
TANZER, M.D., 225 East 106th St., New York 29, N.Y.
TANZI, Vito, 1101 New Hampshire Ave., Apt. 703, Washington, D.C. 20037
TAOKA, George M., Univ. of Toledo, Dept. of Int. Bus., Toledo, Ohio 43606
TAPLIN, Grant B., 1930 Columbia Rd., N.W., Washington, D.C. 20009
TARASCIO, Vincent J., Univ. of North Carolina, Dept. of Econ., Chapel Hill, N.C.
TARASOFSKY, Abraham, Sir George Williams Univ., Dept. of Econ., Drummond St., Montreal 25, Que., Canada
TARASOV-REED, Helen, Box 958 R. 3, Asheville, N.C.
TARPEY, Lawrence X., Univ. of Kentucky, Col. of Com., Lexington, Ky.
TARPLEY, Fred A., Georgia Inst. of Tech., Sch. of Ind. Mgt., Atlanta 13, Ga.
TARSHIS, Lorie, Stanford Univ., Dept. of Econ., Stanford, Calif. 96128
TARULLO, Ronald, 549 N. Neville St., Pittsburgh, Pa.
TASHMAN, Leonard J., 74 Olive St., Providence 6, R.I.
TASKY, Kenneth M., 1931 Kennedy Ave., Apt. T-1, McLean, Va. 22101
TATE, Curtis E., Jr., Univ. of Georgia, Col. of Bus. Admin., Athens, Ga.

TATE, (Mrs.) Juanita D., 1908 Carlton Ave., Greensboro, N.C. 27406
TATKON, (Mrs.) Carol C., Standard Oil Co. (N.J.), 30 Rockefeller Plaza, Rm. 2548, New York, N.Y. 10020
TATTERSALL, James N., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
TAUB, Allan, 3222 W. Carmen, Chicago, Ill. 60625
TAUBENBLATT, Selig A., 5807 Conway Rd., Bethesda, Md. 21811
TAUBENFELD, (Mrs.) Rita F., Southern Methodist Univ., Sch. of Law, Dallas, Tex.
TAUBMAN, Paul J., 2030 F. St. N.W., Washington, D.C.
TAUS, Esther R., 1882 Grand Concourse, New York 57, N.Y.
TAUSSIG, Michael K., City Univ. of New York, Dept. of Econ., 133rd St. and Convent Ave., New York, N.Y.
TAUSSIG, Russell, 90 Morningside Dr., Apt. 4G, New York, N.Y. 10027
TAYLOR, Albion G., 2401 Sturgis Rd., Colorado Springs, Colo.
TAYLOR, Amos E., Parkton, Md. 21120
TAYLOR, Benjamin J., Indiana Univ., Owen Hall 201, Bloomington, Ind. 47405
TAYLOR, Charles T., 1193 Belaire Dr., N.E., Atlanta, Ga. 30319
TAYLOR, Dale E., 4426 Graywood, Long Beach 8, Calif.
TAYLOR, Gary C., 12423 Madeley Lane, Bowie, Md. 20715
TAYLOR, George E., 2872 Alanzo Lane, Costa Mesa, Calif. 92626
TAYLOR, George R., 229-B Thomas Dr., Monroe Park, Wilmington, Del. 19807
TAYLOR, George W., 1900 Rittenhouse Sq., Philadelphia 3, Pa.
TAYLOR, George W., 1011 Arlington Blvd. W-1041, Arlington 9, Va.
TAYLOR, Gordon T. C., 104 Wilson St., Albany, Calif. 94507
TAYLOR, Harold C., W.E. Upjohn Inst. for Employ. Res., 300 S. Westnedge Ave., Kalamazoo, Mich. 49007
TAYLOR, John N., Jr., 822 Pondway Rd., Dayton, Ohio 45419
TAYLOR, Milton C., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
TAYLOR, Norman E., Montana State Univ., Sch. of Bus. Admin., Missoula, Mont. 59801
TAYLOR, Norman W., Franklin and Marshall Col., Dept. of Econ., Lancaster, Pa.
TAYLOR, Overton H., Vanderbilt Univ., Dept. of Econ., Nashville, Tenn. 37203
TAYLOR, Paul N., Univ. of Connecticut, Storrs, Conn. 06268
TAYLOR, Philip E., Univ. of Connecticut, Dept. of Econ., Storrs, Conn. 06268
TAYLOR, Robert G., Qtrs. 4120, U.S. Air Force Academy, Colo. 80840
TAYLOR, Theodore J., 2216 Ohio St., Lawrence, Kans. 66044
TAYLOR, Virdon W., 1385 Pepperhill Dr., Florissant, Mo. 63033
TAYLOR, W. Bruce, Res. Anal. Corp., McLean, Va. 22101
TAYLOR, Wayne C., 14 Orange St., Nantucket, Mass. 02554
TAYLOR, Zachary, Jr., La Grange Col., La Grange, Ga. 30240
TCHA, Ken, 100-32, 1-ka, Hoe-hyun Dong, Chung-ku, Seoul, Korea
TEACH, Richard D., Purdue Univ., Kronnert Grad. Sch. of Ind. Admin., West Lafayette, Ind.
TEAD, Ordway, Four Corners Farm, R.F.D. 1, Newtown, Conn. 06470
TEAF, Howard M., Jr., Haverford Col., Haverford, Pa. 19041
TEAGUE, Alan J., 7205 Forsyth, University City, Mo. 63105
TEBBEN, F. Wendell, Box 7605, Franklin Sta., Washington, D.C. 20044
TEBBUTT, Arthur R., Northwestern Univ., Sch. of Com., Evanston, Ill.
TEBEAU, Robert L., 10 Arbor Lane, Huntington, N.Y.
TEELE, Thurston F., c/o Checci Contract/USAID, Amer. Embassy, Monrovia, Liberia
TEETS, Philip G., 2122 Orrington Ave., Evanston, Ill. 60201
TEFERTILLER, K. R., Univ. of Florida, Dept. of Agric. Econ., 162-B McCarty Hall, Gainesville, Fla. 32603
TEICHERT, Pedro C. M., Univ. of Mississippi, Sch. of Bus. and Gov., Dept. of Econ. and Bus. Admin., University, Miss. 38677
TEIGEN, Ronald L., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
TEITEL, Simon, 800 West End Ave., Apt. 1-A, New York, N.Y. 10025
TEITELBAUM, Perry D., 1705 Sanford Rd., Silver Spring, Md.
TEITZ, Michael B., Univ. of California, Dept. of City and Regional Plan., Berkeley 4, Calif.
TELLA, Alfred, 2401 Calvert St., N.W., Washington 8, D.C.
TELLA, (Mrs.) Dorothy M., 2401 Calvert St., N.W., Washington, D.C. 20008
TELLER, Azriel A., Johns Hopkins Univ., Dept. of Polit. Econ., Baltimore, Md. 21218
TELLEW, Fuad H., California State Poly. Col., Dept. of Soc. Sci., San Luis Obispo, Calif.
TELSER, Lester G., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
TEMBY, J. L., 705 N. Main St., Dodgeville, Wis. 53533
TEMIN, Peter, Massachusetts Inst. of Tech., Sloan Sch. of Mgt., Cambridge, Mass. 02139
TEMPLE, Frederick C., 2330 79th Ave., Baton Rouge, La.
TEMPLETON, William H., Findlay Col., Findlay, Ohio 45840
TEMSKY, Morton, 34 Glen Rd., Granby, Conn. 06035
TENCO, John, 225 N. Hutchins, Lodi, Calif. 95240
TENENBAUM, Henry, c/o Amer. Embassy, Bogota, Colombia
TENENBAUM, Marcel, 41-50 78th St., Elmhurst, N.Y. 11373
TENG, Quek Kai, c/o Quek Shin and Sons, Ltd., 1, Prince St., Singapore 1, Malaya
TENNANT, Richard B., 16 Wendt Ave., Larchmont, N.Y. 10538
TEPER, Lazare, Int. Ladies' Garment Workers' Union, 1710 Broadway, New York, N.Y. 10019
TEPHAVAL, Sunai, Dept. of Com. Intelligence, Ministry of Econ. Affairs, Rajdamnern Ave., Bangkok, Thailand
TEPPER, Lester, 2707 Navarre Dr., Chevy Chase, Md. 21620
TERBORGH, George W., 4582 N. 26th St., Arlington, Va.
TERESHTENKO, Valery J., Mechnikova 10/2, Apt. 106, Kiev, U.S.S.R.
TERLECKYJ, Nestor E., 1400 S. Joyce St., Apt. B-1114, Arlington, Va.
TERNENT, James A. S., CETREDE, Univ. do Ceará, Caixa Postal No. 172, Fortaleza, Ceará, Brazil
TERPSTRA, Vern, 927 Turner Ave., Drexel Hill, Pa. 19206
TERRANELLA, John G., 4425 N. Damen Ave., Chicago, Ill. 60625
TERRELL, R. D., Australian Nat. Univ., Sch. of Gen. Studies, Dept. of Statis., Box 4, P.O., Canberra, A.C.T., Australia
TESZLER, Roger C., Inst. for the Soc.-Econ. Study of Dev. Regions, 2a Linnaeusstraat, Amsterdam, The Netherlands
TEUBAL, Miguel, Heredia 1750, Buenos Aires, Argentina
THABET, George T., Saint Patrick's Col., Ottawa, Ont., Canada
THAKUR, Chandra D., Apartado 6948, Caracas, Venezuela
THAN, Tran Quy, 4 Chemin des Vallieres, Ville D'Avray (S. and O.), France
THANAWALA, Kishor, Pritam, 11th Rd., Khar, Bombay 52, India
THANH, Au T., 86, Nguyen Du, Saigon, Vietnam
THANOS, Costas A., 129 vas. Sophias Ave., Athens, Greece
THARRINGTON, James M., 59-06 Linden St., Ridgewood, N.Y. 11227
THATCHER, George W., P.O. Box 67, Oxford, Ohio 45056
THATCHER, Lionel W., 4130 Mandan Crescent, Madison, Wis. 53711
THAYER, Ralph I., Washington State Col., Pullman, Wash.

THEILMAN, Ward, Univ. of Massachusetts, Dept. of Fin., Amherst, Mass.
THEINE, Roger R., W136 N6844 Claas Rd., Menomonee Falls, Wis. 53051
THEK, Robert R., Riegel Paper Corp., Grand Central Sta., P.O. Box 170, New York 17, N.Y.
THELEN, Max, 136 Alvarado Rd., Berkeley, Calif.
THELWELL, Donald S., Dutchess Community Col., Poughkeepsie, N.Y.
THEOBALD, Adrian D., c/o First Fed. Savings and Loan, 111 N. Jefferson Ave., Peoria, Ill.
THEODORE, Chris A., 27 Turning Mill Rd., Lexington, Mass. 02173
THEODORE, Christ N., 5727 N. Artesian St., Chicago, Ill. 60645
THEODORIDES, Angelos, Dept. of State, USAID/Guatemala, Washington, D.C. 20520
THERRIEN, Ernest R., Hills Beach Rd., Biddeford, Me. 04005
THIBODEAU, Delmar A., P.O. Box 164, Augusta, Me. 04332
THIEL, Floyd I., R.F.D. 1, Box 284C, Laurel, Md. 20810
THIEMANN, Charles L., Fed. Home Loan Bank of Cincinnati, 309 Vine St., Cincinnati, Ohio 45202
THIEWVONGS, Boonmee, Nat. Econ. Dev. Bd., Krung Kasem Rd., Bangkok, Thailand
THIMESTER-FLEMING, Renate, P.O. Box 1071, University, Ala. 35486
THIMM, Alfred L., Union Col., Schenectady, N.Y.
THIN, Maung Tun, Int. Monetary Fund, Alternate Exec. Dir., Washington 25, D.C.
THINH, Nguyen Dang, 36 Huynh Thuc Khang, Saigon, Vietnam
THIRWALL, Anthony P., Univ. of Leeds, Dept. of Econ. and Com., Leeds 2, England
THODEY, Alan R., Univ. of Illinois, 305 Mumford Hall, Urbana, Ill. 61803
THOMAN, Clemens B., Univ. of Nebraska, Econ. Dept., Lincoln 8, Neb.
THOMANDER, Alan R., 470 Jefferson Ave. Apt. 4J, Elizabeth, N.J. 07201
THOMAS, Ja-Cy, Catawba Col., Salisbury, N.C. 28144
THOMAS, John J., London Sch. of Econ., Houghton St., Aldwych, London W.C. 2, England
THOMAS, Lloyd B., 722 Hinman Ave., Evanston, Ill. 60202
THOMAS, Michael J., Syracuse Univ., Col. of Bus. Admin., 116 College Pl., Syracuse, N.Y. 13210
THOMAS, Morgan D., Univ. of Washington, Dept. of Geography, Seattle 5, Wash.
THOMAS, Morgan I., 115 Ellis Ave., Mankato, Minn.
THOMAS, Philip S., Kalamazoo Col., Kalamazoo, Mich. 49001
THOMAS, Ralph L., 5023 Frew Ave., Pittsburgh, Pa. 15213
THOMAS, Raymond D., 2349 Catalina Ave., Southern Hills, Springfield, Mo. 65804
THOMAS, Richard G., Jr., 1771 High School Dr., Brentwood, Mo. 63144
THOMAS, Robert P., Univ. of Washington, Dept. of Econ., Seattle, Wash.
THOMAS, Robert W., Iowa State Univ., Dept. of Econ., 314 East Hall, Ames, Iowa 50010
THOMAS, Rollin G., 636 Rose, West Lafayette, Ind. 47178
THOMAS, Wayne C., Univ. of Missouri, 203 Whitten Hall, Columbia, Mo. 65202
THOMAS, Woodlief, 3804 Raymond St., Chevy Chase, Md. 20015
THOMASSEN, Henry, Univ. of British Columbia, Dept. of Econ., Vancouver 8, B.C., Canada
THOMOPOULOS, Panayotis A., 5 Ravine St., Athens 140, Greece
THOMPSON, Arthur A., Jr., Virginia Poly. Inst., Dept. of Econ., Commerce Hall, Blacksburg, Va. 24061
THOMPSON, Brinton P., Virginia Military Inst., Dept. of Econ., Lexington, Va.
THOMPSON, Carey C., Univ. of Texas, Dept. of Econ., Austin, Tex.
THOMPSON, Cecil B., 3137 Topaz Lane, Apt. C, Fullerton, Calif. 92631
THOMPSON, Charles M., Jr., North Texas State Col., P.O. Box 5163, Denton, Tex.
THOMPSON, Donald S., 3352 Norwood Rd., Shaker Heights, Ohio 44122
THOMPSON, Douglas N., 738 16th Ave., Salt Lake City 3, Utah
THOMPSON, Earl A., Univ. of California, Los Angeles, Dept. of Econ., Los Angeles, Calif. 90024
THOMPSON, Fred A., Riverside City Col., 3650 Fairfax Ave., Riverside, Calif. 92506
THOMPSON, Gerald E., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
THOMPSON, Helen S., 238 East St., East Walpole, Mass. 02032
THOMPSON, Herbert M., Jr., 4840 Vallejo St., Denver, Colo.
THOMPSON, Howard E., Univ. of Wisconsin, Com. Bldg., Madison, Wis. 53706
THOMPSON, Ivan B., 50 S. Maple Ave., Fairborn, Ohio 45324
THOMPSON, James H., 1346 Dogwood Ave., Morgantown, W.Va.
THOMPSON, John C., 2310 The Village Dr., Louisville 5, Ky.
THOMPSON, Lawrence E., Harvard Grad. Sch. of Bus. Admin., Soldiers Field, Boston 63, Mass.
THOMPSON, Orville K., 9732 St. Andrews Dr., Fairfax, Va.
THOMPSON, Paul J., 211 Beech St., Mankato, Minn.
THOMPSON, Ralph B., Univ. of Florida, Col. of Bus. Admin., Dept. of Mktg., Gainesville, Fla.
THOMPSON, Robert R., 3873 W. 54th St., Los Angeles, Calif. 90043
THOMPSON, Robert W., McMaster Univ., Dept. of Polit. Econ., Hamilton, Ont., Canada
THOMPSON, Samuel H., Iowa State Univ., Ames, Iowa 50010
THOMPSON, W. N., Univ. of Illinois, Dept. of Agric. Econ., 304 Mumford Hall, Urbana, Ill. 61803
THOMPSON, Walter F., 350 E. Main St., Grove City, Pa. 16127
THOMPSON, Wilbur R., Wayne State Univ., Dept. of Econ., Detroit 1, Mich.
THOMPSON, William D., 211 Travelstead, Carbondale, Ill.
THOMPSON, William F., 106 Debaran Lane, Rosemont, Pa. 19010
THOMSON, Dorothy L., 4 Stockbridge Rd., Yonkers, N.Y.
THOMSON, Herbert, Muskingum Col., New Concord, Ohio 43762
THOMSON, Procter, Claremont Men's Col., Econ. Dept., Claremont, Calif.
THORELLI, Hans B., Indiana Univ., Sch. of Bus., Bloomington, Ind.
THORELLI, (Mrs.) Sarah S., Indiana Univ., Sch. of Bus., Bloomington, Ind.
THOREN, Raymond P., 445 E. 86th St., New York, N.Y.
THORKELSON, H. John, Univ. of Connecticut, U-63, Storrs, Conn. 06268
THORN, Richard S., 1347 Shady Ave., Pittsburgh, Pa. 15217
THORNBURG, Jason D., 24450 Rockford Ave., Dearborn, Mich.
THORNBURY, Arthur S., 3044 W. Grand Blvd., Detroit 2, Mich.
THORNDIKE, Samuel L., Jr., Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee, Wis. 53211
THORNE, Alfred P., Univ. of Puerto Rico, Facultad De Ciencias Sociales, Rio Piedras, P.R. 00745
THORNER, Daniel, 9 rue Guy-de-la-Brosse, Paris 5, France
THORNHILL, Jack N., East Carolina Col., Sch. of Bus. Admin., Greenville, N.C.
THORNTON, Allan F., 4822 Upton St., N.W., Washington 8, D.C.
THORNTON, Jack E., The Citadel, Dept. of Bus. Admin., Charleston, S.C.
THORP, (Mrs.) Clarice B., Amer. Embassy-USRO, APO New York 09777
THORP, Willard L., Amer. Embassy/USRO, APO New York 09777
THORSON, Douglas Y., Bradley Univ., Dept. of Econ., Peoria, Ill. 61606
THORSON, Joseph M., USAID Liberia, c/o American Embassy, Monrovia, Liberia
THROCKMORTON, H. Bruce, 1287 Virginia Ave., Lakewood, Ohio 44107

THROOP, Adrian W., Univ. of Pennsylvania, Wharton Sch., Econ. Dept., Philadelphia, Pa. 19104
THROUP, (Mrs.) Mary A., 4 Riverview Rd., Apalachin, N.Y.
THUNBERG, (Mrs.) Penelope H., U.S. Tariff Comm., 8th and E Sts., N.W., Washington, D.C. 20436
THUNBERG, Rudolf, 98 Remsen St., Brooklyn, N.Y. 11201
THUROW, Lester C., Harvard Univ., Littauer Center, Rm. 321, Cambridge, Mass. 02138
THWEATT, William O., Amer. Consulate Gen. —SP, APO New York 09676
TICKNOR, Thomas J., Kalamazoo Col., 305 Hoben Hall, Kalamazoo, Mich.
TICKTON, Sidney G., 1180 Ave. of the Americas, New York, N.Y. 10036
TIE, Khouw Bian, MOTEX Corp., 375 Park Ave., Suite 1201, New York, N.Y. 10022
TIEBOUT, Charles M., Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
TIEMANN, Arthur E., 8805 Sundale Dr., Silver Spring, Md.
TIERNO, Angelo J., 2456 Silver Oak Rd., Pittsburgh 20, Pa.
TIETENBERG, Thomas H., 2611 Arlington Blvd., Apt. 202, Arlington, Va. 22201
TILL, Irene, 4630 N. 32nd Rd., Arlington 7, Va.
TILL, Thomas E., Christ the King Rectory, 3001 Belmont Blvd., Nashville, Tenn. 37212
TILLSON, George E., Mobile Chem., Mktg. Res., Kordite Dept., Macedon, N.Y. 14502
TILTON, John E., 2000 4th St. S., Apt. 4, Arlington, Va. 22204
TIMBERG, Eleanor E., 3519 Porter St., N.W., Washington 16, D.C.
TIMBERG, Sigmund, 3519 Porter St., N.W., Washington 16, D.C.
TIMBERLAKE, Richard H., Jr., Univ. of Georgia, Dept. of Banking and Fin., Athens, Ga.
TIMLIN, Mabel F., 1201 Spadina Crescent, E., Saskatoon, Sask., Canada
TIMMONS, John F., Iowa State Univ., Ames, Iowa
TIMMS, Howard L., 1333 E. Davis St., Bloomington, Ind.
TINARI, Frank D., 2465 Cambreleng Ave., Bronx, N.Y. 10458
TINBERGEN, Jan, 31 Haviklaan, The Hague, The Netherlands
TINNEY, Robert W., 3021 Knight, Apt. 112, Dallas, Tex. 75219
TINSLEY, John F., P.O. Box 313, Madison, N.J. 07940
TINSLEY, Peter A., 3334 Lockheed Blvd., #202, Alexandria, Va. 22306
TINTNER, Gerhard, Univ. of Southern Calif., Dept. of Econ., University Park, Los Angeles 7, Calif.
TIONGSON, Simplicio A., 720 W. 27th St., Apt. 348, Los Angeles 7, Calif.
TIPPETTS, Charles S., Route 3, Box 78B, Annapolis Rd., Annapolis, Md. 21403
TIPTON, (Mrs.) Gene B., 12116 E. Magnolia St., El Monte, Calif. 91732
TIRADO, Irma G., 109 2-B University Ave., Rio Piedras, P.R. 00925
TITUS, Paul M., Kenyon Col., Gambier, Ohio 43022
TJERSLAND, Tore, 3323 Oliphant, San Diego, Calif. 92106
TOAN, Cao, c/o USAID Vietnam, Amer. Embassy, Saigon, Vietnam
TOAN, (Mrs.) Edith B., 299 Elm Rd., Briarcliff Manor, N.Y. 10510
TOBBE, C. Walter, Manhattan Col., Christian Brothers Residence, Bronx, N.Y. 10471
TOBEY, Harry G., 4531 Province Line Rd., Princeton, N.J. 08540
TOBIER, Emanuel, c/o Regional Plan Asso., 230 W. 41st St., New York, N.Y. 10036
TOBIN, James, Box 2125 Yale Sta., New Haven, Conn.
TODA, Yasushi, Harvard Univ., Russian Res. Center, 1737 Cambridge St., Cambridge, Mass. 02138
TODD, Arthur W., Lincon Electric Co., 22801 St. Clair St., Cleveland 17, Ohio
TODD, B. J., North Carolina State Univ., Dept. of Econ., 220 1911 Bldg., Raleigh, N.C. 27607
TODD, Norman W., 329 Spalding Rd., Sharpley, Wilmington 3, Del.
TODD, Paul, Jr., Box 511, Kalamazoo, Mich.
TODD, Phillips, Box 7, Danbury, Neb.
TODD, Ralph H., Eastern Oregon Col., Dept. of Econ., LaGrande, Ore.
TODD, William F., Arabian Amer. Oil Co., 505 Park Ave., New York 22, N.Y.
TODER, Eric J., Dept. of Econ., Harkness 204, Univ. of Rochester, Rochester 27, N.Y.
TOKAR, John A., 126 Hicks Lane, Great Neck, N.Y.
TOKUNAGA, Edward M., 6124 W. 2nd St., Rio Linda, Calif. 95673
TOLER, Ralph E., 2814 Janney Ave., Muncie, Ind. 47306
TOLFSEN, (Mrs.) Miriam, 34-19 90th St., Apt. B 23, Jackson Heights, N.Y. 11372
TOLLES, N. Arnold, Cornell Univ., New York State Sch. of Ind. and Labor Rela., Ithaca, N.Y.
TOLLEY, George S., 902 Lake Boone Trail, Raleigh, N.C.
TOLLEY, Howard E., 10043 Glenmere Rd., Fairfax, Va. 22030
TOLLI, (Mrs.) Ellinor F., The College Inn, Box 411, 1729 Athens St., Boulder, Colo. 80302
TOM, C. F. Joseph, Lebanon Valley Col., Annville, Pa. 17003
TOMANN, Horst, Pfarrstr. 3, 88 Ansbach, Germany
TOMASEVICH, Jozo, 2815 S. Court, Palo Alto, Calif.
TOMASKE, John A., 7943 E. Nannestad St., South San Gabriel, Calif. 91777
TOMBER, Milton W., 275 Central Park West, New York 24, N.Y.
TOMES, George K., International Harvester Co., 401 N. Michigan Ave., Chicago, Ill. 60611
TOMEY, Edward A., 909 Lanyard Lane, Kirkwood, Mo. 63122
TOMLIN, Breffni, 59, Beechpark Ave., Blackrock, Dublin, Ireland
TOMLINSON, Alan S., 11 W. Welling Ave., Pennington, N.J. 08534
TOMLINSON, Percy, 6024 Lakeview Dr., S.W., Calgary, Alta., Canada
TOMPA, Robert J., Monmouth Col., Dept. of Bus. Admin., West Long Branch, N.J. 07764
TOMPSON, Walter, P.O. Box 5452, Los Angeles, Calif. 90055
TONER, Jerome L., St. Martins Abbey, St. Martins Col., Olympia, Wash.
TONG-PIL, Cho, Korea Univ., Asiatic Res. Center, Anam-dong, Songbuk-ku, Seoul, Korea
TONGUE, William W., 212 Shoreline Dr., Park Ridge, Ill. 60068
TOOL, Marcus R., Sacramento State Col., Sacramento 19, Calif.
TOOLE, George H., 9218 Brian Dr., Vienna, Va. 22180
TOPKIS, Bernard H., 3212 Northampton St., N.W., Washington 15, D.C.
TOPPING, Frances K., 2800 Woodley Rd., N.W., Washington 8, D.C.
TORDA, Theodore S., 2374 Euclid Heights Blvd., Apartment 202, Cleveland 6, Ohio
TORELLI, Giovanni A., Int. Bank for Recon. and Dev., 1818 H St., N.W., Washington 25, D.C.
TOREN, James W., c/o The Donner Corp., 1710 Fidelity-Philadelphia Trust Bldg., 123 S. Broad St., Philadelphia, Pa.
TORGERSON, Harold W., Northwestern Univ., Sch. of Com., Evanston, Ill.
TORGERSON, Malcolm S., Arizona State Col., Sch. of Bus. Admin., Flagstaff, Ariz.
TORI, Carol A., 100 Poe St., Hartsdale, N.Y. 10530
TORRES, James F., Montes Urales 610, Mexico 10, D.F., Mexico
TORRES-HASSEY, Arturo G., Carlos Fernandez No. 3, Colonia del Valle, Mexico 12, D.F., Mexico
TORRUELLAS, Luz M., Univ. of Puerto Rico, Dept. of Soc. Sci., Rio Piedras, P.R. 00745
TORTO, Raymond G., 5-A Evans Rd., Marblehead, Mass.
TORUN, Teoman Y., Hazine Genel Mudurlugu, Ankara, Turkey
TOSCANO, Peter R., 206 W. Jackson St., Macomb, Ill. 61455
TOSDAL, Harry R., 70 Bow Rd., Belmont, Mass. 02178
TOTTEN, Donald W., 305 Donahoo St., Statesboro, Ga.

TO-U, Nam, Kukmin Col., Changson-dong, Chongno-ku, Seoul, Korea
TOUNTAS, Socrates, 1912 E. Beverly Rd., Milwaukee, Wis. 53211
TOUQAN, Adber R. S., Jordan Dev. Bd., Amman, Jordan
TOUSLEY, Rayburn D., Washington State Univ., Col. of Econ. and Bus., Pullman, Wash.
TOVAR, Renato, Rua Roquete Pinto 88, Apt. 301, Rio de Janeiro, Brazil
TOVIAN, (Mrs.) Elizabeth, 58 Belknap Rd., West Hartford, Conn. 06117
TOW, Clarence W., Fed. Res. Bank of Kansas City, Kansas City, Mo. 64106
TOWATA, Arthur T., 421 Belleview Ave., Alton, Ill.
TOWBER, Richard I., 85-15 Main St., Briarwood, Queens, N.Y.
TOWER, Edward, 130 Morris Ave., Providence 6, R.I.
TOWER, John E., 286 LaSalle St., Buffalo, N.Y. 14215
TOWEY, Richard E., Oregon State Univ., Dept. of Econ., Corvallis, Ore. 97330
TOWLE, Lawrence W., 11 Riggs Ave., West Hartford, Conn. 06107
TOWNE, Arthur W. H., 29 Hickory Hill Rd., Eastchester, N.Y. 10709
TOWNE, Marvin W., USAID, Box A, APO New York 09694
TOWNSEND, David, Northwestern State Col. of Louisiana, Sch. of Arts and Sci., Natchitoches, La. 71457
TOWNSEND, Everett B., Beaver Col., Grey Tower Campus, Easton and Church Rds., Glenside, Pa. 19038
TOWNSEND, Horace, Jr., Philadelphia Col. of Textiles and Sci., School House Lane, Philadelphia, Pa. 19144
TOWNSEND, Paul A. R., 50 Franklin Ave., Montreal 16, Que., Canada
TOWNSEND, Roswell G., Wilson Col., Chambersburg, Pa. 17201
TOWNSEND, William S., 5713 Highland Hills Dr., Austin 31, Tex.
TOWNSHEND-ZELLNER, Norman, California State Col., Dept. of Econ., Fullerton, Calif.
TOYER, Aurelia, 750 St. Marks Ave., Brooklyn 16, N.Y.
TOZZI, Jim J., 1946 Kennedy Dr., McLean, Va. 22101
TRACH, Jerry S., 55 Seneca Ave., Staten Island, N.Y. 10301
TRAEDER, Maynard A., Midland Lutheran Col., Dept. of Bus. and Econ., Fremont, Neb. 68025
TRAINER, Orvel L., 1302 15th Ave., Greeley, Colo.
TRAINER, Patricia D., 221 S. Gale Dr., Beverly Hills, Calif. 90211
TRAINER, Richard D., Ohio Univ., 72 University Ter., Athens, Ohio 45701
TRAINOR, John F., Kamiak Apts., Apt. A-4, Pullman, Wash. 99163
TRAINOR, Richard J., 1443 Dewberry Ct., McLean, Va. 22101
TRANT, James B., 9132 Redbud St., Baton Rouge, La.
TRAORE, Diawa M., Ministry of Nat. Educa., Conakry, Guinea
TRAPPENIERS, Felix, 308 Boulevard du Bouverlain, Brussels 16, Belgium
TRAVIS, Robert W., Div. of Bus., San Jose State Col., San Jose 14, Calif.
TRAYLER, Orba F., Univ. of Alabama, P.O. Box 1247, Huntsville, Ala. 35807
TRAYWICK, Leland E., 651 N. 59th St., Omaha, Neb. 68132
TREACY, John J., Texas A. and M. Univ., College Station, Tex.
TREADWAY, Arthur B., 5110 S. Kenwood Ave., #601, Chicago, Ill. 60615
TREANOR, Glen R., 5526 Greystone St., Chevy Chase, Md. 20015
TREBING, Harry M., Indiana Univ., Sch. of Bus., Bloomington, Ind.
TREDWELL, Thomas A., 402 Alder Lane, San Mateo, Calif. 94403
TREFFTZS, Kenneth L., 5402 Weatherford Dr., Los Angeles, Calif. 90008
TREJO, Saul, 2 Yale Sta., New Haven, Conn. 06520
TREMBLAY, Rodrigue, 555 Westridge Dr., Portola Valley, Calif. 94026
TREMEWAN, A. Dolores, 626 N. Virginia, Reno, Nev.
TREML, Vladimir G., Franklin and Marshall Col., Dept. of Econ., Lancaster, Pa.
TRENCHARD, George O., Firestone Tire and Rubber Co., Akron 17, Ohio
TRENCK, Stephonie M., Washington Univ., Dept. of Econ., St. Louis, Mo.
TRENDOWSKI, Thomas W., 20051 Regent Dr., Detroit 5, Mich.
TRENTON, Rudolph W., Oklahoma State Univ., Sch. of Com., Dept. of Econ., Stillwater, Okla.
TRESCOTT, Paul B., GPO Box 2453, Bangkok, Thailand
TRESCOTT, (Mrs.) Ruth P., GPO Box 2453, Bangkok, Thailand
TRESPASZ, Walter R., 27 Hickory Rd., West Orange, N.J.
TRESTRAIL, Richard W., Batelle Memorial Inst., 505 King Ave., Columbus, Ohio 43201
TRETHEWEY, Richard J., 615 W. Nickerson, #301, Seattle, Wash. 98119
TREVINO, Francisco Abel, C. Salazar OTC. 1245, Monterrey, N.L., Mexico
TREYZ, George I., 749 Rugby Rd., Bryn Mawr, Pa.
TREZEVANT, Walter S., 4921 1st St., N.W., Washington, D.C. 20011
TREZISE, Philip H., Amer. Embassy USOECD, APO New York 09777
TREZZA, Bruno, Via S. Gennaro al Vomero 20, Naples, Italy
TRIANTIS, Stephen G., Univ. of Toronto, Dept. of Polit. Econ., 100 St. George St., Toronto 5, Ont., Canada
TRICE, Andrew H., 440 46th St., Sacramento 19, Calif.
TRIEU, Vo Long, Agric. Econ. and Statis. Serv., 28 Bis, Mac Dinh Chi, Saigon, Vietnam
TRIFFIN, Robert, Yale Univ., 37 Hillhouse Ave., New Haven, Conn. 06520
TRIFON, Raphael, c/o Dept. of Ind. and Mgt. Eng., Technion, Haifa, Israel
TRINKLEY, Raymond W., 481 Papurah Rd., Fairfield, Conn.
TRIPLETT, Jack E., Jr., Univ. of Oregon, Inst. of Ind. and Labor Rela., Eugene, Ore. 97403
TRIPP, Chester D., 200 S. Michigan Ave., Chicago, Ill. 60604
TRIPP, L. Reed, Lehigh Univ., Col. of Bus. Admin., Bethlehem, Pa.
TRISKO, Ralph L., 12901 Cherrywood Lane, Bowie, Md. 20715
TRIVEDI, A. R., 1973 Dracena Dr., Los Angeles 27, Calif.
TRIVOLI, George W., 219 14th St., Apt. 4, Charlottesville, Va. 22903
TROOP, Andrew H. S., 88 Bellamy Rd., South, Scarborough, Ont., Canada
TROTH, John R., 2524 Franklin St., Bellevue, Neb. 68005
TROTTER, Leona, Fairleigh Dickinson Univ., Teaneck, N.J.
TROTTER, Richard G., 16 S. 37th St., Philadelphia, Pa.
TROXEL, C. Emery, Wayne Univ., Dept. of Econ., Detroit 1, Mich.
TROY, Leo, 5 Lakeview Dr., West Orange, N.J. 07052
TROZZO, Charles L., Herminie, Pa. 15637
TRUBAC, Edward R., 19160 Wedgewood Dr., South Bend, Ind. 46637
TRUEBLOOD, Lorman C., 5914 Harwick Rd., Washington, D.C. 20016
TRUED, Merlyn N., Treasury Dept., Rm. 3434, Washington, D.C. 20220
TRUESDELL, Leon E., 3429 Ordway St., N.W., Washington 16, D.C.
TRUITT, William J., Louisiana State Univ., Baton Rouge, La. 70803
TRUJIC, Aleksandar, Planning Inst., People's Comm. of Belgrade, Belgrade, Yugoslavia
TRUMAN, Edwin M., 31 Elliott St., New Haven, Conn. 06519
TRUMBO, Mark, Aurora Col., Aurora, Ill. 60507
TRUNDLE, Sidney A., Jr., Manufacturers Hanover Trust Co., 350 Park Ave., New York 22, N.Y.
TRUSK, Robert J., United Nations, Rm. 2608, New York, N.Y.

TRUSTY, Wayne B., 198 Springbank Dr., Lot 115, London, Ont., Canada
TRYON, Joseph L., Center for Dev. Plan., 1524 18th St., N.W., Washington, D.C. 20036
TSAGRIS, B. E., Arizona State Univ., Tempe, Ariz.
TSAI, Mau-Suug, Evansville Col., Evansville, Ind.
TSANTIS, Andreas C., Brown Univ., Econ. Dept., Providence, R.I. 02912
TSCHAPPAT, Henry H., Jr., 488 Columbia, Elgin, Ill.
TSENG, Chi-Tung, Nanyang Univ., Jurong Rd., Singapore 22, Malaya
TSIANG, Sho Chieh, 623 Bonnie Brae Ave., Rochester, N.Y. 14618
TSOUR, I. Lang, Univ. of Nevada, Dept. of Econ., Col. of Bus. Admin., Reno, Nev.
TSUKAHARA, Theodore, Jr., 1251 College View Dr., Apt. E, Monterey Park, Calif. 91754
TSURU, Shigeto, Hitotsubashi Univ., Inst. of Econ. Res., Kunitachi, Tokyo, Japan
TU, Nguyeh Manh, Dir., Coop. Res. Training Center, 255, Le Van Duyet, Gia Dinh, Vietnam
TU, Yien-I, Univ. of Kentucky, Dept. of Econ., Lexington, Ky.
TUAN, Nguyen Anh, Dir. Gen. of Plan., Ben Chuong Duong, Saigon, Vietnam
TUAN, Nguyen D., 24/5 Vo Tanh, Phu-Nhuan, Saigon, S. Vietnam
TUCHI, Ben J., St. Francis Col., Dept. of Bus. Admin., Loretto, Pa. 15940
TUCHSCHERER, Thomas H., 2040 N. 72nd Ct., Elmwood Park, Ill. 61529
TUCK, Charles C., Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
TUCKER, Donald P., Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
TUCKER, Donald S., 59 Foster St., Cambridge, Mass. 01922
TUCKER, Murray A., 620 Interlake Dr., Madison, Wis.
TUCKER, Ralph B., West Lake Ave., Guilford, Conn. 06437
TUCKER, Randall W., Trinity Col, Hartford 6, Conn.
TUCKER, Robert H., P.O. Box 426, Halifax, Va. 24558
TUCKMAN, Howard P., Univ. of Wisconsin, Dept. of Econ., Madison, Wis.
TUERCK, David G., 105-A Cresap Rd., Charlottesville, Va. 22903
TUFTS, Robert W., 352 W. College St., Oberlin, Ohio 44074
TUGADO, Angelesio C., Box 981, Manila, Philippines
TUGMAN, James L., 2399 Edgerton Rd., University Hgts., Cleveland 18, Ohio
TUGMAN, (Mrs.) Lena E., 2399 Edgerton Rd., University Hgts., Cleveland 18, Ohio
TULASIEWICZ, J. Bruno, Iowa Teachers Col., Soc. Sci. Dept., Cedar Falls, Iowa 50613
TULEY, Richard L., 22953 Republic, Riverside, Calif.
TULKENS, Henry G., Center for Operations Res. and Econometrics, 52, de Croylaan, Heverlee-Louvain, Belgium
TULL, D. S., 2026 E. Fern Dr., Fullerton, Calif.
TULLIER, Marion Paul, 2026 Rice Ave., Baton Rouge, La.
TULLOCK, Gordon, Univ. of Virginia, Rouss Hall, Charlottesville, Va.
TULLY, George B., 7832 Luxmore Dr., Knoxville, Tenn. 37919
TUMA, Elias, Univ. of California at Davis, Dept. of Econ., Davis, Calif.
TUMANENG, Erlinda F., St. Mary Col. Le Fer Hall, St. Mary-of-the-Woods, Ind. 47876
TUNG, Nguyen V., 83 Hung Vuong, Thi Nghe, Saigon, Vietnam
TUNG, Paul P., 6137 Tooley St., San Diego, Calif. 92114
TUNG, T. R., Colorado State Univ., Dept. of Econ., Fort Collins, Colo. 80521
TUPA, Edward J., 757 E. Main St., Lansdale, Pa. 19446
TURE, Norman B., Nat. Bur. of Econ. Res., 1755 Massachusetts Ave., N.W., Suite 314, Washington, D.C. 36
TURK, Georges H. E., Beirut Off., c/o Francis DuPont and Co., One Wall St., New York, N.Y. 10005
TURK, Howard S., 8628 Cromwell Dr., Springfield, Va. 22151
TURNBELL, Richard B., 1335 26th Ave., Apt. 1, San Francisco, Calif. 94122
TURNBULL, John G., Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
TURNER, Carl B., G3A University Apts., Durham, N.C. 27701
TURNER, Clarence L., 123 S. Broad St., Philadelphia, Pa. 19109
TURNER, Earl K., Univ. of South Western Louisiana, Box 481, Lafayette, La.
TURNER, Glen C., Colorado State Col., Dept. of Econ., Greeley, Colo.
TURNER, Marjorie S., San Diego State Col., San Diego, Calif. 92115
TURNER, Robert C., Indiana Univ., Sch. of Bus., Bloomington, Ind. 47405
TURNOVSKY, Stephen J., Harvard Univ., 317 Richards Hall, Cambridge 38, Mass.
TUROFF, Sidney, R.R. 1, Box 360, Storrs, Conn. 06268
TURUNEN, Robert E., 11660 Norringham Rd., Detroit, Mich. 48224
TURVEY, Ralph, The Electricity Council, 30 Millbank, London, S.W. 1, England
TUSSING, A. Dale, Syracuse Univ., Dept. of Econ., Syracuse, N.Y. 13210
TUSSING, Arlon R., Univ. of Alaska, College, Alaska 99735
TUSSING, Richard, 1604 W. University Ave., Champaign, Ill. 61821
TUTHILL, John W., USEC, APO New York 09667
TUTTLE, Donald, 1100 Delaney Ave., Apt. A-11, Orlando, Fla. 32806
TUTTLE, Donald L., Univ. of North Carolina, Sch. of Bus. Admin., Chapel Hill, N.C.
TUTTLE, Frank W., Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla.
TUTTLE, William G. T., U.S. Military Academy, Dept. of Soc. Sci., West Point, N.Y. 10996
TWEETEN, Luther G., Oklahoma State Univ., Dept. of Agri. Econ., South Ag. Hall, Stillwater, Okla.
TWEH, H. Roosevelt, Sr., Bur. of Labor, Dept. of Com., Monrovia, Liberia
TYBJERG, Lars, Prinsesse Alexandrines alle 20, Charlottenlund, Denmark
TYBOUT, Richard A., Ohio State Univ., Dept. of Econ., Columbus 10, Ohio
TYCHSEN, Charles E., U.S. Naval Postgrad. Sch., Code 6408, Monterey, Calif. 93940
TYLER, John R., 5621 S. University Ave., Chicago, Ill. 60637
TYRAN, Benjamin, 2780 Ralston Ave., Hillsborough, Calif. 92346
TZANEFF, Steven, 1225 13th St., N.W., Washington 5, D.C.
TZANNETAKIS, George, 19 Brookside Dr., Milburn, N.J. 07041
TZUDIKER, Roland A., 310 Garrison St., Denver, Colo. 80226

UDALL, Alan T., 64 York St., Guilford, Conn. 06437
UDIS, Bernard, Univ. of Colorado, Dept. of Econ., Boulder, Colo. 80304
UFEN, Charles H., North Carolina State Univ., Econ. Dept., Raleigh, N.C. 27607
UGOH, Sylvester, Univ. of Nigeria, Dept. of Econ., Nsukka, Eastern Nigeria
UHLER, Robert G., Capt. INF 086680, HQS 24th INF. DIV., APO New York, N.Y. 09112
UHLER, Russell S., 1111 N. Dartmouth, Apt. 221, Claremont, Calif.
UHLINGER, Charles W., 32 Crocker St., Rockville Centre, N.Y.
UHR, Carl G., Univ. of California, Div. of Soc. Sci., Riverside, Calif.
ULFFERS, Carl A., 195 Broadway, New York 7, N.Y.
ULIN, Jeremy C., 1606 32nd St., N.W., Washington, D.C. 20007
ULIN, Robert P., Socony Mobil Oil Co., Dept. of Econ., 150 E. 42nd St., New York 17, N.Y.
ULLIAN, Frieda S., 35 Dolphin Rd., Newton Centre, Mass. 01355
ULLMAN, Reginald G., 44 Knollbrook Rd., Rochester, N.Y. 14610

ULLMANN, Oswald, Int. Econ. Publishers, Inc., 4000 Brandywine St., N.W., Washington, D.C. 20016
ULLRICH, Francis J., 2621 Palisade Ave., New York 63, N.Y.
ULMAN, Lloyd, 776 Creston Rd., Berkeley, Calif. 94708
ULMER, Carl D., 1282 Stoltz Rd., Bethel Park, Pa. 15102
ULMER, Melville J., Haringvliet 62, Rotterdam 1, Netherlands
ULREY, Ivon W., 4075 Sunridge Rd., Pebble Beach, Calif. 93953
ULRICH, Herbert, 403 Ratingen, Industriestrasse 48, Germany
ULSETH, George W., 615 E. Polk Ave., Eau Claire, Wis. 54701
UMBERGER, Paul M., Richmond Professional Inst., 901 West Franklin St., Richmond 20, Va.
UMBRETT, Myron H., 2111 Ridge Ave., Evanston, Ill.
UMSTOTT, Haven D., 1518 N. Ivanhoe St., Arlington 5, Va.
UNDERHILL, H. Fabian, 518 N. Delaware St., Indianapolis, Ind. 46204
UNDERWOOD, Donald M., Knox Col., Sellew House, 553 S. West St., Galesburg, Ill.
UNGER, Joseph, 30-A Joralemon St., Apt. 19, Brooklyn, N.Y. 11201
UNGER, Samuel G., Michigan State Univ., Agric. Econ. Dept., East Lansing, Mich. 48823
UNTERBERGER, S. Herbert, 129 Broome Lane, Merion, Pa. 19066
UONG, Tran Cu, Off. of the Vice Premier, 17, Ben Chuong Duong, Saigon, Vietnam
UPDEGRAFF, Gail E., 129-1/2 N. Sheldon Ave., Ames, Iowa 50010
UPDIKE, Helen H., 46 Reservoir St., Cambridge, Mass. 02138
UPGREN, Arthur R., MacAlester Col., St. Paul 1, Minn.
UPHOFF, Walter H., 705 Marine Avenue, Boulder, Colo. 80302
UPPAL, Jogindar S., Michigan State Univ., Dept of Soc. Sci., Bessey Hall, East Lansing, Mich. 48823
UPRAVAN, Souk, Comm. for Rural Dev., Commissariat aux Affaires Rural, Vientiane, Laos
UPSON, Brent T., 3044 W. Grand Blvd., Detroit 2, Mich.
UPTON, R. Miller, 709 College St., Beloit, Wis. 53511
URBAIN, Yves, 115, Chaussee de Tirlemont, Corbeek-Lo, Louvain, Belgium
URBAN, Francis, St. Michael's Col., Cerrillos Rd., Santa Fe, N.M. 87502
URBANTKE, Hugh E., P.O. Box 2135, Houston Col. Sta., Huntsville, Tex.
URI, George, Irelan, Uri, Mayer and Sheppue, P.O. Box 7929, San Francisco, Calif. 94119
URNER, Joseph F., 533 Pleasant St., Belmont, Mass. 02178
URQUHART, Norman R., 220 Blauvelt Rd., Pearl River, N.Y. 10965
URQUIDI, Victor L., Apartado Postal 2263, Mexico 1, D.F., Mexico
USELDING, Paul, 1410 Chicago Ave., Evanston, Ill. 60201
UTSUMI, Yoichi, 39 2-chome Sekimori-cho Suma-ku, Kobe, Japan
UYEN, Trinh Thi Ngoc, Pub. Relations Mgr., Societe Shell, Duong Thong Nhat, Saigon, Vietnam
UZAWA, Hirofumi, Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637

VACANTE, Frank J., 920 S. Franklin St., Wilkes-Barre, Pa.
VACCARA, (Mrs.) Beatrice, 11228 Bybee St., Silver Spring, Md.
VACCHELLI, Giovanni, Piazza Sallustio No. 9, Rome, Italy
VADAKIN, James C., Univ. of Miami, Dept. of Econ., Coral Gables, Fla. 33926
VAGHEFI, Reza, Michigan State Univ., Grad. Sch. of Bus. Admin., Eppley Center, East Lansing, Mich. 48823
VAILE, Roland S., 935 W. 3rd Ave., Escondido, Calif.
VAKIL, Firouz, 2644 Hillegass Ave., Berkeley, Calif. 94704
VALASKAKIS, Kimon, 922 E. Shore, Ithaca, N.Y.
VALAURI, (Mrs.) Fadra T., 29-49 169th St., Flushing 58, N.Y.
VALENTINE, Dennis E., 1310 Springvale, San Antonio, Tex. 78227
VALENTINE, Lloyd M., Univ. of Cincinnati, Col. of Bus. Admin., Cincinnati, Ohio 45221
VALENTINE, Roger W., c/o Halsey, Stuart and Co., 123 S. La Salle St., Chicago, Ill. 60690
VALETTE, Jean-Paul, 43 Algonquin Rd., Chestnut Hill, Mass. 02167
VALIUNAS, Andrew V., 2032 Belmont Rd., N.W., Washington, 9, D.C.
VALLDEJULI, Juan J., 2422 N. Washington Blvd., Arlington, Va. 22201
VALLES, Jean-Paul, 29 Tulip Lane, Coltsneck, N.J.
VALLEY, Brother Philip, St. Anselm's Col., Manchester, N.H. 03102
VAN ALSTYNE, Carol, North Carolina Fund, P.O. Box 687, Durham, N.C. 27702
VAN AMERONGEN, Pieter A., 7519 20th Ave., N.E., Seattle, Wash. 98115
VAN ARSDELL, Paul M., Univ. of Illinois, David Kinley Hall 311, Urbana, Ill.
VAN AUKEN, K. G., Jr., 2929 University Blvd., West, Kensington, Md. 20795
VAN BALEN, Richard P., Duke Univ., Men's Grad. Center, Box 1120, Durham, N.C.
VAN BEEK, Frits, 3450 Toledo Ter., 603, Hyattsville, Md.
VANCE, David H., 6324 Rockhurst Rd., Bethesda, Md. 20034
VANCE, Lawrence L., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
VAN COLLER, David L., P.O. Box 4587, Johannesburg, S. Africa
VAN DAHM, Thomas E., Carthage Col., Kenosha, Wis.
VAN DE BUNT, Wouter, Market Resch., Amer. Airlines, Inc., 633 Third Ave., New York, N.Y. 10017
VAN DE KLUNDERT, Theo, Vijverlaan 35, Tilburg, The Netherlands
VANDELL, Robert F., 1438 Cove Rd., Charlottesville, Va.
VANDEN BELT, Ben H., Jr., 648 S. Meridan, Dearborn, Mich.
VANDEN BERG, John, Calvin Col., Grand Rapids, Mich.
VAN DEN DOOL, Peter, Univ. of Santa Clara, Sch. of Bus., Santa Clara, Calif.
VAN DEN HAAG, Ernest, 52 W. 9th St., New York 11, N.Y.
VAN DERAA, Patricia, 4501 Connecticut Ave., Washington, D.C. 20008
VANDERBERG, Richard D., 2309 Cypress Way, Apt. 24, Madison, Wis. 53713
VAN DER BRUGH, Pieter G.F.H., 1252 Surrey Ave., Ottawa 10, Ont., Canada
VANDERMEULEN, (Mrs.) Alice J., P.O. Box 536, Claremont, Calif. 91711
VANDERMEULEN, Daniel C., P.O. Box 536, Claremont, Calif. 91711
VAN DER SPEK, Peter G., 1021 S. Jefferson St., Apt. 2, Allentown, Pa. 18103
VAN DER VOORT, Aitcheson B., 1121 Arlington Blvd., Apt. T-208, Arlington 9, Va.
VAN DER WOLF, Berhard H. A., Driekomingenlaan 27, Zierikzee, The Netherlands
VAN DEWATER, John, 18203 Muirland Ave., Detroit 21, Mich.
VAN DUIJN, Gerrit, Hoogvortsweg 15, Tervuren, Belgium
VANEK, Jaroslav, 414 Triphammer Rd., Ithaca, N.Y. 14850
VAN ECK, L. Frederick, Jr., Univ. of Missouri, B and PA Sch., Columbia, Mo.
VAN FLEET, David D., Univ. of Tennessee, Dept. of Ind. and Personnel Mgt., GBA 124, Knoxville, Tenn. 37916
VAN GIESON, Richard A., 367 Bruce, Rohnert Park, Calif. 94928
VAN HAMEL, B. A., Populierstraat 29, The Hague, The Netherlands
VAN HANDEL, Robert, 6 Hillside Ct., Endicott, N.Y.
VAN HORN, Edna, 1901 E. 13th Ave., Denver, Colo. 80206
VAN HORNE, James, Stanford Univ., Grad. Sch. of Bus., Stanford, Calif.

VAN INGEN, John V., 505 Jersey St., College Station, Tex. 77840
VAN KLEECK, Mary, 150 Byrdcliffe Rd., Woodstock, N.Y. 12498
VAN LIERDE, Paul A., 47 Ross, Chateauguay, P.Q., Canada
VAN LOO, Frances, 2409 College, Apt. 12, Berkeley, Calif. 94704
VAN MEIR, Lawrence W., 4801 Camelot St., Rockville, Md. 20853
VAN METER, Michael, Midwest Res. Inst., Econ. Dev. Div., 425 Volker Blvd., Kansas City, Mo. 64110
VAN MOESEKE, Paul, Univ. de Louvain, Cent. de Recherches Econ., 2-B Van Evenstreet, Louvain, Belgium
VAN NEST, William, 44 Putnam St., Somerville, N.J. 08876
VAN NIMMEN, Armand M. J., 2512 Que St., N.W., #307, Washington, D.C.
VAN PESKI, Neva G., Div. of Res. and Statis., Board of Gov. of the Fed. Res. System, Washington, D.C. 20551
VAN POPTA, S., Dr. Schuitstraat, Beverwijk, Netherlands
VAN ROY, Edward, State Univ. of New York at Stony Brook, Dept. of Econ., Stony Brook, L.I., N.Y. 11790
VAN SANT, Edward R., 608 Malcolm Pl., Alexandria, Va. 22302
VANSANT, Franklin D., 304 Pooks Hill Apts., 3 Pooks Hill Rd., Bethesda, Md. 20014
VAN SANTEN, Fred, 24 Emmalaan, Amsterdam 2, The Netherlands
VAN SCHAIK, Gerard, 1455 Vermont St., Sherbrooke, Que., Canada
VAN SCOYOC, Leland S., 1370 E. Wooster St., Bowling Green, Ohio 43402
VAN SEVENTER, A., 1033 Michigan Ave., Ann Arbor, Mich.
VAN SICKLE, John V., 617 Thornwood Rd., Crawfordsville, Ind. 47933
VAN SNELLENBERG, Elayne, 681 Spadina Ave., Toronto 8, Ont., Canada
VAN TASSEL, Alfred J., 105 W. 55th St., New York 19, N.Y.
VAN TASSEL, Charles E., 14007 Woodthorpe Lane, Houston, Tex. 77024
VAN TASSEL, Roger C., Clark Univ., Dept. of Econ., Worcester, Mass.
VAN VALKENBURG, D. A., Malone Col., 515 25th St., N.W., Canton 9, Ohio
VAN VLACK, Philip W., Tarkio Col., Tarkio, Mo. 64491
VAN WAGSTAFF, J., Wake Forest Col., Sch. of Bus. Admin., Box 7285, Winston-Salem, N.C.
VAQAR, Nasrollah, 2109 N.W. Walmer Dr., Portland, Ore. 97229
VARDY, David A., 229 Kenmount Rd., St. John's, N.F., Canada
VARGHESE, K. Thomas, Wayne State Univ., Dept. of Econ., Detroit, Mich. 48202
VARKI, Kodiyat, Y-14 Haus Khas, New Delhi, India
VARKI, (Mrs.) Thankama, Y-14 Haus Khas, New Delhi, India
VARMA, L. R., Labour Bur., Kennedy House, Simla-4, India
VARMA, Ranbir, 685 West End Ave., Apt. 6C, New York 25, N.Y.
VARNER, Albert J., 8532 Freyman Dr., Apt. 119, Chevy Chase, Md. 20015
VARS, R. Charles, Jr., 2340 Grant St., Berkeley, Calif. 94703
VASS, Steven T., 1738 Murfin Ave. 27, Ann Arbor, Mich.
VATTER, (Mrs.) Ethel G., Cornell Univ., Col. of Home Econ., Ithaca, N.Y.
VATTER, Harold G., Portland State Col., Div. of Soc. Sci., Dept. of Econ., Portland, Ore.
VATTER, Robert H., 110 Lenape Rd., Pearl River, N.Y. 10965
VATTER, William J., Univ. of California, Sch. of Bus. Admin., Berkeley 4, Calif.
VAUGHAN, Claude, 308C Havely Dr., Lexington, Ky. 40508
VAUGHAN, Olive E., 222 E. 19th St., New York, N.Y. 10003
VAUGHN, Gerald F., Maryland State Plan. Dept., 301 W. Preston St., Baltimore, Md. 21201
VAUGHN, James A., 581 Cambridge St., Allston, Mass. 01001
VAUGHN, Louis A., Lockheed Aircraft Corp., Cal. Div., ATTN: Engineering Lib., Plant B-1, Dept. 74-13, Bldg. 170, Burbank, Calif. 91503
VAUGHN, Ralph E., 86-05 60th St., Elmhurst, N.Y. 11373
VAZQUEZ, Hector I., 94-10 60th Ave., Apt. 5-A, Rego Park, L.I., N.Y. 11373
VEAZIE, Carl E., 75 Starr Ave., Newington, Conn.
VEDDER, James N., Syracuse Univ., 207 Sims Hall, Syracuse, N.Y. 13210
VEDDER, Richard K., 34 N. McKinley Ave., Apt. 408, Athens, Ohio 45701
VEGA, Jose, Univ. of Chicago, Dept. of Econ., Chicago, Ill. 60637
VEGH, Jeno, P.O. Box 375, Greenwich, Conn. 06831
VEINER, Martin I., 331 Cypress St., Melbourne, Fla. 32903
VEIT, Lawrence A., 401 E. 86th St., New York, N.Y. 10028
VELASCO, Luis E., 220 E. 53rd St., New York, N.Y. 10022
VELAY, Clement C., 15 Chemin de la Vie des Champs, Geneva, Switzerland
VELAZCO, Alonso, 421 Church St., Ann Arbor, Mich. 48104
VELEZ-MONTES, Manuel A., Col. of Soc. Sci., Dept. of Econ., Rio Piedras, P.R. 00745
VELK, Thomas J., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
VELLOSO, Joso P., Rua general Uriquiza 63 Apt., 102-Leblon, Rio de Janeiro, Brazil
VENTURA, Jack S., 2000 N. Adams St., Apt. 733, Arlington, Va. 22201
VERBECK, Xavier H., Dutchess Col., Pendell Rd., Poughkeepsie, N.Y.
VERBRUGGE, James A., Univ. of Kentucky, Col. of Comm., Lexington, Ky. 40506
VERDOORN, P. J., Van Diepenburchstraat 122, The Hague, The Netherlands
VERGARA L. DE S.R., Jose, Pase de Moret 9, Madrid, Spain
VERMA, Sham Das, Plan. Comm., New Delhi, India
VERNON, Jack R., Michigan State Univ., Dept. of Econ., East Lansing, Mich.
VERNON, Raymond, 1 Dunstable Rd., Cambridge, Mass. 02138
VERNON, Thomas T., 2021 Anderson Ave., Manhattan, Kans. 66504
VERSTRAETE, Juan, Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
VERZILLI, Andrew G., 19 Pickwick Dr., Doylestown, Pa. 18901
VESSER, Dale A., Military Educ. Foundation, Box 192, Highland Falls, N.Y. 12528
VESTERGAARD, Harald, Ornekulsvej 11, Charlottenlund, Denmark
VETTERLING, Philip W., 1415 Homeric Ct., McLean, Va. 22101
VEVERKA, Jindrich, Univ. of Leicester, Dept. of Econ., Leicester, England
VEYSOGLU, Recep, Western Michigan Univ., Dept. of Econ., Kalamazoo, Mich. 49001
VICKERS, Douglas, Univ. of Pennsylvania, Wharton Sch. of Fin., Dept. of Fin., Philadelphia 4, Pa.
VICKERS, George H., 5823 Conway Rd., Bethesda, Md.
VICKERY, Glenn W., 7203 Ilex, Houston, Tex. 77012
VICKERY, William E., 6025 Kimbark Ave., Apt. 1, Chicago, Ill. 60637
VICKREY, William S., Columbia Univ., Econ. Dept., New York 27, N.Y.
VICTORIANO, Procopio B., T-16 Area 1, Diliman, U. P. Campus, Quezon City, Philippines
VICTORIUS, J. Curt, Guilford Col. Br., 1209 Nathan Hunt Rd., Greensboro, N.C. 27410
VICTORIUS, Marianne Y., 515 Kerry Dr., Grand View Hills, Bloomington, Ind.
VIDAL-SERIN, Antonio R., Nazca 3356, Buenos Aires, Argentina
VIELLE, Jean-Pierre, Rio Nazas, 309, Monterrey (Ciudad), N.L., Mexico
VIETOR, Hendrik W., 79 Wall St., New York 5, N.Y.
VIETORISZ, Thomas, 2 Washington Sq. Village, New York, N.Y. 10012
VIGEN, James W., Univ. of Toledo, Col. of Bus. Admin., Toledo, Ohio 43606

VIGNOLA, Leonard, Jr., Pepsico, Inc., 500 Park Ave., New York, N.Y. 10022
VIKOR, Desider, North Dakota State Univ., Div. of Soc. Sci., P.O. Box 2494, Fargo, N.D.
VIKSNINS, George J., 3731 T. St. N.W., Washington, D.C. 20007
VILLA, Dino S., 5009 Leeds Dr., S.E., Washington, D.C. 20023
VILLAMIL, Jose J., #42 University Mews, 45th and Spruce Sts., Philadelphia, Pa. 19104
VILLANUEVA, Robert S., 3307 Breton Circle, N.E., Atlanta, Ga. 30319
VILLARD, Henry H., 170 E. 93rd St., New York 28, N.Y.
VILLEGAS, Bernardo M., 1024 C. Ayala, Singalong, Manila, Philippines
VINCENT, Howard L., 1425 4th St., S.W., Washington, D.C. 20024
VINCENT, Phillip E., 481 Forest #6, Palo Alto, Calif. 94301
VINCENT, Vern H., West Virginia Univ., Col. of Com., Morgantown, W.Va.
VINCENT, Warren H., Michigan State Univ., Dept. of Agric. Econ., East Lansing, Mich.
VINER, Jacob, 13 Newlin Rd., Princeton, N.J. 08540
VINGE, Clarence L., Michigan State Univ., Dept. of Geography, 315 Nat. Sci. Bldg., East Lansing, Mich. 48823
VINJE, David L., Gardner, N.D. 58036
VINOD, Hrishikesh D., California State Col.-Fullerton, Sch. of Bus. Admin. and Econ., 800 N. State College Blvd., Fullerton, Calif.
VIRATA, Cesar, P.O. Box 589, Manila, Philippines
VISCASILLAS, Felipe S., Apartado 272, Roosevelt, Hato Rey, P.R. 00659
VISER, Festus J., Memphis State Univ., Sch. of Bus. Admin., Memphis, Tenn. 38111
VITALIANO, Donald F., 3225 Olinville Ave., Bronx, N.Y. 10467
VITULE, Affonso A., Av. Paulista 2239, Apt. 132, Sao Paulo, Brazil
VIVAS, Jorge B., Cullen 5584, Buenos Aires, Argentina
VIVO, G. Hugo, Crystal House, Apt. 706, 2000 S. Eads St., Arlington, Va. 22202
VLAHAKIS, Matthew C., 310 W. 106th St., Apt. 8B, New York, N.Y. 10025
VOEGE, Herbert W., Adrian Col., Adrian, Mich. 49221
VOERTMAN, Robert F., Grinnell Col., Grinnell, Iowa 50112
VOGEL, Glen F., 2101 16th St., N.W., Washington, D.C. 20009
VOGEL, (Mrs.) Pearl S., 132-35 Sanford Ave., Flushing 55, N.Y.
VOGEL, Robert C., Wesleyan Univ., Dept. of Econ., Middletown, Conn. 06457
VOGEL, Ronald J., 404 W. Mifflin St., Madison, Wis. 53703
VOGELSON, Jay M., 3343 Southwestern Blvd., Dallas, Tex.
VOGELY, William A., 6504 Pyle Rd., Bethesda, Md. 20034
VOGLER, Richard S., R.R. 4, Carbondale, Ill.
VOGT, Frederick, New England Col., Henniker, N.H. 03242
VOGT, Karl E., Syracuse Univ., Col. of Bus. Admin., 105 Slocum Hall, Syracuse 10, N.Y.
VOGT, Winfried, Projensdorfer St. 210, 23 Kiel-Wik, Germany
VOICH, Dan, Jr., Florida State Univ., Sch. of Bus., Tallahassee, Fla. 32306
VOLKE, Ernest E., 292 Thompson Ave., East Haven, Conn. 06027
VOLKERT, L. John, 527 Center Pl., Teaneck, N.J. 07666
VOLOUDAKIS, Evangelos, Bank of Greece, Econ. Res. Dept., Athens, POB 105, Greece
VOLYN, Robert P., 78 Berkley Ct., Wayne, N.J. 07472
VON BERTRAB-ERDMAN, Hermann R., 2010 Guadalupe St., Austin 12, Tex.
VON BOVENTER, Edwin, Alfred-Weber Inst. K8, Bergheimer Str. 104-106, 69 Heidelberg, Germany
VON CIRIACY-WANTRUP, Siegfried, Univ. of California, 207 Giannini Hall, Berkeley, Calif.
VON DER LINDE, Gert, Stanford Univ., Graduate Sch. of Bus., Stanford, Calif. 96128
VON FEIANDT, Otto, Joel Dean Asso., Inc., 30 Rockefeller Plaza West, New York, N.Y. 10020
VON GERSDORFF, Ralph C. J., SUDENE, Sala 1525, C.P. 960, Recife, P.E., Brazil
VON HOHENBALKEN, Balder, Iowa State Univ., Dept. of Econ. and Soc., Ames, Iowa
VON KUEGELGEN, Werner, Apartado Aereo 4442, Medellin, Colombia
VON PETERFFY, George A., Harvard Bus. Sch., Baker Lib., Rm. 134, Boston 63, Mass.
VON ZEMENSZKY, Carl, 6394 Drexel Rd., Philadelphia 31, Pa.
VOORHIES, Edwin C., Univ. of California, Dept. of Agric. Econ., Davis, Calif. 95616
VORE, Gregory P., 4701 Paseo de las Tortugas, Torrance, Calif. 90505
VORPAHL, Myron G., 111 Storeywood Dr., San Antonio, Tex.
VOSKUIL, Harmon, Bowling Green State Univ., Bowling Green, Ohio 43402
VOSS, Donald H., 8602 Pepperdine Dr., Vienna, Va. 22180
VOSS, Wellington J., 1327 Underwood St., N.W., Washington, D.C. 20012
VOTAW, Gregory B., 6717 Loring Ct., Bradley Park, Bethesda, Md. 20034
VOTEY, Harold L., Jr., 980 Grizzly Peak Blvd., Berkeley 8, Calif.
VOWELS, Robert C., Indiana State Col., Soc. Studies Dept., Indiana, Pa. 15701
VOYATZIS, John G., 1531 Chew St., Allentown, Pa.
VOYNICH, John J., 1102 Glenview Dr., Carbondale, Ill.
VROMAN, H. William, 2334 Grand Ave., Davenport, Iowa 52803
VROOMAN, David, Jr., St. Lawrence Univ., Dept. of Econ., Canton, N.Y. 13617
VUCHICH, Milan I., Wisconsin State Univ., Dept. of Econ., 860 Algoma Blvd., Oshkosh, Wis. 54902
VUGLEN, Stephen M., R.D. 3, Princeton, N.J 08540
VUKASIN, Peter N., Harpur Col., Dept. of Econ., Binghamton, N.Y.
VUKCEVICH, Radoje, Talladega Col., Talladega, Ala. 35160
VULKOVIC, Nicola, Florida State Univ., Dept. of Econ., Tallahassee, Fla. 32306

WACHTEL, Howard M., 619 E. University Ave., Apt. B-2, Ann Arbor, Mich. 48104
WACHTER, Michael L., 141 Oxford St., Cambridge, Mass.
WADBROOK, William P., Ithaca Col., Col. of Arts and Sci., Dept. of Econ., Ithaca, N.Y.
WADE, Carl E., Nevada Southern Univ., 209 Grant Hall, Las Vegas, Nev. 89109
WADLEIGH, Lloyd O., 26642 W. Cloverknoll, Waukesha, Wis.
WADSTED, Otto G., 395 Riverside Dr., Apt. 5B, New York, N.Y. 10025
WADYCKI, Walter J., 4913 W. Barry Ave., Chicago, Ill. 60641
WAEHLER, Robert C., 19 Cunningham Rd., Wellesley Hills, Mass. 02181
WAEHRER, (Mrs.) Helen Y., 320 W. 89th St., Apt. 6-C, New York, N.Y. 10024
WAELTERMANN, John J., 19761 Silver Spring Dr., Southfield, Mich. 48076
WAELTI, John J., Univ. of California, 207 Giannini Hall, Berkeley, Calif. 94720
WAGERS, Robert W., Hq., Dependent Educa. Group, APO New York 09164
WAGNER, Frank, 6418 Main, Kansas City, Mo. 64113
WAGNER, Gerald C., 705 W. Healey St., Champaign, Ill. 61820
WAGNER, Harvey M., Stanford Univ., 544 Alvaredo Row, Stanford, Calif. 94305
WAGNER, Irving, 15-46 212 St., Bayside, N.Y. 11360
WAGNER, Jack L., 253 Woodside Ave., Ridgewood, N.J.
WAGNER, K. Peter, 8601 Milford Ave., Silver Spring, Md.
WAGNER, Lewis E., 405 Ridgeview Ave., Iowa City, Iowa
WAGNER, Martin, 1506 S. Orchard, Urbana, Ill.
WAGNER, Melvin M., Kasetsart Univ., Dept. of Agric. Econ., Bangkok, Thailand

WAGNER, Pinchas Y., 43 York Downs Dr., Downsview, Ont., Canada
WAGNER, Richard, Univ. of Guelph, Econ. Dept., Guelph, Ont., Canada
WAGNER, Richard E., 121 N. Baker St., Charlottesville, Va. 22903
WAGNER, Sheldon R., Roosevelt Univ., 430 S. Michigan, Chicago, Ill. 60605
WAGNER, Thomas R., Grafton, Iowa 50440
WAGNER, Walter C., Raymond Col., Stockton, Calif. 95204
WAGNER, Walter W., Bieley, Wagner, and Associates, Inc., 1515 N.W. 7th St., Miami, Fla. 33125
WAGONER, William D., Louisiana State Univ. in New Orleans, Dept. of Econ., New Orleans 22, La.
WAGSTAFF, George R., 63 Forest Rd., Demarest, N.J. 07627
WAHN, James D., 275 Morrison Ave., Town of Mt. Royal, Montreal 16, Que., Canada
WAI, U. Tun, c/o Int. Monetary Fund, 19th and H St., N.W., Washington 25, D.C.
WAIN, Jack, 116 Hamilton Ave., Silver Spring, Md. 20901
WAINIO, Weijo W., Kirkkokatu 1-B, Helsinki, Finland
WAITS, Caron R., Texas Christian Univ., Dept. of Econ., Ft. Worth, Tex. 76129
WALBERG, Carl A., 1224 N. Humphrey Ave., Oak Park, Ill. 60302
WALD, Haskell P., 9528 Friars Rd., Bethesda, Md. 20034
WALDAUER, Charles, Rutgers Univ., Dept. of Econ., New Brunswick, N.J. 08903
WALDERHAUG, Albert J., 9911 Dameron Dr., Silver Spring, Md.
WALDMAN, William J., 5620 N. 13th St., Philadelphia 41, Pa.
WALDO, Arley D., Univ. of Minnesota, Inst. of Agric.—TSC, Agric. Ext. Serv., St. Paul, Minn. 55101
WALDORF, William H., 2703 24th St., North Arlington, Va.
WALDRUN, John E., Gannon Col., Perry Sq., Erie, Pa. 16501
WALES, H. E., 140 8th Ave., Brooklyn 15, N.Y.
WALES, Hugh G., Univ. of Illinois, Box 60, Commerce (W), Urbana, Ill. 61803
WALGREEN, John A., 6621 Wakefield Dr., Apt. 604, Alexandria, Va.
WALINSKY, Louis J., 3709 Bradley Lane, Chevy Chase, Md. 21620
WALKA, Joseph J., 900 Memorial Dr., Apt. 601 E., Cambridge, Mass. 02138
WALKER, Burley, Jr., 916 W. Inwood, Arlington, Tex. 76010
WALKER, Charls E., Amer. Bankers Asso., 90 Park Ave., New York, N.Y. 10016
WALKER, David A., Iowa State Univ., Dept. of Econ., Ames, Iowa 50010
WALKER, Franklin V., Fed. Res. System, Bd. of Gov., Bus. Conditions Sec., Washington 25, D.C.
WALKER, George R., 93 Monument St., Concord, Mass. 01742
WALKER, Graham H., 5, Highgate High St., Highgate Village N. 6, England
WALKER, Harry S., Texas Tech. Col., Dept. of Econ., Lubbock, Tex.
WALKER, Hugh D., Vanderbilt Univ., Dept. of Econ., Nashville, Tenn.
WALKER, (Mrs.) Irene B., 6903 Carlton Ter., College Park, Md. 20740
WALKER, James M., 232 Hayden Rd., Tallahassee, Fla.
WALKER, James W., Univ. of Iowa, Phillips Hall, Iowa City, Iowa 52240
WALKER, John E., Jr., Investment Bankers Asso., 425 13th St., N.W., Washington, D.C. 20004
WALKER, John M., 2137 Emerson, Apt. 1, Berkeley, Calif. 94705
WALKER, John R., 502 Baylor Rd., Glen Burnie, Md. 21061
WALKER, Kenneth, Albion Col., Albion, Mich. 49224
WALKER, Kenneth E., 80 Melba Dr., North Ryde, N.S.W., Australia
WALKER, Mabel L., 457 Nassau St., Princeton, N.J. 08540
WALKER, (Mrs.) Mary A., 1913 Francisco St., Berkeley 9, Calif.
WALKER, Mark E., 406 Walker Hall, Normal, Ill. 61761
WALKER, Michael A., 40 E. Valley Rd., Corner Brook, N.F., Canada
WALKER, Pinkney C., Univ. of Missouri, 101 BPA, Columbia, Mo.
WALL, David G., Univ. of Sussex, Econ. Fac., Falmer, Brighton, Sussex, England
WALL, Eulalia L., 2500 Que. N.W., Washington, D.C. 20007
WALLACE, Edward L., 110 Dawnbrook Lane, Williamsville, N.Y. 13493
WALLACE, Elbert S., Univ. of Nebraska, 310 Soc. Sci. Bldg., Lincoln, Neb.
WALLACE, Margaret L., Fed. Res. Bank, Kansas City, Mo.
WALLACE, Phyllis A., 1301 Delaware Ave., S.W., Apt. N-422, Washington, D.C. 20024
WALLACE, Raddie M., P.O. Box 7, Bartlesville, Okla. 74004
WALLACE, Richard L., Florida State Univ., Dept. of Econ., Tallahassee, Fla.
WALLACE, Robert F., Montana State Univ., Dept. of Econ., Missoula, Mont. 59801
WALLACE, William H., Duke Univ., Dept. of Econ., Durham, N.C. 27706
WALLACK, Stanley S., Washington Univ., Econ. Dept., MacMillan Hall, St. Louis, Mo.
WALLE, Ronald T., 5500 Church St., Morton Grove, Ill. 60053
WALLENROD, Lionel L., Sears-Roebuck, Dept. 702-R, 2000 Spring Rd., Oak Brook, Ill. 60521
WALLER, James M., Univ. of Georgia, Col. of Bus. Admin., Athens, Ga.
WALLERSTEIN, L. B., 2801 Quebec St., N.W., Apt. 133, Washington, D.C. 20008
WALLICH, Henry C., Yale Univ., Dept. of Econ., New Haven, Conn.
WALLIN, David E., 1009 N. Marion, Oak Park, Ill.
WALLIS, W. A., Univ. of Rochester, Rochester, N.Y. 14627
WALRAVEN, Kornelis J., Univ. of Texas, Dept. of Fin., Austin, Tex.
WALSH, Brendan M., 423 N. Pleasant St., #2-A, Amherst, Mass. 01003
WALSH, Charles J., Fordham Univ., Grad. Sch. of Arts and Sci., New York, N.Y.
WALSH, Paul R., R. #1, Horan Rd., Vestal, N.Y. 13850
WALSH, Richard G., Univ. of Nebraska, Dept. of Agric. Econ., Lincoln, Neb.
WALSH, Robert L., Chrysler Corp., P.O. Box 1919, Detroit 31, Mich.
WALSH, W. D., Univ. of Western Ontario, Univ. Col. of Arts and Sci., Dept. of Econ. and Soc., London, Ont., Canada
WALSH, William J., III, 6½ Seventh St., S.E., Washington, D.C. 20003
WALTER, Edwin J., 480 Park Ave., New York 22, N.Y.
WALTER, Ingo, 3847 Marrietta, Normandy, Mo. 63121
WALTER, James E., Univ. of Pa., Wharton Sch. of Fin. and Comm., Philadelphia 4, Pa.
WALTER, John D., Jr., 12402 Aboite Center Rd., Fort Wayne, Ind.
WALTER, John T., 601 Creek Crossing Rd., N.E., Vienna, Va. 22180
WALTERS, Charles, Jr., 6205 Lane, Raytown, Mo. 64113
WALTERS, Elvin W., 7902 15th Ave., Hyattsville, Md.
WALTERS, Forrest E., Price Res. and Methods Sec., Com. Ana. Br., ERS, USLA, Washington, D.C. 20250
WALTERS, Harry E., 7510 Inzer St., Springfield, Va. 22151
WALTERS, (Mrs.) Joan M. G., 9 Holmes Ct., Darien, Conn. 06823
WALTERS, Robert H., 7724 Canterbury, Prairie Village, Kans.
WALTERSDORF, Maurice C., 1312 Oak Hill Ave., Hagerstown, Md. 21740
WALTHER, Jaan, 5004 Westbriar Lane, Apt. 14, Houston, Tex.
WALTHER, James V., Univ. of Denver, Col. of Bus. Admin., 1445 Cleveland Pl., Denver 2, Colo.

WALTHER, Ted, 148 Wood St., Lewiston, Me.
WALTON, Clarence, Columbia Univ., Grad. Sch. of Bus., New York, N.Y.
WALTON, Gary M., 151 Lagomarsino, Sacramento 19, Calif.
WALTON, Richard N., Morris St., Oxford, Md. 21654
WALTZ, Thomas W., 1633 Ridge Ave., Evanston, Ill. 60201
WALZER, Norman C., R. #2, Mendota, Ill. 61342
WAMBACH, Robert F., R. 3, North Central Sch., Grand Rapids, Minn. 55744
WANDNER, Stephen A., 709 Campus View House, Bloomington, Ind. 47403
WANG, C. W., 1004 Robroy Dr., Silver Springs, Md.
WANG, Ching F., 110 College St., Angola, Ind. 46703
WANG, George C., Univ. of Tennessee, Dept. of Bus. Admin., Martin, Tenn. 38237
WANG, Hsin F., 424 E. South Ave., Greenville, Ill. 62246
WANG, Kung-Lee, 8 Schindler Ct., Silver Spring, Md. 20903
WANG, Nian-Tzu, 25 Lincoln St., Larchmont, N.Y. 10538
WANG, Sheng-Wu, King's Col., Dept. of Econ., 133 N. River St., Wilkes-Barre, Pa. 18702
WANG, Tong-Eng, Iowa State Univ., Dept. of Econ., Ames, Iowa
WANG, Tso-Yung, c/o Hsin-Lsiang Fang, Supreme Ct., S. Chungking Rd., Taipei, Taiwan, Formosa
WANT, Robert S., 311 N. Spain St., Darlington, S.C. 29532
WAPLES, Eliot O., Midland Mortgage Co., 312 3rd St., S.E., Cedar Rapids, Iowa 50613
WARBURTON, Clark A., 1031 Pine Hill, McLean, Va. 22101
WARD, Benjamin, Univ. of California, Dept. of Econ., Berkeley 4, Calif.
WARD, F. B., 4041 N. Downer Ave., Shorewood, Wis. 53211
WARD, F.L., 630 Williams Crescent, Regina, Sask., Canada
WARD, Frank B., 702 Forest Hills Blvd., Knoxville, Tenn. 37919
WARD, Richard A., Univ. of Southern Calif., Dept. of Fin., Los Angeles, Calif. 90007
WARD, Richard C., 665:A Infantry Post Rd., Ft. Sam Houston, Tex. 78234
WARD, Richard J., 10309 Garden Way, Potomac, Md.
WARDEN, Charles B., Jr., 5626 Sylvan Ave., Bronx, N.Y. 10471
WARDEN, (Mrs.) Katherine D., 5626 Sylvan Ave., Bronx, N.Y. 10471
WARE, Peggy M., 4732 Southmoor Rd., Richmond, Va. 23234
WARE, Ray M., Transylvania Col., Dept. of Econ., Psych. and Soc., Lexington, Ky.
WARING, (Mrs.) Avis P., 1628 Westmorland St., McLean, Va.
WARING, Ronald F., 1628 Westmorland St., McLean, Va.
WARKE, Thomas W., 525 Major Rd., Monmouth Junction, N.J. 08852
WARMBRUNN, Hans, 110-50 68th Dr., Forest Hills, N.Y. 12068
WARMKE, Roman F., Univ. of Minnesota, 1171 SBA, West Bank, Minneapolis, Minn. 55455
WARNE, Clinton, 617 Foster St., Evanston, Ill. 60201
WARNE, William E., 2090 8th Ave., Sacramento 18, Calif.
WARNER, Aaron W., 498 Engle St., Englewood, N.J.
WARNER, Adolphe J., 955 Park Ave., New York 28, N.Y.
WARNER, Harold D., Jr., First Fed. Savings and Loan Asso., Broughton and Abercorn Sts., Savannah, Ga.
WARNER, Larkin B., Oklahoma State Univ., Dept. of Econ., Stillwater, Okla.
WARNER, P. D., III, 1106-J University Village, East Lansing, Mich. 48823
WARNER, Philip J., 15 E. 26th St., New York 10, N.Y.
WARNER, Stanley L., 868 Guanajuato Dr., Claremont, Calif.
WARNER, William D., 1220 S. Monroe, San Angelo, Tex. 76901
WARNING, Donald S., 633 East Ave., Park Ridge, Ill. 60068
WARNKEN, Philip F., Univ. of Missouri, Dept. of Agri. Econ., 200 Mumford Hall, Columbia, Mo. 65201
WARRACK, Allan A., 1420 Hawthorn, Ames, Iowa 50012
WARREN, Forest G., 216 Lawton St., Falls Church, Va. 22046
WARREN, Gerald E., De Pauw Univ., Dept. of Econ., Greencastle, Ind. 46135
WARREN, Howard E., Glenville State Col., Box 25, Glenville, W.Va. 26351
WARREN, Joyce K., 405 E. 56th St., New York, N.Y. 10022
WARREN, Samuel E., Jackson State Col., Dept. of Econ., Geography and Bus., Jackson, Miss. 39217
WARREN, Stanton A., 33 Linview Ter., Buffalo, N.Y. 14216
WARREN, William G., 8508 Remick Ave., Sun Valley, Calif. 91352
WARRINGTON, William E., 514 Anthwyn Rd., Merion Sta., Pa. 19066
WARSHAW, Martin R., 2045 Norfolk, Ann Arbor, Mich.
WASEY, Abdul, Jada Mewend Peshminabafi Kandhar, Kabu, Afghanistan
WASHBURN, Horace H., Univ. of Alabama, Sch. of Com. and Bus. Admin., University, Ala. 35486
WASON, James R., 2423 Foster Pl., S.E., Washington, D.C. 20031
WASOWSKI, Stanislaw, 1603 Holly Ct., McLean, Va. 22101
WASS, Herbert F., 3 Woodside Dr., South Burlington, Vt. 05403
WASSEN, Dimiter E., 1047 Peach Tree Lane, Deerfield, Ill. 60015
WASSERMAN, Max J., 1104 Fontaine Rd., Lexington, Ky.
WASSERMAN, William, Syracuse Univ., Col. of Bus. Admin., 116 College Pl., Syracuse, N.Y. 13210
WASSINK, Darwin, Barnes 7C, Escondido Village, Stanford, Calif. 94305
WASSON, Chester R., 2129½ S. Bentley, West Los Angeles, Calif. 90025
WASSON, (Mrs.) Hilda C., University Sta., Box 8864, Knoxville, Tenn. 37916
WASSON, Robert C., 7937 Bolling Dr., Tauxemont, Alexandria, Va. 22308
WATANABE, Taro, 329 Hatamachi, Ikeda, Osaka, Japan
WATERMAN, George H., III, Nickerson and Waterman Corp., 55 W. 42nd St., New York, N.Y. 10036
WATERS, Alan R., 7614 Troulon, Houston 36, Tex.
WATERS, Edward A., 1013 Loxford Ter., Silver Spring, Md. 20901
WATERS, Lawrence L., Indiana Univ., Psychology 349, Bloomington, Ind. 47405
WATERS, Robert C., 462 Arbramar Ave., Pacific Palisades, Calif. 90272
WATERS, William R., 499 Scarborough Rd., Toronto 13, Ont., Canada
WATERS, William R., 2222 Dayton St., Chicago, Ill. 60614
WATERSTON, Albert, 7117 Braeburn Pl., Bethesda, Md. 21811
WATKINS, Cameron D., Town Center Plaza, Apt. N611W, 1000 6th St., N.W., Washington, D.C. 20024
WATKINS, Dean A., Univ. of Maryland, Dept. of Econ., College Park, Md.
WATKINS, Gordon S., 2663 Tallant Rd., Santa Barbara, Calif. 93105
WATKINS, John B., 3245 Grant St., Evanston, Ill. 60201
WATKINS, Kermit, 424 E. Mississippi, Liberty, Mo. 64068
WATKINS, Melville H., Univ. of Toronto, Dept. of Polit. Econ., Toronto 5, Ont., Canada
WATKINS, Ralph J., 2908 R St., N.W., Washington 7, D.C.
WATKINS, Wayne S., R. #1, Box 87C, Ijamsville, Md. 21754
WATNIK, Morton M., 16 Cross Gates Rd., Madison, N.J. 07940
WATRIN, Christian, Moltkestrasse 4, Rodenkirchen Bei Cologne, Western Germany

WATRY, Peter J., Jr., 81 Second Ave., Chula Vista, Calif. 92010
WATSON, Clifford, 475 Ridgelawn Ave., Hamilton, Ohio
WATSON, Donald A., 1285 E. 20th St., Eugene, Ore.
WATSON, Donald J., c/o General Electric Co., Crotonville, Box 151, Ossining, N.Y.
WATSON, Donald S., George Washington Univ., Washington, D.C. 20006
WATSON, Dorothy E., Wells Col., Aurora, N.Y. 13026
WATSON, Edward T., Northwestern Univ., Sch. of Com., Evanston, Ill.
WATSON, Harold F., 4903 Newcome Dr., San Antonio, Tex. 78229
WATSON, Jesse P., 615 Center Ave., Pittsburgh, Pa. 15215
WATSON, Merrill A., 14 Westminster Rd., Summit, N.J. 07901
WATSON, Robert S., Brentwood Apts., Apt. B-7, Braemar, Olivet St., Halifax, N.S., Canada
WATSON, William R., 240 Vaquero Rd., Arcadia, Calif. 91007
WATTEL, Harold L., 181 Shepherd Lane, Roslyn Heights, N.Y. 11577
WATTERS, Elsie M., Tax Found. Inc., 50 Rockefeller Plaza, New York 20, N.Y.
WATTERS, Francis M., 611 Santa Fe, Albany, Calif. 94507
WATTLES, George M., Rockford Col., Rockford, Ill. 61101
WATTLES, Marshall D., Univ. of Oregon, Dept. of Econ., Eugene, Ore.
WATTS, Lawrence W., 9 Warner Rd., Marion, N.Y. 14505
WATTS, V. O., Northwood Inst., Midland, Mich. 48641
WAUD, Roger N., Univ. of Chicago, Grad. Sch. of Bus., 5836 Greenwood Ave., Chicago, Ill. 60637
WAUGH, Frederick V., 1006 S. 26th St., Arlington, Va. 22202
WAWRZYNIAK, Florian, 45 Ashford St., Allston, Mass. 02134
WAY, John A., 219 Empress Dr., Houston, Tex. 77034
WEATHERBY, James L., Jr., Box 425, College Station, Tex. 77840
WEATHERBY, James S., Atlantic Refining Co., 260 S. Broad St., Philadelphia, Pa. 19101
WEATHERFORD, Willis D., Carleton Col., Northfield, Minn. 55057
WEATHERS, Everett A., One Vincent Rd., Apt. 6-F, Bronxville, N.Y.
WEATHERS, Milledge W., Leopoldstrasse 135/1, Munich, Germany
WEAVER, Alex R. H., 1522 Walnut St., Berkeley, Calif. 94709
WEAVER, Alfred J. S., 3306 Alabama Ave., Alexandria, Va.
WEAVER, Betty M., Ohio Univ., Col. of Bus. Admin., Athens, Ohio 45701
WEAVER, James H., American Univ., Dept. of Econ., Washington 16, D.C.
WEBB, Donald Q., Trinity Univ., Dept. of Econ., San Antonio 12, Tex.
WEBB, John N., Florida Atlantic Univ., Col. of Soc. Sci., Boca Raton, Fla. 33432
WEBB, (Mrs.) Laura M., 3614 Warren St., N.W., Washington 8, D.C.
WEBB, Samuel C., 1508 W. 21st. Ter., Lawrence, Kans.
WEBB, Wallace G., Queens Col., Econ. Dept., Charlotte, N.C. 28207
WEBB, William Y., 3614 Warren St., N.W., Washington 8, D.C.
WEBBER, Edward G., Central Michigan Univ., Dept. of Econ., Mt. Pleasant, Mich. 48858
WEBBINK, Douglas W., Duke Univ., Dept. of Econ., Durham, N.C. 27706
WEBBINK, (Mrs.) Gladys F., 17 Cohawney Rd., Scarsdale, N.Y. 10583
WEBBINK, Paul, 17 Cohawney Rd., Scarsdale, N.Y. 10583
WEBER, Adna F., 8521 114th St., Richmond Hill, L.I., N.Y. 13144
WEBER, (Mrs.) Alma B., 13 Meador Dr., Tuscaloosa, Ala. 35401
WEBER, Arnold R., Univ. of Chicago, Grad. Sch. of Bus., Chicago, Ill. 60637
WEBER, C. E., Univ. of Pittsburgh, Admin. Sci. Center, Pittsburgh, Pa. 15213
WEBER, Carol L., 300 N. Buchanan, Olathe, Kans.
WEBER, Gerald I., Univ. of Illinois, Dept. of Econ., Urbana, Ill. 61803
WEBER, Paul J., c/o Hercules Powder Co., Delaware Trust Bldg., Wilmington, Del.
WEBER, Willett F., Shoreline R., Polson, Mont. 59860
WEBER, William H., III, 604 14th St., N.W., Charlottesville, Va. 22903
WEBERG, Frank, Wilcox and Taylor Sts., Joliet, Ill.
WEBSTER, Arthur G., 913 Birch Ave., Seaside, Calif. 93955
WEBSTER, Donald A., 3819 S St., N.W., Washington 7, D.C.
WEBSTER, Henry H., Univ. of Wisconsin, Dept. of Forestry, Madison, Wis. 53706
WEBSTER, Philip M., Fed. Loan Bank—Greensboro, 617 W. Market St., Greensboro, N.C. 27402
WECKSTEIN, Richard S., Stonehedge, Lincoln, Mass. 01773
WEDDEL, Bernard L., San Jose State Col., Div. of Bus., San Jose 14, Calif.
WEDDELL, R. P., 3801 Marquette Pl. #2G, San Diego, Calif. 92106
WEED, John B., 192 Nassau St., Princeton, N.J. 08540
WEEKLY, James K., Macalester Col., Dept. of Econ., St. Paul 1, Minn.
WEEKS, Dale H., Northern Illinois Univ., De Kalb, Ill. 60115
WEEKS, Eldon E., Washington State Univ., Dept. of Agric. Econ., Pullman, Wash.
WEEKS, John F., Univ. of Michigan, Dept. of Econ., Ann Arbor, Mich.
WEEKS, William J., Univ. of Kansas, 210 Summerfield, Lawrence, Kans.
WEEMS, Robert C., Jr., Univ. of Nevada, Col. of Bus. Admin., Reno, Nev.
WEGGELAND, Robert L., 85 Hillside Ave., Chatham, N.J. 07920
WEHNER, Harrison, Int. Monetary Fund, African Dept., Washington, D.C. 20431
WEHRLE, Leroy S., 515 Carlyle Ave., Belleville, Ill. 62221
WEHRWEIN, Carl F., 4250 N. 25th St., Arlington, Va. 22207
WEICHER, John C., Univ. of California-Irvine, Dept. of Econ., Div. of Soc. Sci., Irvine, Calif. 92650
WEIDENAAR, Dennis J., 240 S. Salisbury, Apt. 13, West Lafayette, Ind. 47906
WEIDENBAUM, Murray L., Washington Univ., Dept. of Econ., St. Louis, Mo.
WEIFENBACH, Annette, 2218 Parnell Ave., Los Angeles, Calif. 90064
WEIGAND, Robert E., De Paul Univ., Col. of Com., 25 E. Jackson Blvd., Chicago, Ill. 60604
WEIL, Leonard, c/o Manufacturers Bank, P.O. Box 6000, Los Angeles 55, Calif.
WEIL, R. L., 6011 Ingleside St., 1-E, Chicago, Ill. 60637
WEIL, (Mrs.) Roberta M., 203 Ingraham Blvd., Hempstead, L.I., N.Y.
WEIL, Rolf A., 430 S. Michigan Ave., Chicago, Ill. 60605
WEILER, Berthold, 250 W. 85th St., New York, N.Y. 10024
WEILER, Emanuel T., Purdue Univ., Herman C. Krannert Grad. Sch. of Ind. Admin., Lafayette, Ind. 47907
WEILER, Valerie D., 250 W. 85th St., New York, N.Y. 10024
WEIMER, A. M., Indiana Univ., Bloomington, Ind.
WEIN, Harold H., Michigan State Univ., Col. of Bus., Dept. of Mgt., East Lansing, Mich.
WEINBERG, Edgar, 9302 Ewing Dr., Bethesda, Md. 20014
WEINBERG, Hyman, SCM Corp., 410 Park Ave., New York, N.Y. 10022
WEINBERG, (Mrs.) Lois N., 79 Beechdale Rd., Dobbs Ferry, N.Y. 10522
WEINBERG, Nat, UAW Special Projects Dept., 8000 E. Jefferson, Detroit 14, Mich.
WEINBERG, Robert S., 79 Beechdale Rd., Dobbs Ferry, N.Y. 10522

WEINER, Clarence M., 350 Fifth Ave., Rm. 2214, New York 1, N.Y.
WEINER, George T., Lake Forest Col., Dept. of Econ., Lake Forest, Ill. 60045
WEINER, K. R., 10751 Rose Ave., Los Angeles, Calif.
WEINER, Louis, 7206 Adelphi Rd., Hyattsville, Md.
WEINER, Mervyn L., 3206 Cummings Lane, Chevy Chase, Md. 20015
WEINER, Sam, 3881 Duncan Pl., Palo Alto, Calif. 94303
WEINER, William, 604 W. Arlington Pl., Chicago, Ill. 60614
WEINFELD, William, 7225 Beechwood Rd., Hollin Hills, Alexandria, Va. 22307
WEINGARTNER, H. M., Massachusetts Inst. of Tech., Sloan Sch. of Mgt., Cambridge, Mass. 02139
WEINRICH, John E., Univ. of Alberta, Dept. of Pol. Econ., Calgary, Alta., Canada
WEINROBE, Maurice D., 1 Delcrest Ct., University City, Mo. 63565
WEINSHANK, Myrna G., 4201 Cathedral Ave., N.W., Washington, D.C. 20016
WEINSTEIN, Joel R., 1550 E. 13th St., Brooklyn 30, N.Y.
WEINSTEIN, Melvin, 2127 California St., N.W., Washington, D.C. 20008
WEINSTEIN, Paul A., Univ. of Maryland, Dept. of Econ., College Park, Md.
WEINSTOCK, Ulrich, 4 Clos de l'Equateur, Trevuren/Brabant, Belgium
WEINTRAUB, Andrew R., 18 Franklin Ct., Somerset, N.J.
WEINTRAUB, David, No. 9 St. Ann's Gardens, St. Ann's, Port of Spain, Trinidad, West Indies
WEINTRAUB, Sidney, U.S. Embassy, Santiago, Chile
WEINTRAUB, Sidney, Univ. of Pennsylvania, Dept. of Econ., Philadelphia, Pa.
WEINTRAUB, Walter, Kingsborough Comm. Col. of City Univ. of New York, Econ. Dept., Oriental Blvd., Manhattan Beach, Brooklyn, N.Y. 11235
WEINWURM, Ernest H., De Paul Univ., Col. of Com., 25 E. Jackson Blvd., Chicago, Ill. 60604
WEIR, John A., 95-L Milford St., Waterloo, Ont., Canada
WEIR, (Mrs.) Kristina H., 810 E. Gordon Pl., Effingham, Ill. 62401
WEIR, Norman E., Jacksonville Univ., Jacksonville, Fla.
WEIR, Thomas O., Box 126, Hiram, Ohio 44234
WEISBROD, Burton A., Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706
WEISENFELD, Albert H., 127 Overlook Ter., Roslyn Heights, L.I., N.Y. 11577
WEISKOPF, William J., 498 Miller Ave., Kent, Ohio 44240
WEISS, Armand B., Center for Naval Anal., 1401 Wilson Blvd., Arlington, Va. 22209
WEISS, Benjamin H., N.Y. State Dept. of Con., 230 Park Ave., New York 17, N.Y.
WEISS, Francis J., 3111 N. 20th St., Arlington, Va. 22201
WEISS, Fred G., 5316 S. Dorchester Ave., Chicago, Ill. 60615
WEISS, George, Bache and Co., 36 Wall St., New York, N.Y.
WEISS, Gus W., Jr., 141 E. 89th St., New York, N.Y.
WEISS, Jeffrey H., 28 Whitman Rd., Apt. 1-4, Waltham, Mass. 02081
WEISS, Joseph S., 320-21 Diamond Village, Gainesville, Fla. 32601
WEISS, Leonard, Amer. Embassy, APO New York 09675
WEISS, Leonard W., Univ. of Wisconsin, Econ. Dept., Madison 6, Wis.
WEISS, Roger W., 1415 E. 54th St., Chicago, Ill. 60615
WEISS, (Mrs.) Shirley F., P.O. Box 1368, Chapel Hill, N.C.
WEISS, Stephen N., Box 239, Harpur Col., State Univ. of New York, Binghamton, N.Y. 13901
WEISS, Steven J., 77 Sacramento St., Somerville, Mass. 02143
WEISS, Steven J., 1555 Filbert Ct., Denver, Colo.
WEISS, Yoram, 8-C Escondido Rd., Stanford, Calif.
WEISSER, Mendel, Memorial Univ. of Newfoundland, Dept. of Econ., St. John's, N.F., Canada
WEISSKOPF, Walter A., 5555 S. Blackstone Ave., Chicago, Ill. 60637
WEISSMAN, Jacob, Hofstra Univ., Dept. of Econ., Hempstead, N.Y.
WEISSMAN, Kenneth P., 142-05 Roosevelt Ave., Apt. 718, Flushing, N.Y.
WEISSMAN, Rudolph L., 2111 Albemarle Rd., Brooklyn 26, N.Y.
WEISZ, Morris, Amer. Embassy, APO New York 09675
WEITZMAN, Phillip, 317 Braun Ct., Ann Arbor, Mich. 48104
WEITZMANN, Albert M., 2405 W. Chester Pike, Broomall, Pa. 19008
WELCH, David A., 1220 Washington Ave., Golden, Colo. 80401
WELCH, Emmett H., Bur. of Statis., Dept. of Int. Affairs, Harrisburg, Pa.
WELCH, Finis R., Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
WELCH, George D., Univ. of Miami, Sch. of Bus. Adm., Dept. of Acctg., Coral Gables, Fla. 33926
WELCH, Richard O., 70 Lakeshire Rd., Rochester, N.Y. 14612
WELCH, Stephen W., 29 Armanino Ct., Oakland 18, Calif.
WELD, Edric A., Jr., 2195 N. St. James Pkwy., Cleveland Heights 6, Ohio
WELDEN, William C., H. P. Hood and Sons, 500 Rutherford Ave., Boston 29, Mass.
WELFLING, Weldon W., 3701 Sutherland Rd., Shaker Heights, Ohio 44122
WELK, William G., 1734 P St., N.W., Washington 6, D.C.
WELLER, LeRoy A., P.O. Box 968, Whittier, Calif. 90608
WELLER, Lorington O., 3 Granger Pl., Apt. 4, Rochester, N.Y. 14607
WELLES, John G., 3602 S. Gilpin St., Englewood, Colo. 80110
WELLFORD, Dabney S., Nat. Cotton Coun. of Amer., Box 12285, Memphis, Tenn. 38112
WELLINGTON, Donald C., 69 10th St., Roxboro, Que., Canada
WELLISZ, Stanislaw H., Columbia Univ., Dept of Econ., New York 25, N.Y.
WELLMAN, Charles A., First Charter Fin Corp., 9465 Wilshire Blvd., Beverly Hills, Calif.
WELLS, Arthur S., Jr., 3 Candlewood Circle, Pittsford, N.Y. 14534
WELLS, Donald A., Southern Illinois Univ., Dept. of Econ., Carbondale, Ill.
WELLS, Donald R., Box 201, University, Miss. 38677
WELLS, Frederick J., 23 Mangano Ct., Watertown, Mass. 02172
WELLS, Garland R., Univ. of Tennessee, Dept. of Forestry, Col. of Agric., Knoxville, Tenn 37901
WELLS, Herman B., Indiana Univ., Bloomington, Ind.
WELLS, Jerome C., Univ. of Ibadan, c/o N.I.S.E.R., Ibadan, Nigeria
WELLS, John D., Inst. for Defense Anal., 400 Army-Navy Dr., Arlington, Va. 22202
WELLS, Louis T., Jr., 207 Adams St., Waltham, Mass. 02081
WELSCH, Delano E., Texas A and M Univ., Dept. of Agric. Econ. and Soc., College Station, Tex. 77843
WELSCH, Donald R., 500 S. Center St., Orange, N.J. 07050
WELSH, Arthur L., State Univ. of Iowa, Bur. of Bus. and Econ. Res., Iowa City, Iowa
WELSH, Charles A., Rollins Col., Dept. of Econ. and Bus. Admin., Winter Park, Fla.
WELSH, James G., 4320 Old Dominion Dr., Arlington, Va. 22207
WELSH, Robert S., Univ. of Kentucky, Agric. Econ. Dept., Lexington, Ky. 40506
WELSHANS, Merle, Washington Univ., Sch. of Bus. and Pub. Admin., St. Louis 5, Mo.
WELSHONCE, James R., Davis and Elkins Col., Dept. of Econ. and Bus., Elkins, W.Va. 26241

WEND, Jared S., Western Michigan Univ., Kalamazoo, Mich. 49007
WENDEL, Helmut F., 11103 Mitscher St., Kensington, Md. 20795
WENDERS, John T., Middlebury Col., Dept. of Econ., Middlebury, Vt. 05753
WENDROW, Philip, Port of N.Y. Authority, 111 Eighth Ave., Dept. of Real Estate, Rm. 715, New York, N.Y. 10011
WENDT, Erwin W., 5112 Duvall Dr., Washington, D.C. 20016
WENDT, Paul F., Univ. of California, Sch. of Bus. Admin., Berkeley, Calif.
WENDZEL, Julius T., 8364 Gull Rd., R. 1, Box 401, Richland, Mich. 49083
WENNBERG, Samuel G., 17 W. Parkway Dr., Columbia, Mo. 65201
WENNIGES, Wiltrud H., 701 Delaware Ave., S.W., Washington, D.C. 20024
WENRICK, Ernest D., 19 Coleman Pl., Apt. 29, Menlo Park, Calif. 94025
WENRICH, Nathaniel, 24 Brookdale Rd., Cranford, N.J. 07016
WENTRUP, Hans J., 146 Beaver St., Manchester, N.H.
WENTWORTH, Gerald, 320 Solana Dr., Los Altos, Calif.
WENTWORTH, Jack R., 3501 Homestead Dr., Bloomington, Ind. 47403
WENTZELL, Homer P., 45 Eagle St., Springfield, Mass. 01107
WENZLAU, Thomas E., Lawrence Col., Appleton, Wis. 54192
WENZLICK, Roy, Roy Wenzlick and Co., 706 Chestnut St., St. Louis 1, Mo.
WERNER, Herbert D., Lewis and Clark Col., 615 S.W. Palatine Hill Rd., Portland 19, Ore.
WERNER, Ray O., Colorado Col., Dept. of Econ., Colorado Springs, Colo.
WERNER, Robert, Wilkes Col., Dept. of Econ., Wilkes-Barre, Pa.
WERNETTE, J. P., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
WERT, James E., Univ. of Buffalo, Sch. of Bus. Admin., Buffalo 14, N.Y.
WERTHEIMER, Robert G., 26 Gray Gardens, Cambridge, Mass. 01922
WERTIMER, Sidney, Jr., College Hill, Clinton, N.Y. 13323
WERTZ, John C., 2950 Yeager Rd., West Lafayette, Ind.
WESOLOWSKI, Karl A., 46 Howe St., Ipswich, Mass. 01938
WESOLOWSKI, Zdzislaw P., 8 Elm St., Cobleskill, N.Y. 12043
WESSEL, Robert H., Univ. of Cincinnati, Dept. of Econ., Cincinnati, Ohio 45221
WESSON, William H., Jr., Univ. of South Carolina, Sch. of Bus. Admin., Columbia, S.C.
WEST, Donald A., North Carolina State Univ., P.O. Box 5368, Raleigh, N.C. 27607
WEST, (Mrs.) Eleanora de-A, Tufts Univ., Fletcher Sch., Medford, Mass. 02155
WEST, Robert J., 6829 Wellauer Dr., Milwaukee, Wis. 53213
WEST, Robert L., Tufts Univ., Fletcher Sch., Medford, Mass. 02155
WEST, Sidney D., Jr., P.O. Box 422, Lake Butler, Fla. 32054
WEST, Stan, N.Y. Stock Exchange, Dept. of Res. and Statis., 11 Wall St., New York, N.Y. 10005
WESTBROOK, Luther J., Hq. PACAF (CPTF), U.S.A.F., APO San Francisco 96553
WESTBROOK, William S., 14 Samson Pl., Branville, Ohio 43023
WESTCOTT, George W., 1100 Sixth St., S.W., Apt. 515, Washington, D.C. 20024
WESTCOTT, Robert F., Spencer Stuart and Asso., Inc., 38 S. Dearborn St., Chicago, Ill. 60603
WESTERFIELD, Donald L., 201-A Garfield Ave., Collingswood, N.J. 08108
WESTERLUND, Barton A., Indus. Res. and Extension Center, P.O. Box 3017, Little Rock, Ark.
WESTFALL, Ralph, Northwestern Univ., Sch. of Bus., Evanston, Ill.
WESTFIELD, Fred M., Vanderbilt Univ., Box 1681, Nashville, Tenn. 37203
WESTHAGEN, Milton H., Ripon Col., Dept. of Econ., Ripon, Wis. 54971
WESTHOFF, William, 1143 Devonshire Rd., Grosse Pointe, Mich. 48138
WESTING, J. H., Univ. of Wisconsin, Sch. of Com., Madison 16, Wis.
WESTON, Gerald, Univ. of Wisconsin-Milwaukee, Milwaukee, Wis. 53211
WESTON, J. F., Univ. of California, Col. of Bus. Admin., Los Angeles 24, Calif.
WESTON, Janet L., 508 W. Park Ave., Champaign, Ill.
WETMORE, John M., Fed. Home Loan Bank, 104 S. Michigan Ave., Chicago, Ill. 60603
WETTERGREEN, Conrad J., Hollow Rd., Salt Point, N.Y. 12578
WETZEL, James R., 103 G St., S.W., #B-401, Washington, D.C. 20024
WEXLER, Imanuel, Univ. of Connecticut, Dept. of Econs., Box U-63, Storrs, Conn. 06268
WEYFORTH, William O., 104 Thicket Rd., Baltimore, Md. 21212
WEYMAR, F. Helmut, 107 Mercer St., Princeton, N.J. 08540
WHALEN, Edward L., Indiana Univ., Dept. of Econ., Bloomington, Ind.
WHALEN, Thaddeus J., Jr., Univ. of Santa Clara, Sch. of Bus., Santa Clara, Calif.
WHALEY, R. S., Utah State Univ., Col. of Forest, Range and Wildlife Mgt., Logan, Utah 84321
WHANG, Kyu B., 5226 E. 98th St., Cleveland, Ohio 44125
WHARTON, Clifton R., Jr., Agric. Dev. Coun., 630 Fifth Ave., Rm. 3155, New York 20, N.Y.
WHATLEY, Hugh C., CMR Box 4692, Webb AFB, Tex. 79721
WHEATLEY, John J., Univ. of Washington, Col. of Bus. Admin., Seattle, Wash. 98105
WHEATON, Donald J., Northeastern Univ., Col. of Bus. Admin., Dept. of Econ., Boston, Mass.
WHEELER, Bayard O., Univ. of Washington, Col. of Bus. Admin., Seattle 5, Wash.
WHEELER, Harvey J., Clemson Col., Dept. of Soc. Sci., Clemson, S.C.
WHEELER, Oliver P., 27650 Central Dr., Los Altos Hills, Calif. 94022
WHEELER, Robert D., Long Beach State Col., Dept. of Econ., Long Beach, Calif. 90804
WHELAN, Carl J., 3600 S St., N.W., Washington, D.C.
WHIPPLE, Clayton E., Amer. Embassy, Agri. Attache, Brussels, Belgium
WHIPPLE, William, Jr., 395 Mercer Rd., Princeton, N.J.
WHISTON, William B., 55 Brewster St., Cambridge, Mass. 02138
WHITAKER, Benjamin P., 1625 Wendell Ave., Schenectady, N.Y. 12308
WHITAKER, Gilbert R., Northwestern Univ., Sch. of Bus, Dept. of Bus. Econ., Evanston, Ill. 60201
WHITAKER, John K., Univ. of Bristol, Dept. of Econ., 40 Berkeley Sq., Bristol 8, England
WHITBECK, Volkert S., Bank of N.Y., 48 Wall St., New York 15, N.Y.
WHITCOMB, Hale C., 400 N. River Rd., Apt. 102, West Lafayette, Ind. 47906
WHITE, Alfred D., 3769 Oliver St., N.W., Washington, D.C. 20015
WHITE, Alvin C., 1078 N. Broad St., Galesburg, Ill. 61401
WHITE, Byron, New Haven Col., P.O. Box 1306, New Haven, Conn. 06505
WHITE, C. M., 1803 Virginia Rd., San Marino, Calif. 91108
WHITE, Carroll S., 521 W. Venice Ave., Venice, Fla. 33595
WHITE, Charles P., Univ. of Tennessee, Sch. of Com., Knoxville, Tenn.
WHITE, David E., West Virginia Univ., Div. of Forestry, Morgantown, W.Va.
WHITE, Donald J., 25 Pilgrim Rd., Milton, Mass. 02186
WHITE, Elizabeth D., USDA, ERS, Market Potentials Br., Washington, D.C. 20250
WHITE, Emmett L., 2993 Dale Dr., N.E., Atlanta 5, Ga. 30305
WHITE, Evan D., G-1 Section, Headquarters VII Corps, APO New York 09107
WHITE, John P., 230 Miles Ave., Syracuse, N.Y. 13210

WHITE, Lawrence J., Harvard Univ., Adams G-12, Cambridge 38, Mass.
WHITE, Leonard, Jr., Univ. of Arkansas, Sch. of Bus., Fayetteville, Ark. 72701
WHITE, Mary L., 742 W. Pensacola St., Tallahassee, Fla. 32304
WHITE, Melvin I., 5405 Newington Rd., Washington, D.C. 20016
WHITE, Roger S., 1078 N. Broad St., Galesburg, Ill. 61401
WHITE, Thomas E., 110 Periwinkle Ct., Greenbelt, Md. 20770
WHITE, Walter E., 1833 Riverside Dr., Apt. 1102, Ottawa 8, Ont., Canada
WHITE, Wilford L., 3131 Nebraska Ave., N.W., Washington, D.C.
WHITE, William H., Int. Monetary Fund, 19th and H St., N.W., Washington, D.C. 20431
WHITE, William L., Mass. Inst. of Tech., Sch. of Ind. Mgt., 52-202, Cambridge, Mass. 02139
WHITE, William T., U.S. Embassy, Belgrade, APO 09696, New York, N.Y.
WHITEHEAD, B. J., 5827 Elwood St., Pittsburgh 32, Pa.
WHITEHURST, Clinton H., Clemson Col., Dept. of Ind. Mgt., Clemson, S.C.
WHITEHURST, Gentry F., 25001 138th Pl., S.E., Kent, Wash. 98031
WHITELEY, Virgil E., 3427 Cork Oak Way, Palo Alto, Calif. 94303
WHITESEL, Theodore L., Arlington State Col., Dept. of Bus. and Econ., Arlington, Tex.
WHITESELL, William E., 739 College Ave., Lancaster, Pa. 17603
WHITIN, Thomson M., Wesleyan Univ., Econ. Dept., Middletown, Conn.
WHITING, Albert W., 5049 S. 22nd St., Arlington, Va. 22206
WHITLEY, Irvin L., 1036 Belair Dr., Rantoul, Ill.
WHITMAN, Marcus L., Univ. of Alabama, Sch. of Com. and Bus., Box 1985, University, Ala. 35486
WHITMAN, (Mrs.) Marina V., 5308 Ellsworth Ave., Pittsburgh, Pa.
WHITMAN, Ray D., 40 W. 77th St., New York, N.Y. 10024
WHITMAN, Robert L., 28 Greenwood Ave., Indian Head, Md. 20640
WHITMAN, William T., Emory Univ., Sch. of Bus. Admin., Atlanta, Ga. 30322
WHITMORE, Harland W., Jr., 1624 River Ter. Dr., East Lansing, Mich. 48823
WHITMORE, William J., Univ. of Kentucky, Com. Col., Lexington, Ky. 40506
WHITNEY, Simon N., 35 Harrison Ave., Highland Park, N.J. 07422
WHITNEY, William G., 4506 Regent St., Philadelphia, Pa. 19143
WHITSETT, J. M., Texas Christian Univ., Sch. of Bus., Ft. Worth 29, Tex.
WHITT, John D., Univ. of the Americas, KM. 16 Carretera, Mexico-Toluca, Apdo. Postal 72-893, Mexico 10, D.F., Mexico
WHITTAKER, James W., 429 Roedel Pl., Paramus, N.J. 07652
WHITTEN, David O., Louisiana State Univ., New Orleans, Dept. of Econ. and Fin., New Orleans, La. 70122
WHITTEN, Herbert O., 1028 Conn. Ave., N.W., Rm. 223A, Washington, D.C.
WHITTEN, William C., Jr., 101 Cherokee Rd., Clemson, S.C. 29631
WHITTIER, Peter, 1387 Birch Hill Rd., Mountainside, N.J. 07092
WHITTINGTON, Floyd L., Amer. Embassy, APO San Francisco 96301
WHITTLESEY, Charles R., 106 Avonbrook Rd., Wallingford, Pa. 19086
WHOLIHAN, John T., Bradley Univ., Col. of Bus. Admin., Peoria, Ill. 61606
WHYEL, George, Jr., 920 Oakland, Ann Arbor, Mich.
WICH, Henry S., 20427 Balfour, Harper Woods, Mich. 49844
WICHMANN, Arthur A., 805 W. Taylor, Fairfield, Iowa 52556
WICKENS, (Mrs.) Aryness J., 2210 Hunter Mill Rd., Vienna, Va. 22180
WICKENS, David L., 2210 Hunter Mill Rd., Vienna, Va. 22180
WICKER, Elmus R., Indiana Univ., Dept. of Econ., Bloomington, Ind.
WICKERSHAM, Edward D., Univ. of Detroit, Col. of Com., Detroit, Mich. 48221
WICKEY, George R., St. Anselm's Col., Manchester, N.H.
WICKMAN, Kenneth P., Univ. of Massachusetts, Dept. of Econ., Amherst, Mass.
WICKS, John H., Univ. of Montana, Dept. of Econ., Missoula, Mont. 59801
WIDENMANN, Hans A., C. M. Loeb, Rhoades, and Co., 42 Wall St., New York 5, N.Y.
WIDICUS, Wilbur W., Jr., Oregon State Univ., Dept. of Bus. and Tech., Corvallis, Ore.
WIDMAIER, Peter H., Univ. of Basel, Inst. of Applied Econ. Res., Leonhardsgraben 3, Basel, Switzerland
WIDMANN, Ernest R., Drexel and Co., 1500 Walnut St., Philadelphia, Pa. 19001
WIDMER, Raymond F., Univ. of Arkansas, Box 1629, Fayetteville, Ark. 72701
WIEGAND, G. Carl, Southern Illinois Univ., Dept. of Econ., Carbondale, Ill.
WIEGAND, Ursula F., Geo-Mar Apts. 3, R.D. 3, Shippensburg, Pa. 17257
WIEGNER, Edward A., 2974 N. Frederick Ave., Milwaukee, Wis.
WIELAND, (Mrs.) Peg D., 45-45 46th St., Woodside 77, L.I., N.Y.
WIENER, Bernard, 86 Springside Ave., New Haven, Conn. 06515
WIENER, Edward, 1051 E. 14th St., Brooklyn 30, N.Y.
WIER, Melville B., U. S. Military Academy, Dept. of Soc. Sci., West Point, N.Y. 10996
WIERS, Paul, Shadyside, Md. 20867
WIESEN, Thomas F., 2216 Broadway, Lubbock, Tex. 79401
WIESENFELD, Henry M., 3023 St. Paul St., Baltimore, Md.
WIESNER, Eduardo, C.E.D.E. Univ. de los Andes, Bogota, Colombia
WIGGINS, (Mrs.) Suzanne E., San Jose State Col., Dept. of Econ., San Jose 14, Calif.
WILBER, Charles K., 6511 60th Ave., Riverdale, Md. 20840
WILBURN, (Mrs.) Jean, 64 E. 94th St., New York, N.Y. 10028
WILCOX, Clair, 510 Ogden Ave., Swarthmore, Pa. 19081
WILCOX, Ernest D., 314 W. 105th St., Apt. 1-C, New York, N.Y. 10025
WILCOX, Walter W., Lib. of Cong., Washington 25, D.C.
WILDE, James A., Univ. of North Carolina, Dept. of Econ., Chapel Hill, N.C.
WILENSKY, Gail S., 1029 Island Dr., No. 106, Ann Arbor, Mich.
WILES, James L., 10 Larch Rd., Wellesley, Mass. 02181
WILES, William W., Bd. of Gov. of the Fed. Res. System, Div. of Res. and Statis., Banking Mkts. Unit, Washington, D.C. 20551
WILEY, Allen V., Bowling Green Univ., Dept. of Econ., Bowling Green, Ohio 43402
WILEY, Jay W., Purdue Univ., Econ. Dept., Lafayette, Ind.
WILHELM, Ross J., Univ. of Michigan, Sch. of Bus. Admin., Ann Arbor, Mich.
WILHELM, Warren, c/o The Texas Co., 135 E. 42nd St., Rm. 1626, New York 17, N.Y.
WILKEY, Harry L., Central Missouri State Col., Warrensburg, Mo. 64093
WILKIE, John A., c/o Ed. Tallard, R. 1, Old Sauk Rd., Middleton, Wis. 53562
WILKINS, B. Hughel, Oregon State Univ., Dept. of Econ., Corvallis, Ore.
WILKINS, John G., 109 Chester Circle, New Brunswick, N.J.
WILKINSON, Bruce W., Univ. of Western Ontario, Dept. of Econ., London, Ont., Canada
WILKINSON, Harold F., 53 Cedar Rd., Belmont, Mass.
WILKINSON, (Mrs.) Matilda W., 1100 31st St., Des Moines, Iowa 50265
WILKINSON, Maurice, Columbia Univ., Grad. Sch. of Bus., New York, N.Y. 10027
WILL, Robert E., Carleton Col., Dept. of Econ., Northfield, Minn. 55057
WILL, Robert M., Univ. of British Columbia, Dept. of Econ. and Polit. Sci., Vancouver 8, B.C., Canada
WILL, Thomas E., 202 E. Bruceton Rd., Pittsburgh, Pa.

WILLARD, Robert E., Nevada Southern Univ., Sch. of Bus., Las Vegas, Nev. 89109
WILLAX, Paul A., Buffalo Area Chamber of Comm., Dept. of Res. and Educa., 238 Main St., Buffalo, N.Y. 14202
WILLCOX, Peter W., 20 Copeland Ave., Reading, Mass.
WILLEN, Hans G., Arthur Andersen and Co., 80 Pine St., New York 5, N.Y.
WILLES, Mark H., 234 Grove St., Mt. Kisco, N.Y. 10549
WILLETT, Edward R., Wakefield, Mass. 01880
WILLETT, Thomas D., 722 Park St., Charlottesville, Va.
WILLEY, Howard D., 151 E. 83rd St., New York 28, N.Y.
WILLGERODT, Hans, Stachelsgut 31, 506 Bensberg, Germany
WILLHITE, Winfield L., Box 323, Mt. Pleasant, Mich. 48858
WILLIAMS, A. R. P., 754 Proctor Rd., Burlington, Ont., Canada
WILLIAMS, Armand J., 541 Lucerne Ave., Davis Island, Tampa, Fla. 33606
WILLIAMS, Clifford G., Indiana Univ., Dept. of Econ., and Div. of Econ. Res., Ballantine Hall, Bloomington, Ind.
WILLIAMS, Constance, 11 Knox St., Boston 16, Mass.
WILLIAMS, Conway S., 965 Highland Ave., Gettysburg, Pa. 17325
WILLIAMS, E. M., Plains, Kans.
WILLIAMS, Edward B., Morehouse Col., Atlanta 14, Ga.
WILLIAMS, Ellis T., 4208 46th St., N.W., Washington 16, D.C.
WILLIAMS, Eric C., 12504 Fairhill Blvd., Cleveland, Ohio 44120
WILLIAMS, Ernest W., Jr., 415 Jane's Lane, Stamford, Conn.
WILLIAMS, Felix S., South Fork Rd., Moorefield, W.Va. 26736
WILLIAMS, Harold R., 1424 A St., Lincoln, Neb. 68502
WILLIAMS, Harold S., Central Washington Col., Ellensburg, Wash. 98926
WILLIAMS, Harry, Econ. and Polit. Studies Div., 400 Army-Navy Dr., Arlington, Va. 22202
WILLIAMS, Jack F., Jr., Loyola Univ., 820 N. Michigan Ave., Chicago, Ill. 60611
WILLIAMS, James H., 162 Jefferson Ave., Tenafly, N.J. 07670
WILLIAMS, James I., 16325 Shamhart Dr., Granada Hills, Calif. 93227
WILLIAMS, James R., 5096 Cherryhill Crescent, Burlington, Ont., Canada
WILLIAMS, (Mrs.) Jean H., 4816 Cumberland Ave., Chevy Chase, Md. 21620
WILLIAMS, John B., 30 Lowell Rd., Wellesley Hills, Mass. 02181
WILLIAMS, John H., 148 Coolidge Hill, Cambridge, Mass. 01922
WILLIAMS, John R., 8112 Hatteras Lane, Springfield, Va. 22151
WILLIAMS, Kenneth B., 4816 Cumberland Ave., Chevy Chase, Md. 21620
WILLIAMS, Ned, Box 61, University, Miss. 38677
WILLIAMS, Obadiah D., Southern Univ., Box 9504, Baton Rouge, La.
WILLIAMS, Randall S., U. S. Embassy, APO 254, New York, N.Y. 09254
WILLIAMS, Robert J., Purdue Univ., Dept. of Agric. Econ., Lafayette, Ind.
WILLIAMS, Robert M., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles 24, Calif.
WILLIAMS, Roger J., Jr., 425 Topping Hill Rd., Westfield, N.J.
WILLIAMS, Walter, 3519 Raymond, Chevy Chase, Md. 20015
WILLIAMS, Walter E., 1854 1/2 S. Cloverdale Ave., Los Angeles, Calif. 90019
WILLIAMS, William V., Hamline Univ., Dept. of Econ., St. Paul 1, Minn.
WILLIAMSON, Harold F., Amer. Econ. Assoc., 629 Noyes St., Evanston, Ill.
WILLIAMSON, Harold F., Jr., Univ. of Illinois, Dept. of Econ., Urbana, Ill.
WILLIAMSON, Jeffrey G., Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706
WILLIAMSON, John H., Univ. of York, Heslington, Dept. of Econ., York, England
WILLIAMSON, Kossuth M., Filley Rd., Haddam, Conn. 06438
WILLIAMSON, Oliver E., Univ. of Pennsylvania, Wharton Sch. Dept. of Econ., Philadelphia, Pa.
WILLIAMSON, Robert B., Univ. of Texas, Fin. Dept., Austin 12, Tex.
WILLIAMSON, Samuel H., Purdue Univ., Econ. Dept., West Lafayette, Ind. 47906
WILLIAMSON, Thomas L., 2719 Hesyler Rd., Toledo, Ohio 43617
WILLIAMSON, William R., 3400 Fairhill Dr., Washington 23, D.C.
WILLING, (Mrs.) Pearl R., 4704 Delafield Ave., New York 71, N.Y.
WILLINGHAM, John J., Pennsylvania State Univ., Col. of Bus. Admin., Boucke Bldg., University Park, Pa. 16802
WILLIS, Cleve E., C-7 Orchard Pk., Davis, Calif.
WILLIS, David M., 2311 12th St., Moline, Ill.
WILLIS, J. Brooke, Savings Banks Trust Co., 200 Park Ave., New York, N.Y. 10017
WILLIS, James E., Univ. of Texas, Col. of Bus. Admin., Dept. of Gen. Bus., Austin, Tex. 78712
WILLIS, Parker B., 60 Brattle St., Apt. 401, Cambridge, Mass. 01922
WILLIS, Philip W., 2 Winter St., Northfield, Vt. 05663
WILLIS, Robert J., Univ. of Washington, Dept. of Econ., Seattle, Wash. 98105
WILLITS, Francis P., III, Pennsylvania State Univ., Rm. 203 Weaver Bldg., University Park, Pa.
WILLITS, Joseph H., Box 441 A Bridgetown Pike, R.D. 1, Langhorne, Pa.
WILLOUGHBY, Christopher R., Int. Bank for Recon. and Dev., 1818 H St., N.W., Washington 25, D.C.
WILLS, Ian R., 1203 1/2 W. Main St., Apt. 1-C, Urbana, Ill. 61801
WILLS, John H., Northern Trust Co., 50 So. LaSalle St., Chicago, Ill.
WILLS, Walter J., Southern Illinois Univ., Sch. of Agric., Carbondale, Ill.
WILMETH, Jo B., P.O. Box 7413, Benjamin Franklin Sta., Washington, D.C. 20044
WILMOT, William V., Jr., Univ. of Florida, Col. of Bus. Admin., Gainesville, Fla. 32601
WILSON, Andrew A., 181 Coleridge Ave., Palo Alto, Calif. 94301
WILSON, Charles Z., Jr., 3604 Briar Lane, Endwell, N.Y.
WILSON, Daniel P., IBM Extension Suisse, 15 rue Pierre Fatio, Geneva, Switzerland
WILSON, David A., Dept. of Forestry, Ottawa, Ont., Canada
WILSON, Douglas B., 101 E. Lafayette St., Syracuse, N.Y. 13205
WILSON, Elizabeth, 1 Waterhouse St., Cambridge, Mass. 01922
WILSON, Francis A. H., Caius Col., Cambridge, England
WILSON, George W., Indiana Univ., Dept. of Econ., Bloomington, Ind. 47405
WILSON, Gordon W., Dept. of Labour Lib., Confederation Bldg., Ottawa 4, Ont., Canada
WILSON, Howard R., Asst. Commercial Secy. for Canada, P.O. Box 851, Lagos, Nigeria
WILSON, Jack A., 8511 La Verne Dr., Adelphi, Md 20783
WILSON, Jack W., North Carolina State Col., Econ. Dept., Raleigh, N.C.
WILSON, James F., Nat. Bank of Cleveland, 127 Public Sq., Cleveland 14, Ohio
WILSON, John D., 6 Sunset Ave., Bronxville, N.Y.
WILSON, John J., 9103 Monte Vista Ave., Montclair, Calif. 94037
WILSON, John O., 3248 Williamsburg Rd., Ann Arbor, Mich.
WILSON, John S. G., Univ. of Hull, Dept. of Econ. and Com., Hull, Yorkshire, England
WILSON, Leonard M., Jr., 29 Eliot St., Sherborn, Mass. 01770
WILSON, Lynton R., Cornell Univ., Cascadilla Hall 236, Ithaca, N.Y. 14850
WILSON, Richard E., 2505-D East Shore Pl., Reno, Nev. 89502
WILSON, T., Univ. of Glasgow, Glasgow, W. 2, Scotland
WILSON, Theodore R., 1480 Pleasant Valley Way, West Orange, N.J. 07052

WILSON, Thomas A., 245 Fort Hill Rd., Scarsdale, N.Y. 10583
WILSON, Thomas F., 438 W. 116th St., New York, N.Y. 10027
WILSON, Thomas H., 2905 W. Bowie, Ft. Worth, Tex. 76109
WILSON, Travis, 2742 Redding Rd., N.E., Atlanta 19, Ga.
WILT, Glenn A., Jr., Arizona State Univ., Col. of Bus. Admin., Tempe, Ariz.
WIMBERLY, Annie B., 2221 40th Pl., N.W., Apt. 2, Washington, D.C. 20007
WIMMER, Larry T., 1321 E. Madison Park, Chicago, Ill. 60615
WIMMERGREN, Lyle E., Liberty Hill Rd., Henniker, N.H. 03242
WIMSATT, Genevieve B., 3721 Alton Pl., N.W., Washington, D.C. 20016
WINAKER, Martin P., 225-C Kennedy Blvd., Lakewood, N.J. 08701
WINAKOR, Thora Geitel, Iowa State Univ., Textile and Clothing Dept., Ames, Iowa
WINAKUR, Ira, 1500 Kanawha St., Adelphi, Md. 20783
WINCH, David M., Univ. of Alberta, Dept. of Polit. Econ., Edmonton, Alta., Canada
WINCHESTER, John F., 59 Field St., Torrington, Conn.
WINDER, John W. L., Univ. of Toronto, Dept. of Polit. Econ., Toronto, Ont., Canada
WINDMULLER, John P., New York State Sch. of Ind. and Labor Rela., Cornell Univ., Ithaca, N.Y.
WINEGARDEN, Calman R., Univ. of Toledo, Toledo, Ohio 43606
WINESTONE, Robert L., 6408 81st St., Bethesda, Md. 20034
WINFREY, John C., Washington and Lee Univ., Dept. of Econ., Lexington, Va.
WING, George A., 3422 Monteith Ave., Cincinnati, Ohio 45208
WING, Mary C., 2126 Connecticut Ave., N.W., Washington, D.C. 20008
WINGATE, John W., City Col., 17 Lexington Ave., New York 10, N.Y.
WINGER, Alan R., Univ. of Kentucky, Econ. Dept., Lexington, Ky. 40506
WINGREN, W. Martin, 2340 Piedmont Ave., Berkeley, Calif. 94704
WINKELMAN, Richard D., Arizona State Univ., Col. of Bus. Admin., Dept. of Econ., Tempe, Ariz. 85281
WINKELMANN, Don, 204 21st St., Ames, Iowa
WINKLER, James E., 202 Paterson Ave., Hasbrouck Heights, N.J. 08040
WINN, Edward L., Jr., V.P.I., Col. of Bus., Blacksburg, Va. 24061
WINN, Willis J., Univ. of Pennsylvania, Wharton Sch., Fin. Dept., Philadelphia, Pa. 19104
WINNICK, Andrew J., 4713 Cottage Grove Rd., Apt #4, Madison, Wis. 53716
WINNICK, Louis, 46-17 244th St., Douglaston, N.Y. 12055
WINSLOW, George K., 47 Central St., Huntington, L.I., N.Y.
WINSNESS, Warren D., 6680 Lucia Lane, Apt. 6, Minneapolis, Minn. 55432
WINSTON, Gordon C., Williamstown Col., Econ. Dept., Williamstown, Mass. 02167
WINSTON, John D., c/o Barbara Winston, 2820 Scott St., San Francisco, Calif. 94122
WINTER, Egon P., 54 Roosevelt Circle, Palo Alto, Calif.
WINTER, George R., Univ. of Alberta, Dept. of Agri. Econ., Edmonton, Alta., Canada
WINTER, Lothar G., Univ. of New Mexico, Col. of Bus. Admin., Albuquerque, N.M.
WINTER, Sidney G., Univ. of California, Dept. of Econ., Berkeley 4, Calif.
WINTER, Stephen I., 43-05 44th St., Sunnyside, N.Y. 12782
WINTERGALEN, Edward, Regis Col., Econ. Div., Denver 11, Colo.
WINTERS, Richard J., Seton Hill Col., Greensburg, Pa. 15601
WINTERS, Robert A., 10 Adams Dr., Princeton, N.J. 08540
WIPPERN, Ronald F., Univ. of Minnesota, Sch. of Bus. Admin., Minneapolis 14, Minn.
WIRICK, Grover C., Jr., 2116 Brockman, Ann Arbor, Mich.
WIRIODIPURO, Sutadi, Bank of Indonesia, Econ. Res. Dept., Djl. M.H. Thamrin 14, Djakarta, Indonesia
WIRPEL, (Mrs.) Estelle, 1167 Asbury Ave., Winnetka, Ill.
WIRPEL, Sander W., 1167 Asbury Ave., Winnetka, Ill.
WIRTHLIN, Richard B., Brigham Young Univ., Econ. Dept., Rm. 304 Knights Bldg., Provo, Utah 84601
WISCHKAEMPER, Paul, Virginia Poly. Inst., Sch. of Bus., Blacksburg, Va.
WISE, Donald E., 3424 Bentley Ave., Los Angeles, Calif. 90034
WISE, Prof. T. F., 55 Ossington Ave., Ottawa 1, Ont., Canada
WISEMAN, Lloyd, 9 Meadowlark Lane, Hutchinson, Kans. 67501
WISEMAN, Michael L., c/o Wiseman's Store, Mountain Home, Ark. 72653
WISH, John R., 21923 University Station, San Juan, P.R. 00931
WISHART, Charles M., 2 Briar Hill Rd., Paris, Ill. 61944
WISSEL, Peter E., 371 Old Army Rd., Scarsdale, N.Y.
WISSMAN, Harry B., c/o Arthur Little, Inc., 35 Acorn Park, Cambridge, Mass. 02140
WISNER, Robert N., Univ. of Tennessee, Dept. of Agric. Econ., Knoxville, Tenn. 37901
WITHERELL, William H., 45 Henry Ave., Princeton, N.J.
WITHERS, William H., R.D. 1, Box 139, Eatontown, N.J. 07724
WITNEY, Fred, Indiana Univ., Dept. of Econ., Bloomington, Ind.
WITT, Jarvis, 2018 36th St., Lubbock, Tex. 79412
WITT, Lawrence W., Michigan State Col., Dept. of Agric. Econ., East Lansing, Mich.
WITTE, James G., Jr., Indiana Univ., Dept. of Econ., Bloomington, Ind.
WITTICH, Claus, 523 W. 112th St., New York, N.Y. 10025
WITTICH, Gunter, Univ. of Calif., Dept. of Econ., Berkeley, Calif. 94720
WITTMAN, John, Jr., Texas Tech. Col., Dept. of Econ., Lubbock, Tex. 79409
WITTRICH, Karl F. J., P.O. Box 6, New York, N.Y. 10033
WOELFLEIN, Kevin G., 1 E. Rosetree Rd., Media, Pa. 19063
WOFFORD, Ben M., Box 8324, Univ. Sta., Reno, Nev. 89507
WOGART, Jan P., Univ. of Texas, Dept. of Econ., Austin, Tex.
WOJNILOWER, Albert M., The First Boston Corp., 20 Exchange Pl., New York, N.Y. 10005
WOJTUN, Bruno S., 750 N. 8th St. Unit 5, Apt. 19, Sunbury, Pa. 17801
WOJTYLA, Henry L., 197 Pascock Ave., Emerson, N.J. 07630
WOLD, Ivor P., Box 234, Peterboro, N.H. 03458
WOLDE-TSADIK, Sileshi, P.O. Box 138, Col. of Agric., Dire Dawa, Ethiopia
WOLF, Bernard M., 2758 Yale Sta., New Haven, Conn. 06520
WOLF, Carl, 44 Bennet Ave., New York 33, N.Y.
WOLF, Charles, Jr., RAND Corp., 1700 Main St., Santa Monica, Calif.
WOLF, Franz B., 5420 Connecticut Ave., Washington, D.C. 20015
WOLF, Harold Arthur, Univ. of Colorado, Sch. of Bus., Boulder, Colo.
WOLF, Joseph C., 1430 Parkchester Rd., Bronx, N.Y. 10462
WOLF, Morton, 2719 No. 45th St., Philadelphia 31, Pa.
WOLF, Ronald H., Univ. of Tennessee, Dept. of Econ., Knoxville, Tenn.
WOLF, William B., 435 Hilledge Dr., Laguna Beach, Calif. 92651
WOLFE, Albert B., 1841 Roxbury Rd., Columbus, Ohio 43212
WOLFE, Cedric, 649 E. 14th St., New York, N.Y. 10009
WOLFE, French Eugene, 3419 Manor Hill Dr., Cincinnati 20, Ohio
WOLFE, Harry C., Bartow, West Virginia 24920
WOLFE, J. N., Univ. of Edinburgh, Dept. of Polit. Econ., Edinburgh, Scotland
WOLFF, Harold, Booz-Allen and Hamilton, Inc., 380 Madison Ave., New York, N.Y. 10017

WOLFF, Jacques, 7 rue Leon Vaudoyer, Paris (VII), France
WOLFF, Reinhold P., Econ. Res., Inc., P.O. Box 1536, Coral Gables, Fla.
WOLFSON, D. J., c/o African Dept., Int. Monetary Fund, 19th and H Sts., Washington, D.C.
WOLFSON, Murray, Oregon State Univ., Dept. of Econ., Corvallis, Ore.
WOLFSON, Robert J., c/o System Dev. Corp., 2500 Colorado Ave., Santa Monica, Calif. 90406
WOLKISER, Arthur M., Laird, Bissell and Meeds, 120 Broadway, New York, N.Y. 10005
WOLL, Artur, 63 Giessen, Roderring 31, Germany
WOLLAN, (Mrs.) Christine N., 808 S. Lincoln Ave., Urbana, Ill. 61801
WOLLMAN, Nathaniel, Univ. of New Mexico, Dept. of Econ., Albuquerque, N.M.
WOLLOCH, Zygfryd B., 13 Overlook Rd., Scarsdale, N.Y.
WOLLSTADT, Roger D., Center for Naval Anal., 1401 Wilson Blvd., Arlington, Va. 22209
WOLMAN, Abel, Johns Hopkins Univ., 513 Ames Hall, Baltimore 18, Md.
WOLOSOFF, Sylvia, 635 E. 14th St., New York, N.Y. 10009
WOLOTKIN, Paul, State Univ. of New York, Agric. and Tech. Inst., Bus. Tech. Dept., Farmingdale, N.Y. 11735
WOLOZIN, Harold, 219 N. Edison St., Arlington 3, Va.
WOLTER, James H., Univ. of Michigan-Flint Col., Dept. of Bus. Admin., 1321 E. Court St., Flint 3, Mich.
WOLTMAN, Harry R., 10919 Garden Grove Ave., Northridge, Calif.
WONG G. HONG, Henry, P.O. Box 1109, Manila, Philippines
WONG, Vivian L. A., John and Middleton Sts., Campelville, East Coast Demera, British Guiana
WONNACOTT, Paul, Univ. of Maryland, Econ. Dept., College Park, Md. 20742
WONNACOTT, R. J., Univ. of Western Ontario, Dept. of Econ., London, Ont., Canada
WOO, John C. H., California State Col. at Fullerton, Div. of Bus. Admin. and Econ., 800 N. State College Blvd., Fullerton, Calif. 92631
WOO, Philip M. S., St. Francis Col., Dept. of Bus. Admin., Loretto, Pa. 15940
WOO, Se Hyung, State Univ. Col., Oswego, N.Y. 13126
WOOD, Carl, 804 S. Washington, Mt. Pleasant, Mich. 48858
WOOD, Donald F., 1131 Sherman Ave., Madison, Wis. 53703
WOOD, Edgar W., Baylor Univ., Dept. of Econ., Waco, Tex. 76706
WOOD, Frank O., 2155 McDaniel Ave., Evanston, Ill.
WOOD, Harry F., Org. 2-1101, M.S.14-76, Boeing Co., P.O. Box 3707, Seattle, Wash. 98124
WOOD, J. W., Jr., 2501 Cedar Springs Rd., Dallas, Tex. 75201
WOOD, Kenneth S., Jr., Dalhousie Univ., The Inst. of Pub. Affairs, Halifax, N.S., Canada
WOOD, Lee C., Univ. of Wichita, Wichita, Kans. 67208
WOOD, (Mrs.) Mary S., 356 Orenda Circle, Westfield, N.J. 07090
WOOD, Norman J., Univ. of Georgia, Col. of Bus. Admin., Athens, Ga.
WOOD, Oliver G., Jr., Univ. of South Carolina, Col. of Bus. Admin., Columbia, S.C. 29208
WOOD, Ralph C., 5917 Cheshire Dr., Wildwood Manor, Bethesda, Md. 21811
WOOD, Ramsay, Off. of Program Policy, Dept. of Housing and Urban Dev., 1626 K St., N.W., Rm. 404 Normandy Bldg., Washington, D.C.
WOOD, Robert L., Jr., 5921 Arlington Blvd., Arlington, Va. 22203
WOOD, W. Donald, Queen's Univ., Ind. Rela. Center, Dunning Hall, Kingston, Ont., Canada
WOODARD, Florence M., 475 Main St., Burlington, Vt.
WOODARD, Francis O., Wichita State Univ., Dept. of Econ., Wichita, Kansas 67208
WOODBURY, (Mrs.) Mildred F., 323 Caversham Rd., Bryn Mawr, Pa. 19010
WOODBURY, Robert M., 323 Caversham Rd., Bryn Mawr, Pa. 19010
WOODBURY, Wendell W., 315 S. Pitt St., Alexandria, Va. 22314
WOODFINE, William J., St. Francis Xavier Univ., Dept. of Econ., Antigonish, N.S., Canada
WOODLAND, Don L., Oklahoma State Univ., Col. of Bus., Dept. of Fin., Stillwater, Okla. 74075
WOODRUFF, Archibald M., Univ. of Hartford, West Hartford, Conn. 06090
WOODRUFF, Cliff, Jr., 910 Silber Rd., Apt. 3B, Houston, Tex. 77024
WOODRUFF, Ruth J., Univ. of New Hampshire, Durham, N.H. 03824
WOODS, Donald H., Univ. of California, Grad. Sch. of Bus. Admin., Los Angeles, Calif. 90024
WOODS, Richard S., Univ. of Pennsylvania, W-280 Dietrich Hall, Philadelphia, Pa. 19104
WOODWARD, Burton M., 2114 W. University Ave., Gainesville, Fla. 32603
WOODWARD, Donald B., Dawn Harbor Lane, Riverside, Conn. 06878
WOODWARD, John T., 4010 Underwood St., Hyattsville, Md. 20782
WOODWORTH, G. Walter, Dartmouth Col., Amos Tuck Sch., Hanover, N.H. 03755
WOODWORTH, Laurence N., 2810 Crest Ave., Cheverly, Md. 21620
WOODWORTH, Robert T., 5701 N.E. 56th St., Seattle, Wash. 98105
WOOL, Harold, 6716 Brigadoon Dr., Bethesda, Md. 20034
WOOL, (Mrs.) Muriel, 6716 Brigadoon Dr., Bethesda, Md. 20034
WOOLF, (Mrs.) Anne P. H., 817 McCall Dr., Norman, Okla. 73069
WOOLF, Donald A., 817 McCall Dr., Norman, Okla. 73069
WOOLLEY, Herbert B., c/o Nat. Bur. of Econ. Res., 26L Madison Ave., New York, N.Y. 10016
WORCESTER, Dean A., Jr., Univ. of Washington, Dept. of Econ., Seattle 5, Wash.
WORLAND, Stephen T., Univ. of Notre Dame, Dept. of Econ., Notre Dame, Ind. 46556
WORLEY, Frederick M., Univ. of Pennsylvania, Wharton Sch., Philadelphia 4, Pa.
WORLEY, James S., Vanderbilt Univ., Dept. of Econ. and Bus. Admin., Nashville 5, Tenn.
WORMSER, Felix Edgar, Rockwood Lane, Greenwich 6, Conn.
WORONIAK, Alexander, 1435 Geranium St., N.W., Washington 12, D.C.
WORRELL, Albert C., 360 Prospect St., New Haven, Conn. 06511
WORSLEY, Thomas B., 2210 Marthas Rd., Alexandria, Va.
WORSWICK, G. D. N., 7, Highmore Rd., Blackheath, London, S.E. 3, England
WORTHAM, John L., Texas Christian Univ., Dept. of Econ., Ft. Worth 29, Tex.
WORTHING, Marion W., 3703 Leland St., Chevy Chase, Md., 20015
WORTHINGTON, Elmer H., Bank of Com., 100 Church St., New York 7, N.Y.
WORTHINGTON, Paul, 168-20 127th Ave., Jamaica, N.Y. 11434
WORTHINGTON, Ralph E., 2400 57th St. N., St. Petersburg, Fla. 33710
WORTMAN, Max S., Jr., 827 River St., Iowa City, Iowa 52241
WORTMAN, Wilferd, College Hgts. A206, Kearney, Neb. 68847
WORTON, David A., 43 Stillwater Dr., Ottawa 14, Ont., Canada
WOTRUBA, Thomas R., San Diego State Col., Mktg. Dept., San Diego, Calif. 92115
WOUDENBERG, Henry W., Jr., 1109-E University Village, East Lansing, Mich. 48823
WRAY, Paul C., Jr., 5807 S. Dorchester Ave., Chicago, Ill. 60637
WREAN, William H., Dartmouth Col., Dept. of Econ., Hanover, N.H. 03755
WRIGHT, Arthur W., Oberlin Col., Dept. of Econ., Oberlin, Ohio 44074
WRIGHT, Chester W., 5747 Blackstone Ave., Chicago, Ill. 60637
WRIGHT, Colin, 909 Forest Ave., Evanston, Ill. 60202
WRIGHT, David M., 420 Fortson Dr., Athens, Ga.
WRIGHT, David R., 5964 Green Meadow Pkwy., Apt. D., Baltimore, Md. 21209

WRIGHT, Earl M., 955 Hartsough, Plymouth, Mich.
WRIGHT, George W., 8629 Polk St., Woodside, McLean, Va. 22101
WRIGHT, James L., 1313 Crestview Dr., Blacksburg, Va.
WRIGHT, John S., Univ. of Ill. at Chicago Circle, Col. of Bus. Admin., P.O. Box 4348, Chicago, Ill. 60680
WRIGHT, Kenneth M., 101 Brook St., Garden City, N.Y.
WRIGHT, Leonard T., State Univ. of New York at Albany, Sch. of Bus., Albany 3, N.Y.
WRIGHT, Leslie C., Univ. of Edinburgh, Dept. of Polit. Econ., Edinburgh, Scotland
WRIGHT, Lloyd I., 542 W. 112th St., Apt. 5-M, New York, N.Y. 10025
WRIGHT, Milton S., Wilberforce Univ., Wilberforce, Ohio 45384
WRIGHT, Richard W., Life Ins. Agcy. Mgmt. Asso., 170 Sigourney St., Hartford, Conn. 06105
WRIGHT, Vincent P., 25 Roselyn Ter., San Francisco, Calif. 94118
WRIGHT, Wallace, 713 8th St., Ames, Iowa
WRIGHT, Walter L., 3rd, 3902 W St., N.W., Washington, D.C. 20007
WRIGHT, Wilson, Procter and Gamble Co., 6th and Main Sts., Cincinnati, Ohio
WRIGHTSMAN, Dwayne E., Univ. of New Hampshire, Whittemore Sch., Durham, N.H. 03824
WRIGHTSON, R. H., Univ. of Virginia, Dept. of Econ., Charlottesville, Va.
WRONE, James G., 108 E. Healey #1, Champaign, Ill. 61822
WRONG, (Mrs.) Elaine G., 24 W. 69th St., New York, N.Y. 10023
WU, Carson K. H., Univ. of Minnesota, 306 Haecker Hall, St. Paul, Minn. 55101
WU, De-Min, Univ. of Kansas, Dept. of Econ., Lawrence, Kans. 66045
WU, G. Shown, 50 Wen Han 1st Rd., Hsin-Hsin Dist., Kaohsiung City, Taiwan
WU, Hsiu-Kwang, Boston Univ., Col. of Bus. Admin., 685 Commonwealth Ave., Boston 15, Mass.
WU, Yeen-Kuen, 1015 N. Cedar St., Pine Bluff, Ark. 71601
WU, Yuan-Li, 1055 Middle Ave., Menlo Park, Calif.
WUBBEN, William D., Charlotte Col., P.O. Box 20428, Charlotte 2, N.C.
WUCHER, Joseph L., 9036 149th St., Apt. 2-B, Jamaica, L.I., N.Y.
WULF, Jürgen, Friedrich-Ebert-Str. 16, 6301 Treis/Lda., Germany
WUNDERLICH, Gene L., U.S. Dept. of Agric., Econ. Res. Ser. RDED., Washington 25, D.C.
WUORI, Frederick A., Eastern New Mexico Univ., Portales, N.M. 88130
WYAND, Martin J., 2213 S. High St., Denver, Colo. 80210
WYAND, Robert R., II, 3200 Lenox Rd., #B-411, Atlanta, Ga. 30324
WYCKOFF, Vertrees J., Guilford Col., Dept. of Econ., Greensboro, N.C.
WYKOFF, Frank C., 2417 B. Ellsworth, Berkeley, Calif. 94704
WYMAN, Parker D., USBER, APO 742, New York, N.Y. 90742
WYMAN, Peter R., 2401 Regan, Boise, Idaho
WYND, (Mrs.) Alice W., 14 Landsdowne Lane, Rochester, N.Y. 14618
WYNER, Henry D., 3147 Tennyson St., N.W., Washington, D.C. 20015
WYNNE, William H., 3401 Thornapple St., Chevy Chase, Md. 21620
WYNNYCZUK, Alexej, Rensselaer Poly. Inst., Dept. of Econ., Troy, N.Y. 12181
WYSS, Hans, 4019 N. 25th St., Arlington, Va. 22207
WYTHE, George, Cosmos Club, Washington, D.C.

XUAN, Nguyen D., Minister of National Economy, 59, Gia Long, Saigon, Vietnam

YAARI, Menahem E., Cowles Found., Box 2125, Yale Sta., New Haven, Conn. 06520
YADAV, Gopal J., 220 University Ave., Kingston, Ont., Canada
YAGER, Charles A., U.S. Embassy—OECD, APO, New York 09777
YAHR, Merle, 243 W. 99th St., Apt. 5-C, New York 25, N.Y.
YAKE, David J., Nursery Rd., Titusville, N.J. 08560
YAKEL, Ralph, Jr., 105 Beacon St., Bridgeport, Conn. 06605
YALE, Jordan P., 81 Peach Tree Dr., East Norwich, L.I., N.Y. 11732
YAMAMURA, Kozo, San Diego State Col., San Diego 15, Calif.
YAMANAKA, Tohru, Sacramento State Col., Dept. of Econ., 6000 Jay St., Sacramento, Calif. 95819
YAMAZAWA, Ippei, 20-16 Honmachi, 5-chome, Koganei, Tokyo, Japan
YANCE, Joseph V., 18 Centre St., Cambridge, Mass. 02139
YANCEY, Claud W., Sr., R.F.D. #1, Mansfield, Ark. 72944
YANCEY, Thomas A., Univ. of Illinois, 444 Commerce W., Urbana, Ill. 61803
YANEZ-PEREZ, Luis, Sierra de las Vertientes No. 431, Mexico 10, D.F., Mexico
YANG, Charles, 685 West End Ave., New York, N.Y. 10025
YANG, Hung-Yu, 1 Lane 100 Sun Chiang Rd., Taipeh, Taiwan
YANKOVIC, Donald J., Univ. of Pittsburgh, Dept. of Econ., Pittsburgh, Pa. 15213
YANKOVICH, Sister Mary L., Clarke Col., Dubuque, Iowa 52001
YAP, Emmanuel Q., Philippine Economic Soc., Central Bank of the Philippines, Port Area, Manila, Philippines
YAP, Mow-Ming, Box 453 G.R.C., Indiana Univ., Bloomington, Ind.
YAPAR, Nihat F., P.K. 55, Yenisehir, Ankara, Turkey
YASA, Memduh, Univ. of Istanbul, Faculty of Econ., Istanbul, Turkey
YAS'ER, Yasar, State Inst. of Statis., Ankara, Turkey
YASUBA, Yasukichi, Princeton Univ., Dept. of Econ., Princeton, N.J. 08540
YATES, Edward H., Defense Res. Corp., P.O. Box 3587, Santa Barbara, Calif. 93107
YEADON, Raymond J., 64-19B 186th Lane, Flushing, N.Y. 11365
YEAGER, Francis S., 5095 Fieldwood, Houston 17, Tex.
YEAGER, George M., Franklin Cole and Co., 2 Wall St., New York, N.Y. 10005
YEAGER, Leland B., Univ. of Virginia, Dept. of Econ., Rouss Hall, Charlottesville, Va. 22903
YEATS, Alexander J., Univ. of Nebraska, Dept. of Econ., Lincoln, Neb.
YEE, Harold T., 3394 Adeline St., Berkeley, Calif. 94703
YEH, Chung-Jeh, 35-G University Park, Orono, Me. 04473
YEH, Kung Chia, 16939 Bollinger Dr., Pacific Palisades, Calif. 90272
YENNI, Jacques E., Loyola Univ., Col. of Bus. Admin., New Orleans 15, La.
YEPEZ, Luis Fernando, Dist. Venezolana de Azucares, Ave. Urdaneta, Edif. Luz Elec. de Venezuela, 7° Piso, Caracas, Venezuela
YETT, Donald E., Univ. of California, Sch of Pub. Health, Los Angeles, Calif. 90024
YEUNG, Patrick P. T., Claremont Grad. Sch., Econ. Dept., Harper Hall, Claremont, Calif 91711
YIN, (Mrs.) Helen Wan-hung, 2505 W. 119th St., Inglewood, Calif. 90303
YIN, Yi-chang, 2505 W. 119th St., Inglewood, Calif. 90303
YING, John Tsung-shun, Rose Poly. Inst., Terre Haute, Ind. 47803
YNTEMA, Dwight B., 940 Paw Paw Dr., Holland, Mich. 49423
YNTEMA, Theodore O., 3950 Franklin Rd., Bloomfield Hills, Mich. 48013
YOCUM, James C., Ohio State Univ., College of Com. and Admin., Columbus, Ohio 43210
YODER, Dale, Stanford Univ., Grad. Sch. of Bus., Stanford, Calif. 96128
YODER, Fred R., Campbellsville Col., Campbellsville, Ky. 42718
YOHE, William P., Duke Univ., Dept. of Econ. and Bus. Admin., Durham, N.C. 27706
YOHO, James G., Duke Univ., Sch. of Forestry, Durham, N.C.

YONEDA, Kimimaru, Inst. of Asian Econ. Affairs, 42 Honmura-cho, Ichigaya, Shinjuku-Ku, Tokyo, Japan
YOON, I. N., Culver-Stockton Col., Econ. Div., Canton, Mo. 63435
YOON, Segi, 220 Congress St., Brooklyn, N.Y. 11201
YORDON, Wesley J., Jr., Univ. of Colorado, Dept. of Econ., Boulder, Colo.
YORK, James O., 333 Montgomery St., San Francisco, Calif. 94104
YOST, John W., Ind. Ser. Bur., 331 Madison Ave., New York, N.Y. 10017
YOTOPOULOS, Pan A., Univ. of Wisconsin-Milwaukee, Dept. of Econ., Milwaukee, Wis. 53211
YOU, Man He, Southern Oregon Col., Ashland, Ore. 97520
YOUEL, Harlan E., Gulf Oil Corp., Gulf Bldg., Pittsburgh 30, Pa.
YOUNG, Arthur N., 985 E. California Blvd., Apt. 303, Pasadena, Calif. 91106
YOUNG, B. F., Stoneleigh, Alger Court, Bronxville 8, N.Y.
YOUNG, David W., 1317 Brockton Ave., Los Angeles, Calif. 90025
YOUNG, Edwin, Univ. of Maine, Alumni Hall, Orono, Me. 04473
YOUNG, Frank, 23 Salt Box Lane, Darien, Conn.
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YOUNG, Howell P., Mutual of N.Y., 1740 Broadway, New York 19, N.Y.
YOUNG, James E., Univ. of Arizona, Dept. of Econ., Tucson, Ariz.
YOUNG, James J., 184 LaSalle Ave., Hampton, Va.
YOUNG, John H., Univ. of British Columbia, Dept. of Econ. and Polit. Sci., Vancouver 8, B.C., Canada
YOUNG, John P., 985 E. California Blvd, Apt. 303, Pasadena, Calif. 91106
YOUNG, Lee, 200 E. Main St., Union, Mo. 63084
YOUNG, Leslie G., 10827 149th St., Edmonton, Alta., Canada
YOUNG, Ralph A., 2836 Chesapeake St., N.W., Washington 8, D.C.
YOUNG, Robert A., Univ. of Arizona, Dept. of Agric. Econ., Tucson, Ariz.
YOUNG, Robert C., Ford Found., 8 Rawdon St., Calcutta, India
YOUNG, Shik Chun, 20850 24th Ave. S., Seattle, Wash. 98188
YOUNG, Stanley E., 10315 Ford Rd., Fairfax, Va. 22030
YOUNG, Theodore E., 155 E. 52nd St., New York 22, N.Y.
YOUNGDAHL, C. Richard, 115 Slope Dr., Short Hills, N.J. 07078
YOUNGMAN, Anna P., 4201 Massachusetts Ave., N.W., Washington 16, D.C.
YOUNGQUIST, Joel C., 624 University Ave., Burbank, Calif.
YOUSSEF, George M., Univ. of Western Ontario, Univ. of Col. of Arts and Sci., London, Ont., Canada
YOUTSLER, James S., Skidmore Col., Saratoga Springs, N.Y. 12866
YU, Kuo-Hwa, Bank of China-Int. Cooperation Admin., No. 15 Chungshan Road, Second Section, Taipei, Taiwan
YU, Tai Hyun, 166, Naisu-Dong, Chongno-Ku, Seoul, Korea
YU, Tom T., 99 Summit Rd., Beacon Hill, Port Washington, L.I., N.Y. 11050
YUCEL, Nail C., Washington State Univ., Troy Hall, Rm. 305, Pullman, Wash. 99163
YUCELIK, Mustafa Z., Yenimahalle, Dorduncu Durak, CINAR Sokak No. 164/4, Ankara, Turkey
YUDIN, Elinor B., 415 E. 85th St., New York 28, N.Y.
YUN, Seh Ik, 13-67 Pyung-Dong, Sudaemoom-Ku, Seoul, Korea
YUNUS, Muhammad, Vanderbilt Univ., Dept. of Econ., P.O. Box 1139, Sta. B, Nashville, Tenn. 37203
YU-TAIK, Kim, Deputy Prime Minister, Capital Bldg., Seoul, Korea
YVON, Bongoy, B.P. 834, Leopoldville XI, Leopoldville, Congo
ZABEL, Edward, Univ. of Rochester, Dept. of Econ., River Campus Sta., Rochester 20, N.Y.
ZABEL, Sheldon A., 2755 N. Pine Grove Ave., Chicago, Ill.
ZABIJAKA, V., 2633 30th St., S.E., Apt. 4, Washington 20, D.C.
ZABLUDOFF, Sidney, 3716 Lorcom Lane, Arlington, Va.
ZABRENSKI, Stephen T., 158-39 Jewel Ave., Fresh Meadows, N.Y. 11365
ZABROWSKI, Edward K., 5073 26th Rd. N., Arlington, Va. 22207
ZACCONE, June, Hofstra Col., Dept. of Econ., Hempstead, N.Y.
ZAFFUTO, Anthony P., 155 New Highway, Commack, L.I., N.Y. 11725
ZAFIRIOU, Rena, Int. Bank for Recon. and Devel., 1818 H St., N.W., Washington 25, D.C.
ZAGARS, Waldemar, 50 W. Broadway, Gettysburg, Pa. 17325
ZAHKA, William, Pennsylvania Military Col., Chester, Pa.
ZAIDI, Mahmood A., Univ. of Minnesota, Dept. of Ind. Rela., Minneapolis, Minn. 55455
ZAK, John, 25-15 12th St., Long Island City, N.Y. 11102
ZAMANI, Hosein, Bank Markazi, Econ. Res. Dept., Tehran, Iran
ZAND, Dale E., New York Univ., Grad. Sch. of Bus., 100 Trinity Pl., New York 6, N.Y.
ZANDER, William L., 2631 N. Robinson Rd., Apt. 9-B, Texarkana, Tex. 75501
ZANETTI, G. N., Victoria Univ., Econ. Dept., P.O. Box 196, Wellington, N. Zealand
ZANNETOS, Zenon S., 11 Nassau Dr., Winchester, Mass. 01890
ZAPATA, Juan A., Univ. of Chicago, Dept. of Econ., 1126 E. 59th St., Chicago, Ill. 60637
ZAPOLEON, Louis B., 816 S.E. Riviera Isle, Ft. Lauderdale, Fla.
ZAPOLEON, (Mrs.) Marguerite, 816 S.E. Riviera Isle, Ft. Lauderdale, Fla.
ZAREMBA, Alois L., 3545 Tolland Rd., Shaker Heights, Ohio 44122
ZAREMBA, Joseph, Fordham Univ., Dept. of Econ., New York 58, N.Y.
ZAREMBKA, Paul, Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706
ZARKA, Annas, 4934 Chestnut St., Philadelphia, Pa. 19139
ZARLEY, Arvid M., Univ. of Kansas, Dept. of Econ., Lawrence, Kans. 66045
ZARNOWITZ, Viktor, 5758 S. Blackstone Ave., Chicago, Ill. 60637
ZAYAT, Edmund D., 4388 E. 58th St., Tulsa, Okla. 74135
ZEBOT, Cyril A., Georgetown Univ., Dept. of Econ., Washington 7, D.C.
ZEC, Gregory C., 97 Lylewood Dr., Tenafly, N.J. 07670
ZECCA, Paschal C., 1347 Ellsworth St., Philadelphia, Pa. 19147
ZECHER, Joseph R., 105 Patterson Ave., Columbus, Ohio
ZEFF, Stephen A., Tulane Univ., Sch. of Bus. Admin., New Orleans 18, La.
ZEIS, Paul M., 1909 Pelham Dr., S.W., Roanoke, Va. 24018
ZELDER, Raymond E., 2744 Carlyle Dr., Kalamazoo, Mich. 49001
ZELENITZ, Allan H., R. #1, Box 310-A, Durham, N.C. 27705
ZELLEN, Michael S., 3315 81st St., Jackson Heights, N.Y. 11372
ZELLER, Paul J., 1675 N. 21st St., Apt. 8, Arlington, Va. 22209
ZELLNER, Arnold, Univ. of Wisconsin, Dept. of Econ., Madison, Wis. 53706
ZELOMEK, A. Wilbert, 39 Fifth Ave., New York, N.Y.
ZEMAN, Allan H., 621 Mellon St., Pittsburgh 6, Pa.
ZEMAN, Morton, 10 Franklin Ave., White Plains, N.Y.
ZEMBAL, Theodore, Weston Col., Weston, Mass. 02193
ZEMECKAS, Kazys J., 526 Union Ave., Oshkosh, Wis.
ZEMELKA, Stanley A., Bellarmine Col., Bus. Admin. Dept., Louisville 5, Ky.
ZEMPEL, Arnold, 3000 Spout Run Pkwy., Arlington, Va. 22201

ZENCO, John, 225 N. Hutchins, Lodi, Calif. 95242
ZENICK, Manuel C., 3509 S St., N.W., Washington, D.C. 20007
ZENIUK, Nikola, 1300 30th St., Bldg. B-2, Apt. 18, Boulder, Colo.
ZENOFF, David B., Harvard Bus. Sch., Doctoral House, Boston 63, Mass.
ZENTZ, Bill, Bank of Commonwealth, Investment Dept., P.O. Box 119, Detroit, Mich. 48231
ZERBY, John A., Univ. of Sydney, Dept. of Econ., Sydney, N.S.W., Australia
ZERFOSS, Karl P., Jr., Supervised Investors Serv. Inc., 120 S. La Salle St., Chicago, Ill. 60603
ZERTUCHE, Juan A., 20 De Noviembre, No. 430 Sur., Monterrey, N.L., Mexico
ZEVIN, Robert B., Univ. of California, Dept. of Econ., Barrows Hall, Berkeley, Calif. 94720
ZEWOLDE, Demeke, c/o Ayele Zewolde, Imperial Bd. of Telecommunications, P.O. Box 1047, Addis Ababa, Ethiopia
ZIALCITA, Edgardo P., 60-F Cordillera St., Quezon City, Philippines
ZICHY, Maurice C., Immaculata Col., Immaculata, Pa. 19345
ZICKEFOOSE, Paul, Box 1072, University Park, N.M. 88070
ZIDERMAN, Adrian, 125 Church Dr., Kingsbury, London, N.W. 9, England
ZIDNAK, Pete, 15011 Freeman Ave., San Jose, Calif. 95127
ZIEGLER, Raymond J., Merrimack Col., North Andover, Mass.
ZIEMER, Richard C., 1478 Spring Vale Ave., McLean, Va. 22101
ZIEVERS, James F., 640 S. Kensington, La Grange, Ill.
ZILBERT, Edward R., 3242 Colby Ave., Los Angeles 66, Calif.
ZIMMER, David W., Akron Area Chamber of Com., 137 S. Main St., Akron, Ohio 44308
ZIMMER, William J., Jr., 4 Peyton St., Winchester, Va. 22601
ZIMMERMAN, Fred M., Control Data Corp., 8100 34th Ave., S., Minneapolis, Minn. 55420
ZIMMERMAN, Hans J., Univ. of Illinois, Dept. of Ind. Admin., 350 Commerce W., Urbana, Ill. 61801
ZIMMERMAN, Robert O., Box 238, Hiram, Ohio 44234
ZIMMERMAN, William E., 1575 New Scotland Rd., Slingerlands, N.Y. 12159
ZIMMERMANN, Horst, Univ. zu Koeln, Seminar fuer Finanzwissenschaft, 5 Cologne, Germany
ZIMRING, O. David, 11 S. La Salle St., #1201, Chicago, Ill. 60603
ZINAM, Oleg, 2541 Warsaw Ave., Cincinnati 4, Ohio
ZINBARG, Edward D., 67-76 Booth St., Forest Hills, Queens, New York 11375
ZINCOME, Louis H., Jr., E-2 University Gardens, Charlottesville, Va. 22903
ZINGLER, Ervin K., Univ. of Houston, Cullen Blvd., Houston, Tex. 77004
ZINK, Eddie F., American Embassy, Managua, Nicaragua
ZINK, Lee B., Southeastern State Col., Durant, Okla. 74701
ZINKAND, Kenneth E., 2160 E. Tremont Ave., Bronx, N.Y.
ZINKE, George, 2985 7th St., Boulder, Colo.
ZINN, David C., 3703 Ottawa, Kalamazoo, Mich.
ZINN, Frederick K., 3579 De Hart, St. Ann, Mo. 63074
ZISKIND, David, 2339 Silver Ridge Ave., Los Angeles, Calif. 90014
ZITNIK, Louis J., Mitchum, Jones and Templeton, 216 Forest Ave., Laguna Beach, Calif. 92651
ZLOTNICK, Jack, Magie Apts. #5N, Faculty Rd., Princeton, N.J. 08540
ZNAYENKO, Wassyl, 404 W. 116th St., New York, N.Y. 10027
ZOAO-ISAYA, Ruffin, 86 Ave. OPALA, Nouvell Cite, Leopoldville, Rep. of the Congo
ZOELLNER, John F., 6436 Woodson Dr., Shawnee Mission, Kans. 66202
ZOERB, Carl R., Nibelungenstr. 55a, 8 Munich 19, Germany
ZOETEWEIJ, Hubertus, Intern. Labour Off., Geneva, Switzerland
ZOFFER, H. J., Univ. of Pittsburgh, Grad. Sch. of Bus., Pittsburgh, Pa. 15213
ZOGRY, Arnold J., 11304 College View Dr., Wheaton, Md. 20902
ZOHAR, Uri, 327 Harvard Ave., Claremont, Calif.
ZOLLER, Richard B., Armour and Co., P.O. Box 9222, Chicago, Ill. 60690
ZONDAG, C. H., USAID, c/o Amer. Embassy, APO New York, N.Y. 09271
ZOOK, Paul D., Southern Methodist Univ., Sch. of Bus. Admin., Dallas, Tex. 75222
ZORBAUGH, Grace S., 1431 N. Mountain Ave., Tucson, Ariz.
ZORN, Eugene C., Jr., Republic Nat. Bank of Dallas, Dallas, Tex. 75222
ZOTTMANN, Luiz, 511 W. 112th St., Apt. 9, New York, N.Y. 10025
ZOTTOLA, Armand H., Catholic Univ. of America, Econ. Dept., Washington 17, D.C.
ZSCHOCK, Dieter K., Ind. Rela. Sect., P.O. Box 248, Princeton, N.J. 08540
ZSOLDOS, Laszlo, Univ. of Delaware, Sch. of Bus. and Econ., Robinson Hall, Newark, Del. 19711
ZUBEK, Brother Stanley, Hamilton Central High, 643 Dayton St., Hamilton, Ohio 45011
ZUBER, Ernest V., Jr., 110 Westlawn Park, Iowa City, Iowa
ZUBROW, Reuben A., 300 Bellevue Dr., Boulder, Colo.
ZUCKER, Albert, 33-68 21st St., Long Island City, N.Y. 11106
ZUCKERMAN, Irving, 1718 I St., N.W., Washington, D.C. 20006
ZUKOWSKI, Walter H., Colby Col., Waterville, Me.
ZUNTZ, Michael, 3008 Pine Spring Rd., Falls Church, Va.
ZUPNICK, Elliot, 5550 Fieldston Rd., Riverdale, N.Y. 14775
ZUVEKAS, Clarence, Jr., c/o H. E. Biggs, 1927 St. Clair Ave., Brentwood, Mo. 63144
ZUWAYLIF, Fadil, San Fernando State Col., 18111 Nordhoff St., Northridge, Calif. 91326
ZUZIC, Marko, 69 College Ave., Factoryville, Pa. 18419
ZWANZIG, Otto E., 1385 Chas. Huot St., Sillery, Quebec, Que., Canada
ZWERNEMAN, James A., St. Edward's Univ., 3001 S. Congress Ave., Austin, Tex. 78704
ZWICK, Charles, 4611 29th Pl., N.W., Washington, D.C. 20008
ZWICKER, Charles H., 56 Arbor Rd., Roslyn Heights, N.Y. 11577
ZYMELMAN, Manuel, 20 Hope St., Auburndale, Mass. 02166

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Year	President	First Vice-President	Vice-Presidents	Secretary-Treasurer	Editor, American Economic Review	Counsel	Elected Members of Executive Committee
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*** For the years 1886 through 1952, see the 1956 *Handbook*, pages 454–63.**

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Evsey D. Domar
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Kermit Gordon
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Wolfgang F. Stolper

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O. H. Brownlee
Kermit Gordon
Rendigs T. Fels
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Wolfgang F. Stolper
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Abba P. Lerner
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Albert Rees
Richard Caves
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Richard A. Easterlin

1966
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Awards

In 1947, the Association instituted two awards: the Francis A. Walker Medal, to be given not more frequently than once every five years "to the living American economist who in the judgment of the awarding body has during his career made the greatest contribution to economics," and the John Bates Clark Medal, to be given every two years "to that American economist under the age of forty who is adjudged to have made a significant contribution to economic thought and knowledge." Recipients of these medals to date are:

Francis A. Walker Medal

1947	Wesley C. Mitchell	1957	Frank H. Knight
1952	John Maurice Clark	1962	Jacob Viner

John Bates Clark Medal

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1949	Kenneth E. Boulding	1959	Lawrence R. Klein
1951	Milton Friedman	1961	Robert M. Solow
1953	No award	1963	Hendrik S. Houthakker
1955	James Tobin	1965	Zvi Griliches

In 1965 the Association instituted the award of Distinguished Fellow. In addition to all past presidents and members who have received the Walker Medal, this award may be granted annually to not more than two "economists of high distinction in the United States and Canada." The following have received this award:

1965

Edward H. Chamberlin — Harold Hotelling

Honorary Members

Ragnar Frisch *1948* .... Oslo
Eugenio Gudin *1962* .... Rio de Janeiro
Sir Roy Harrod *1950* .... Oxford
Sir Ralph Hawtrey *1948* .... London
J. R. Hicks, *1950* .... Oxford
Walter Thomas Layton (Baron Layton of Danehill) *1932* .... London
Erik Lundberg *1962* .... Stockholm
James E. Meade *1962* .... Cambridge
Gunnar Mydral *1947* .... Geneva
Bertil Ohlin *1947* .... Stockholm
G. Ugo Papi *1962* .... Rome
Francois Perroux *1962* .... Paris
Raul Prebisch *1962* .... Santiago
Lionel C. Robbins (Baron of Clare Market) *1947* .... London
Joan Robinson *1950* .... Cambridge
Erich Schneider *1962* .... Kiel
Jan Tinbergen *1950* .... The Hague

Note: The figures in italics indicate the year of election to honorary membership.

ANNUAL MEETINGS

Note: A tabulation of "Statistics of Annual Meetings" from 1885 to 1910 may be found in the papers and discussions on the occasion of the twenty-fifth anniversary of the Association, published in the "Third Series" of the publications of the Association (Vol. XI, 1910, No. 1, pages 92–93). This tabulation gives not only the date and place of meeting but also indicates main subjects discussed and the number of members of the Association. The list of cities in which annual meetings have been held 1910 through 1935 may be found on page 306 of the 1948 Directory.

The following list includes estimates of registration. These figures are very rough, but they give a general idea of how many members of the American Economic Association attend the meetings and of the total attendance of all the affiliated associations. Some figures are estimates of attendance, which includes students and those not registering, and other figures are paid registrations.

Year	*Place*	*Local Arrangements Chairman or Representative*	*A.E.A.*	*Total*
1936	Chicago	G. V. Cox	760	2.938
1937	Atlantic City	E. M. Patterson	687	2,076
1938	Detroit	R. P. Briggs	671	2,346
1939	Philadelphia	W. N. Loucks	793	2,854
1940	New Orleans	R. W. Elsasser	358	696
1941	New York	C. S. Shoup	1,029	1,764
1942	Washington, D. C. (Cleveland canceled)	A. E. Taylor		3,107
1943	Washington, D. C.	A. E. Taylor		1,924
1944	Washington, D. C. (Canceled)	J. Donaldson M. Gilbert		
1945	Cleveland	D. A. Hill R. L. Davison	566	932
1946	Atlantic City	J. W. Hoot		1,704
1947	Chicago	G. J. Cady	1,202	1,437
1948	Cleveland	D. S. Thompson	1,114	1,683
1949	New York	M. R. Gainsbrugh	1,839	3,572
1950	Chicago	C. G. Wright	1,413	3,458
1951	Boston	A. C. Neal	1,091	2,948
1952	Chicago	R. T. Glidden	1,897	2,729
1953	Washington, D. C.	R. A. Young	1,925	4,664
1954	Detroit	E. L. Cushman	1,175	2,001
1955	New York	George Garvy	2,041	4,007
1956	Cleveland	C. A. Barker	1,499	2,795
1957	Philadelphia	K. R. Bopp C. J. Anderson	1,985	2,812
1958	Chicago	George W. Mitchell	1,805	2,291
1959	Washington, D. C.	R. T. Bowman Ewan Clague	2,802	5,674
1960	St. Louis	Homer Jones	1,420	3,185
1961	New York	J. Robert Lindsay	3,010	5,679
1962	Pittsburgh	Clyde Harrell	1,875	3,853
1963	Boston	George H. Ellis Richard de Costa	2,651	4,528
1964	Chicago	E. T. Baughman	3,164	6,441
1965	New York	Fred Klopstock	3,593	5,464
1966	San Francisco	Gault W. Lynn		
1967	Washington, D. C.			
1968	Chicago			
1969	New York			
1970	Detroit			
1971	New Orleans			
1972	Chicago			
1973	New York			

TABLE 1

MEMBERSHIP CLASSES
(at the Midyear)

	1941	*1946*	*1953*	*1956*	*1963*	*1966*
Annual	3,176	4,144	6,709	7,329	9,035	12,526
Junior	83	90	449	515	1,462	1,808
Family	14	64	132	135	140	203
Complimentary	15	17	43	56	102	116
Honorary	19	15	22	18	17	17
Life	37	31	60	77	219	252
Total members	3,344	4,361	7,415	8,130	10,975	14,922

TABLE 2

MEMBERS AND SUBSCRIBERS, 1886–1965

Year	*Annual*	*Life*	*Honorary*	*Total Members*	*Subscribers*	*Total*
1886						182
1893	482	73	17	572	82	654
1894	484	72	16	572	88	660
1895	485	71	16	572	80	652
1896	479	74	15	568	97	665
1897	477	71	15	563	105	668
1898	488	69	14	571	114	685
1899	498	68	12	578	139	717
1900	541	69	11	621	153	774
1901	722	67	11	800	150	950
1902	782	67	11	860	151	1,011
1903	757	67	11	835	155	990
1904	792	67	11	870	155	1,025
1905	800	66	11	877	151	1,028
1906	794	66	11	871	165	1,036
1907						1,002
1908				868	162	1,030
1909				1,205	155	1,360
1910	1,440	69	10	1,519	183	1,702
1911	2,103	78	9	2,190	214	2,404
1912	2,369	88	9	2,466	249	2,715
1913	2,157	83	9	2,249	264	2,513
1914	2,060	81	8	2,149	320	2,469
1915	2,004	80	7	2,091	353	2,444
1916	2,033	80	7	2,120	369	2,489
1917	2,077	87	6	2,170	433	2,603
1918	2,130	87	5	2,222	456	2,678
1919	2,125	86	5	2,216	498	2,714
1920	2,213	84	4	2,301	565	2,866
1921	2,230	102	3	2,335	592	2,927
1922	2,296	102	8	2,406	639	3,045
1923	2,479	98	10	2,587	703	3,290
1924	2,691	98	9	2,798	749	3,547
1925	2,816	92	8	2,916	830	3,746
1926	2,538	88	14	2,640	813	3,453
1927	2,562	86	16	2,664	843	3,507
1928	2,620	78	12	2,710	904	3,614
1929	2,671	79	16	2,766	982	3,748
1930	2,704	76	17	2,797	1,056	3,853
1931	2,626	64	15	2,705	1,076	3,781
1932	2,488	62	19	2,569	1,058	3,627
1933	2,306	58	20	2,384	1,037	3,421
1934	2,433	54	19	2,506	1,066	3,572
1935	2,473	53	18	2,544	1,118	3,662
1936	2,556	48	17	2,621	1,178	3,799
1937	2,652	44	17	2,713	1,219	3,932
1938	2,764	41	19	2,824	1,270	4,094
1939	2,906	40	20	2,966	1,292	4,258
1940	3,089	40	19	3,148	1,327	4,475
1941	3,406	37	19	3,462	1,319	4,781
1942	3,617	37	17	3,671	1,219	4,890
1943	3,749	33	16	3,798	1,293	5,091
1944	3,913	32	16	3,961	1,533	5,894
1945	4,108	31	15	4,154	1,752	5,906
1946	4,618	31	13	4,662	2,161	6,823
1947	5,286	29	14	5,329	2,200	7,529
1948	5,858	27	17	5,902	2,461	8,363
1949	6,578	36	17	6,631	2,504	9,135
1950	6,871	44	21	6,936	2,578	9,514
1951	6,998	48	22	7,068	2,692	9,760
1952	7,191	54	22	7,267	2,755	10,022
1953	7,252	62	21	7,335	2,874	10,209
1954	7,398	69	19	7,486	2,925	10,411
1955	7,463	74	18	7,555	2,963	10,518
1956	8,354	78	18	8,450	3,135	11,585
1957	8,494	91	15	8,600	3,492	12,092
1958	9,073	101	15	9,189	3,659	12,848
1959	10,028	117	14	10,159	3,859	14,018
1960	10,681	142	14	10,837	4,053	14,890
1961†	10,876	164	14	11,054	3,934	14,988
1962†	11,083	182	20	11,285	4,359	15,644
1963	11,727	228	18	11,973	4,306	16,279
1964	12,769	239	17	13,025	4,791	17,816
1965	13,863	247	17	14,127	5,699	19,826

* Figures for the earlier years are probably not accurate. In many cases the Secretary's Reports gave only the dditions, etc., and the totals had to be computed from the total of the previous year.

† Figures furnished by the data processing company for these two years were not accurate.

TABLE 3

GEOGRAPHICAL ANALYSIS OF MEMBERS AND SUBSCRIBERS IN THE UNITED STATES AND POSSESSIONS

(As of May 31, 1966)

NORTHEAST	Me.	N.H.	Vt.	Mass.	R.I.	Conn.	N.Y.	Del.	Pa.	N.J.	Md.	W.Va.	D.C.	*Total*
Members	37	57	35	610	51	267	2,004	33	653	438	651	56	874	5,766
Subscribers	10	11	12	102	11	45	413	6	123	58	45	21	145	1,002
	47	68	47	712	62	312	2,417	39	776	496	696	77	1,019	6,768

MIDDLE	Ohio	Mich.	Ind.	Wis.	Minn.	Ia.	Mo.	Ill.	Total
Members	431	516	273	236	168	151	222	851	2,848
Subscribers	77	78	44	51	47	31	45	129	502
	508	594	317	287	215	182	267	980	3,350

SOUTHEAST	Tenn.	N.C.	Miss.	Va.	Ky.	S.C.	Ga.	Ala.	Ark.	Fla.	La.	Total
Members	99	177	24	561	61	35	97	48	30	162	71	1,365
Subscribers	37	52	19	66	22	21	37	34	17	41	21	367
	136	229	43	627	83	56	134	82	47	203	92	1,732

SOUTHWEST	Okla.	Tex.	N.M.	Ariz.	Total
Members	64	330	28	70	492
Subscribers	21	90	11	14	136
	85	420	39	84	628

NORTHWEST	N.D.	S.D.	Neb.	Kans.	Mont.	Colo.	Wyo.	Idaho	Utah	Total
Members	13	14	55	98	15	116	6	16	31	364
Subscribers	8	11	16	32	6	27	6	9	8	123
	21	25	71	130	21	143	12	25	39	487

FAR WEST	Ore.	Wash.	Cal.	Nev.	Total
Members	110	175	1,134	24	1,443
Subscribers	28	40	242	2	312
	138	215	1,376	26	1,755

NEW STATES	Alas.	Hawaii	Total
Members	10	32	42
Subscribers	4	9	13
	14	41	55

POSSESSIONS	P.R.	Canal Zone	Virgin Is.	Mar. Is.	Guam	Total
Members	37	2	1	2		42
Subscribers	15		1		1	17
	52	2	2	2	1	59

APO & FPO	APO	FPO	Total
Members	121	6	127
Subscribers	42	2	44
	163	8	171

SUMMARY

States	*Members*	*Subscribers*	*Total*
Northeast	5,766	1,002	6,768
Middle	2,848	502	3,350
Southeast	1,365	367	1,732
Southwest	492	136	628
Northwest	364	123	487
Far West	1,443	312	1,755
New States	42	13	55
Possessions	42	17	59
APO and FPO	127	44	171
	12,489	2,516	15,005

TABLE 4

GEOGRAPHICAL ANALYSIS OF MEMBERS AND SUBSCRIBERS FOR SELECTED YEARS UNITED STATES AND FOREIGN

	United States and Possessions										Other Countries								
	N.E.	*Middle*	*S.E.*	*S.W.*	*N.W.*	*Far West*	*New States*	*Possessions*	*A.P.O. F.P.O.*	*U.S. Total*	*Europe*	*Africa*	*Australasia*	*No. Amer.*	*Cent. Amer.*	*So. Amer.*	*Foreign Total*	*Grand Total*
1933																		
Members	1,353	596	125	57	83	166		—		2,380	65	6	27	37	3	3	141	2,521
Subscribers	237	180	78	47	55	70		7		674	109	6	132	25	2	7	281	955
Total	1,590	776	203	104	138	236		7		3,054	174	12	159	62	5	10	422	3,476
1943																		
Members	2,098	706	422	95	115	211		12		3,659	35	5	4	39	4	11	98	3,757
Subscribers	396	270	160	65	70	100		7		1,068	26	11	37	36	3	22	135	1,203
Total	2,494	976	582	160	185	311		19		4,727	61	16	41	75	7	33	233	4,960
1953																		
Members	3,562	1,653	761	177	183	584		22	34	6,976	154	19	92	106	27	41	439	7,415
Subscribers	630	366	230	90	91	164		13	17	1,601	483	39	343	65	28	49	1,007	2,608
Total	4,192	2,019	991	267	274	748		35	51	8,577	637	58	435	171	55	90	1,446	10,023
1963																		
Members	4,655	2,341	1,040	408	334	1,162	28	26	96	10,090	246	38	213	235	92	64	888	10,978
Subscribers	718	401	267	102	92	226	10	10	34	1,860	507	77	625	93	51	71	1,424	3,284
Total	5,373	2,742	1,307	510	426	1,388	38	36	130	11,950	753	115	838	328	143	135	2,312	14,262
1966																		
Members	5,766	2,848	1,365	492	364	1,443	42	42	127	12,489	418	79	353	397	158	67	1,472	13,961
Subscribers	1,002	502	367	136	123	312	13	17	44	2,516	1,192	133	1,018	172	146	57	2,718	5,234
Total	6,768	3,350	1,732	628	487	1,755	55	59	171	15,005	1,610	212	1,371	569	304	124	4,190	19,195